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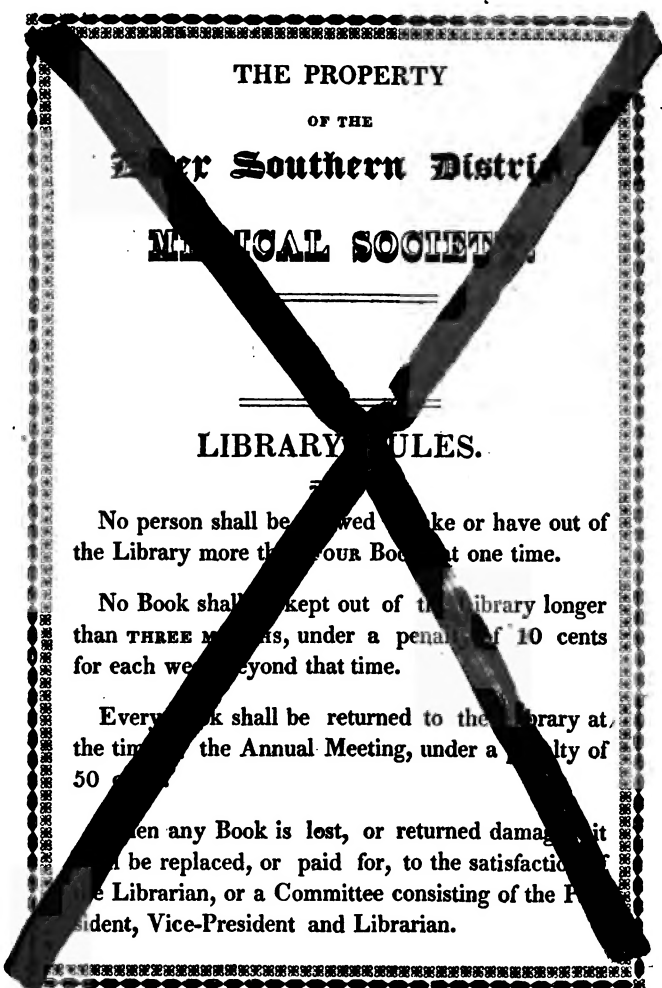
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VOL. XLV.

FROM JANUARY TO JUNE, 1821

*Et quoniam variant morbi, variabimus artes;
Mille mali species, mille salutis erunt.*

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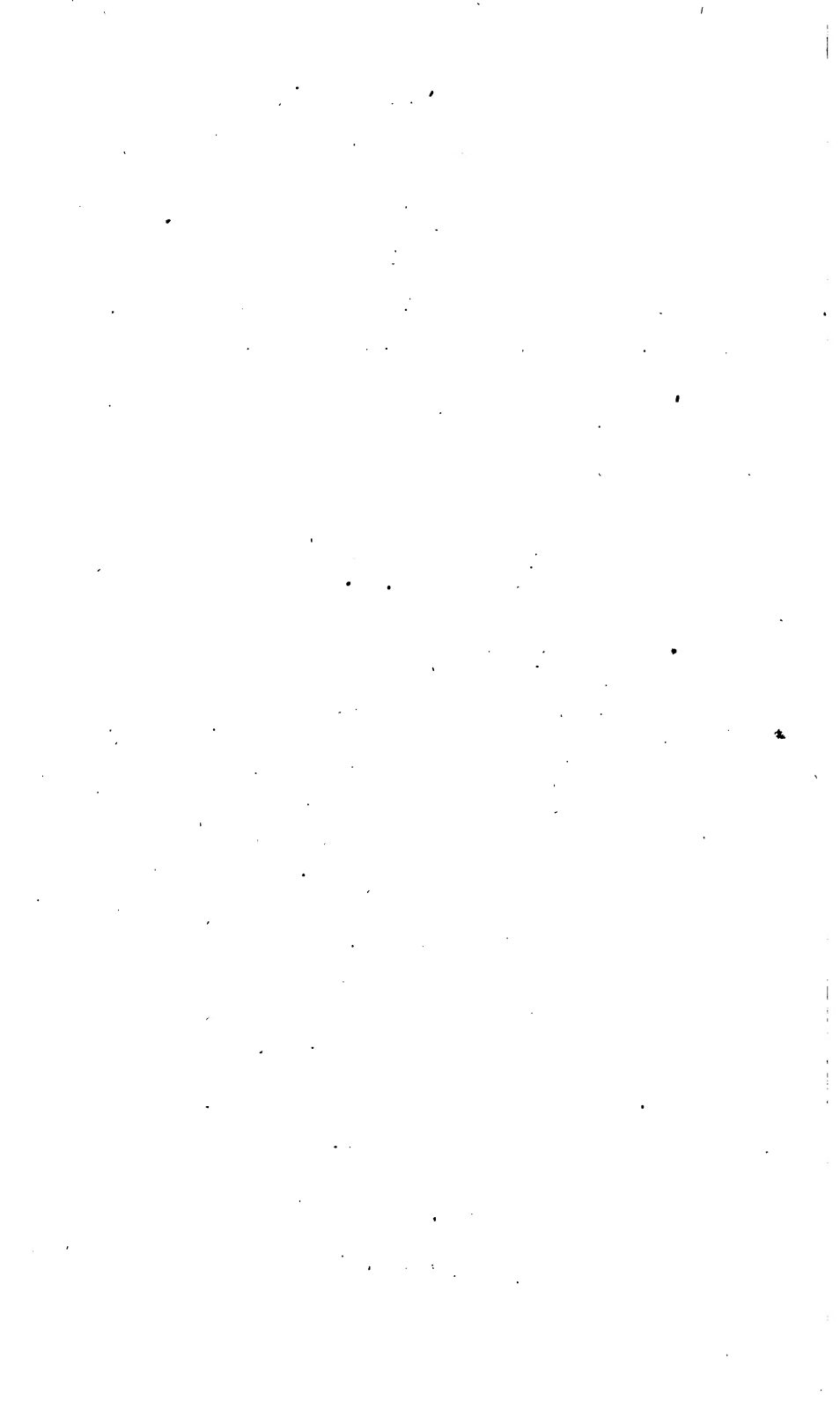
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1 OF VOL. XLV.]

JANUARY, 1821.

[NO. 263.]

NOTICE.

The next of the series of PRÆMIA to the several volumes of this Journal, (which commenced with that to the forty-third volume,) comprising a History of the Progress of Medicine and its auxiliary Sciences for the half-year immediately previous to the period of their production, respectively, will be published on the last day of January. One of the especial intentions of those Præmia, is to present a comprehensive view of the state and progress of Medicine throughout Europe generally, and in the United States of America; an object that cannot be effected in the regular monthly Numbers of the Journal, because of the small extent of space which can be there appropriated to this purpose.

Original Communications, Select Observations, etc.

History of a Case of the successful Formation of an Artificial Anus in an Adult, by DANIEL PRING, Member of the College of Surgeons in London: with an Account of an analogous Operation in two Cases, by G. FREER, Esq. of Birmingham.

THE case which forms a principal subject of the following memoir, derives the little value and interest it might possess chiefly from the two circumstances, that there are but few examples of a similar practice in surgery, and that the one here related exhibits a more successful result than general experience would incline us to anticipate.

Mrs. White, who is a widow, but has never borne children, is sixty-four years of age; she is rather of a full habit, with a florid complexion; her health has generally been pretty good, although she has not, at former periods of her life, been exempt from severe indispositions, principally affecting her head. About the middle of the summer of 1819, she was first troubled with acute pains in the abdomen, the general tenderness of which was much increased by pressure; more particularly so on the left side, a little below the ribs. Her stools often contained blood, with a large proportion of mucus or lymph, and were otherwise commonly of a very bad quality. Her disease was considered as dysenteric, and was treated principally by mercury, ipecacuanha, opium, and saline aperients. In the course of a fortnight from the commencement of this plan her gums became affected by the mercury, although the quantity of the blue-pill which she had taken did not, upon an average, exceed from five to ten grains a-day; her teeth were loose, and

an ulcer formed on one cheek. The mercury was discontinued for a week; and, as her symptoms had before improved under it, the same treatment was then resumed for another week, when, the tongue and cheek becoming sore, it was again discontinued. The mouth speedily recovered from the mercurial affection, and she left Bath for her residence in Gloucestershire, apparently in good health, at least with very little remains of her former symptoms. I was informed that, about the Christmas following, she had suffered an attack of inflammation of the bowels, with obstinate constipation. Her bowels had for some time previously been occasionally painful, and the discharges from them irregular: she had none but fluid stools, and, when not suffering a spontaneous diarrhœa, was under the necessity of taking purgative medicines; to the neglect of which was imputed the attack of inflammation which occurred about Christmas. Recovering from this attack, she relapsed into her former state of chronic disease, of a dysenteric character.

Mrs. White returned to Bath in February, 1820, and a medical treatment, very little differing from that before described, was again instituted, apparently with some benefit. She first mentioned to me, in the beginning of March, that she felt an obstruction in the lower bowels: every thing, she said, appeared to stop at one place; and, when the contents of the bowels passed beyond this point, she was relieved from the distress of pain and distention which she before felt. She described the seat of the obstruction as being very low. I examined the rectum with my finger, and, as high as it would reach, I could perceive nothing like disease. I then attempted to introduce a rectum-bougie, which passed freely about four inches up the gut, and then met with some impediment. The bougie was introduced every second day, and, on the third or fourth introduction, it passed freely beyond that which I had supposed was a stricture; but, whether this contraction had previously existed, or was excited by the irritation of the bougie, I do not feel competent to determine. In subsequent examinations, a middle-sized rectum-bougie passed readily about seven inches; its further progress was completely impeded. It was attempted to introduce a large-sized urethra-bougie: this also stopped at the same place, and bent with the pressure employed to introduce it further. After reiterated trials with bougies of various sizes, the result was so little satisfactory, that no advantage could be reasonably expected from persevering in this plan of treatment: it was therefore abandoned. For some weeks afterwards, the symptoms underwent very little variety. As long as the bowels were kept open by the regular use of castor oil, Epsom salts, &c. the distress of the symptoms was mitigated: but, these means failing, as they sometimes did, a constipation

ensued, which was with difficulty overcome by the most powerful purgatives, aided by clysters with soap, aloes, &c.

With great difficulty some small purging stools had been procured almost every day, up to the 25th of June, when the obstruction, which had long been increasing, appeared to have become complete. All the medical resources of art were afterwards exhausted in fruitless attempts to procure evacuations. Salts, senna, aloes, colocynth, jalap, scammony, gamboge, elaterium, calomel, were given in their largest doses, and variously combined: castor oil was also given in doses of three ounces; and, as vomiting was by no means frequent, these medicines were commonly retained. Injections of every sort, and by different means, were also administered: they were sometimes retained for about half an hour, to the amount of four or six ounces, and were then forcibly expelled. It was attempted to pass a flexible catheter beyond the obstruction, through which clysters might be thrown into the bowels above the seat of it: whether this attempt succeeded in its mechanical object, was not certain. The pulse generally, during the period of this obstruction, was about 90, seldom exceeding 100: the tongue was dry, but clean; there was no vomiting, unless this action was excited by the presence of medicines or food; there was considerable distention, and some tenderness, of the abdomen, but the degree of the latter was not very great. She was once bled, without any relief to her symptoms, rather as a measure of prevention than of necessity; and, when all other means had failed, some large doses of laudanum were given, without any reasonable expectation, on the supposed possibility of the existence of spasm.

It appeared quite clear, after the persevering trial of the above means for many days, that the obstruction, by whatever cause produced, was one which could not be overcome. A somewhat similar case, of a medical practitioner in this city, had a short time before terminated fatally; which case was reported in your Journal by Mr. WHITE. Anticipating that the case of my patient might soon become one of equal extremity, I determined to give her a chance for her life, by making an artificial anus, if she was willing that her life should be prolonged upon such terms. I had heard vaguely, by report, that such an operation had been resorted to by Mr. FREER, of Birmingham, to whose example and instruction I have always much pleasure in acknowledging my obligation: but I was not, at that time, acquainted with the particulars of his operation, or with the exact nature of the disease for the relief of which it was instituted. I accordingly proposed this dernier resort to my patient, who did not violently object to it. I wished her, however, to have

another opinion upon her case before she decided on this question; and was happy to avail myself of the well-known professional judgment and skill of Mr. GEORGE NORMAN. I met Mr. Norman in consultation on the 6th of July; and, after a very long and accurate investigation of the nature of the disease, so far as it could be judged of by its history, and by examination of the rectum, both with the finger and with bougies, Mr. Norman concurred with me in thinking that nothing was to be gained by a longer trial of medicines; and also that the case, if allowed a spontaneous course, must speedily have a fatal termination. Mr. Norman also agreed in thinking that an artificial anus offered the only resource for prolonging her life, but thought it eligible to wait twenty-four hours longer.

The following day, the 7th of July, twelve days from the commencement of the total retention of feces, I was accompanied by Mr. GEORGE SKINNER, on whom a considerable share of the treatment subsequently devolved, and again met Mr. Norman. We found that the state of our patient was not at all improved, but rather that she was sinking under her protracted disease. The formation of an artificial anus was again proposed, and all its possible advantages and disadvantages fairly stated. The patient was desirous of living upon any terms, and agreed to submit to the operation. Accordingly, having placed her on a table, I made an incision on the left side of the abdomen, beginning about two inches above, and one inch on the inside, of the anterior superior spinous process of the ilium. This incision was extended obliquely downwards and inwards to within three-quarters of an inch of the edge of Poupart's ligament: the fascia covering the abdominal muscles was thus exposed to the extent of between three and four inches. An opening was then made through the external and internal oblique, and the transversalis, muscles; which opening was enlarged, with a bistoury conducted by my finger, to the extent of the external incision. The peritoneum being now laid bare, a small opening was made in this membrane, which was enlarged to the extent of between two and three inches. The patient being greatly agitated, and the diaphragm and abdominal muscles thrown into violent action, a considerable protrusion occurred at this time of the small intestines from the superior part of the wound: these were, however, readily replaced. The colon was thus freely exposed a little above its sigmoid flexure, at which place I made an incision into it of about an inch and a half in length: this incision was made on the left side of the gut, in order to avoid some small branches of the inferior mesenteric artery, which were distributed on the right. The contents of the bowels were immediately expelled with great force to a considerable distance: as the feces escaped,

the gut collapsed, and began to subside from its place: a ligature was therefore passed through it at the lower part of the opening, the apposition of which to the external wound was then preserved until the bowels were copiously evacuated. The obstruction could not be felt by the finger, introduced through the wound: no attempt was therefore made to overcome it in this direction. The opening in the gut was attached to the external wound by four sutures, one in its superior and inferior margins, and one on each side; and the wound of the integument was closed by two sutures above, and one below, the opening of the intestine: it was then dressed with sticking-plaster above and below the opening into the gut, and this latter place was covered by a light compress; the patient was then put into bed.

Very little disturbance of the constitution succeeded immediately to the operation. There was no vomiting: the patient felt relieved from the pain of distention; passed liquid stools frequently; took gruel; and slept a little in the course of the night. The following day, the wound looked well: the pulse was between 100 and 110; the tongue dry; there was very little soreness of the abdomen. She took some aperient medicine, gruel, and beef-tea. The wound, on the third day from the operation, was very much inflamed, and the places of the sutures were disposed to ulcerate: a partial union of the integument had taken place, but it was to a very trifling extent. Her general appearance, upon the whole, was unfavourable: her pulse was quick and feeble, and the tongue dry and ulcerated. It was proposed to cut away the stitches, but, as the union was so very imperfect, it was apprehended that there would follow a protrusion of the small intestines; and so little sanguine were our expectations of a favourable result, that it was observed by Mr. Norman, there was very little fear of the stitches cutting their way through, for that they would last as long as the patient: at this time no other expectation could be reasonably entertained. The inflammation of the wound, which was of the erysipelatous kind, continued to increase, and in the course of eight or ten days had produced a most extensive sloughing, — a complete destruction of skin, cellular membrane, and fascia, for several inches around the wound. The colon was exposed for the whole extent of the incision into it; its edges were everted, its colour was a deep-red, and it was found to have contracted a firm adhesion to the muscular margin by which it was surrounded. During this sloughing process, the wound was poulticed, and the patient took bark, ammonia, and aromatic confection, port-wine, beef-tea, and purgatives; the mouth, tongue, and fauces, which were covered with aphthæ and ulcerations, were frequently washed with a solution of alum

in honey and water, by means of sponge fastened to a piece of whalebone.

The inflammation, which had nearly covered the abdomen and loins, had abated, and the sloughing had terminated in about a fortnight after the operation. There was then a large, ill-conditioned, open wound; and the sloughing of the cellular membrane and fascia under the skin was so extensive that a probe would scarcely reach the termination of it in any direction. The powers of the constitution were so reduced, that there was very little hope of the healing of a wound so extensive: it did, however, heal slowly; and the union of the surfaces of a large pouch, which extended from the wound up to the ribs and towards the back, was promoted by a seton.

During the first four or five weeks after the operation, the patient was unable to void her urine voluntarily, which was therefore drawn off with a catheter, commonly twice a-day. She had, in the course of the healing of the wound, rigors, which were followed by fever, tenderness of the abdomen, sometimes vomiting; and she suffered several attacks of erysipelatous inflammation, which sometimes nearly covered the back, loins, nates, and posterior parts of the thighs. Her back became excoriated, and very troublesome from the irritability of the skin, from lying constantly upon it, and from the impossibility of keeping her always dry, as feces were almost constantly discharged.

With great labour and assiduity, the case, notwithstanding every possible impediment and disadvantage which could oppose recovery, slowly improved in its circumstances. When her general health was good, the wound healed rapidly; but there was seldom a steady amendment for many days together: fever, erysipelatous inflammation, sickness, or something, occurred to render recovery protracted. It was a ticklish and precarious business, and required throughout a very nice steering. She was fortunate in having an excellent nurse, by whose constant and indefatigable care in cleaning the wound, removing dirty clothes, &c. great additional mischief was prevented. The feces appeared to be very irritating to the skin: it is probable that much of the inflammation might be imputed to this quality.

If any additional proof were required of the completeness of the obstruction, and of the necessity of the operation in this case, it may be found in the circumstance that the patient had no evacuation whatever from the natural anus during the first three months after the commencement of the obstruction. As she was constantly under the influence of aperient medicines, the contents of the bowels were always fluid, and their peristaltic movements preternaturally excited: it may therefore be

inferred, as the continuity of the intestine was preserved, that, unless the obstruction had been in a degree to render the gut during this time totally impervious, some portion of the fluid contents of the bowels would in so long a period have descended into the rectum, from whence it would have been expelled; as clysters had been, which, to the amount of four or five ounces, were seldom retained longer than half an hour. My conjecture on the circumstance of there being for so long a time no evacuation from the natural anus, was, that the gut was impervious at the place of the obstruction, and that the portion of intestine below the wound had contracted, from defect of distention, and would in time become obliterated; the more readily, as its internal surface is naturally disposed for an active absorption. This conjecture was, however, erroneous, as conjectures are apt to be. On the 1st of October, more than three months from the commencement of the obstruction, an indurated and elongated portion of feces passed from the natural anus: the same thing has since occurred irregularly. These evacuations are, however, so scanty and extenuated, that there is at present but little reason to hope that the natural calibre of the gut will ever be restored.

The artificial opening has not shown lately any disposition to contract. The edges of the incision of the gut, in the course of the first fortnight, had united for a very trifling extent: the opening, therefore, towards the lower part is intersected by a point of union, leaving a large aperture above, and a small one below: the feces are, of course, principally discharged through the former. About a month since, she had an attack of apoplexy, which was followed by complete paralysis of the right arm. She was bled, purged, and blistered; the arm was frequently immersed in hot water, and rubbed with a stimulating liniment: she recovered the use of her arm in about four days, and her health was again as good as formerly. A fortnight after this attack, she complained of pain, vertigo, sense of weight in the head, and had had a restless night: ten ounces of blood were taken from the back of the neck by cupping, followed by a dose of calomel, salts, and senna. In a few days she was again free from complaint.

The artificial anus has now been established between five and six months: the object of the operation has been completely attained. The patient's general health is apparently good, and she has had no return of the dysenteric symptoms before mentioned. Her pulse is commonly about 70; her tongue clean, her complexion florid; her digestion is good, and so well disposed to make the most of her food, that her plan of diet is still directed to be one of rather strict abstinence. She has recovered her flesh in a great degree. She is able to sit up, or

walk about the house: the latter, however, is an experiment which she has not ventured often. She has generally one or two stools a-day; and she does not, upon the whole, experience so much inconvenience from the manner of the evacuation as might have been expected. The bowel, when she is upright, has a great disposition to project: this would not have been the case if the skin covering it had not unluckily sloughed. The projection of the bowel does not amount to a considerable prolapsus: it is merely a distention of the portion which is exposed. It is expected that this disposition to prolapsus will be restrained when use has made her contrivances a little more perfect. She has a truss, somewhat similar to that for exomphalos, constructed with a circular spring, and a large pad containing a weak spiral spring, which is preserved in its place by means of straps: this contrivance has not, however, yet answered so well as a compress, confined by a band, pinned tightly around her.

At the time of making this artificial anus, it was projected rather upon the analogy afforded by that which sometimes succeeds to a strangulated hernia, than from any certain knowledge of precedents. I have since found that an incision was first made into the colon by M. Durét, a surgeon at Brest, in a child born with an imperforate anus, in whom the termination of the rectum could not be discovered by an incision in the natural situation of this opening. The report of this case descends to the twenty-fifth month, when the child was suffering from inversion (*double renversement*) of the gut, which it was not possible, as it is stated, either to prevent or correct. This case, which is, I believe, the first and only one on record, is related by Sabatier.* Mr. Callisen, surgeon of Copenhagen, has proposed to discover the left side of the colon in its course in the lumbar region, where he supposes it to be on the outside of the peritoneum, and to make an incision into it between the edge of the false ribs and the crista of the ilium, parallel to the anterior edge of the quadratus lumborum muscle. This operation was performed by Mr. Callisen on the dead subject with difficulty; an example which is not likely to be followed on the living one, as it does not promise any particular advantage. I had, a few weeks since, the pleasure of seeing Mr. Freer (to whom surgery was already indebted for the first successful operation of tying the external iliac artery) in Bath; and he then confirmed the fact, which I had heard reported vaguely by some and contradicted by others, of his having made an incision into the colon with a view to the formation of an artificial anus. Mr. Freer has been so obliging as to favour

* *Médecine Opératoire*, tome troisième, p. 336.

me with the report of his experience in this operation, which want of leisure, want of health, or want of inclination, had hitherto prevented his making public himself: and, as he has added to the obligation by allowing me to make any use of his cases which I may think proper, giving the detail in his own words, I am happy in the opportunity of comprising in this paper the whole of our experience on an operation which is likely to prove an useful addition to the resources of surgery.

“ On the 24th of December, 1817, I was requested to meet in consultation Dr. Johnstone and Dr. De Lys, of Birmingham, and Mr. Short, surgeon of Solihull, in the case of Mr. Lowe, a respectable farmer, residing about seven miles from Birmingham. Mr. Lowe was about forty-seven years of age, remarkably temperate and regular in all his habits, and had enjoyed uninterrupted good health till about fifteen months ago; when he began to suffer from dyspepsia, attended with a deficient secretion of bile. Dr. Johnstone had prescribed for him occasionally, with temporary advantage, mercurial medicines, so as to produce slight affection of the gums, and he had made use of gentle tonics: he had also taken the Leamington water. At the time I saw him, he complained of a fixed dull pain at the lower part of the belly, and was a good deal troubled with flatulence and other dyspeptic symptoms; he was exceedingly costive, and his feces were compressed in a very unnatural manner; he had also a slight difficulty in voiding urine.

“ On introducing the finger *per anum*, we ascertained that there existed a contraction of the rectum, but situated so high that it could scarcely be reached by the end of the finger. The prostate gland was considerably enlarged. It was agreed in consultation, that frequent clysters should be administered, and that mechanical means should be resorted to, to overcome, if possible, the stricture of the rectum. Accordingly, rectum-bougies were employed regularly for several weeks. The patient seemed to improve in his general health, and was able to use moderate exercise, in attending to his business. Still he had great difficulty in voiding his stools, and their appearance showed that the rectum was as contracted as ever. Mr. Short informed us that he could not pass the bougie above five inches up the gut; and the disease was evidently gaining ground. Under these circumstances, Dr. De Lys and myself were sent for on the 30th of January, when we met Mr. Short, who gave us the above account of the failure of the means hitherto tried. The bougie could not pass beyond the contraction, and clysters had ceased to produce their accustomed effect.

“ It was plain that the disease had made progress: the patient, having had no stool since the 27th, was exceedingly uneasy, and his bowels were very tense, though not in more pain than usual.

Small doses of elaterium were prescribed, and stimulating clysters of aloes directed; but without success.

"On the 3d of February, we were again sent for. The patient had passed no stool, and had vomited, which was very unusual with him. His belly was swelled and hard, but not tender to the touch; his pulse quick, and his countenance expressive of considerable anxiety. We again examined the rectum, but without being able fairly to reach the stricture; and no bougie, not even of the smallest size, could be made to pass through the contracted part of the gut. Under these circumstances it was thought right that an attempt should be made, by means of a bistoury, to relieve the stricture of the gut. I introduced a long bistoury,* guided by the fore-finger, along the hollow of the sacrum, in the best manner I could, till I met with a resistance which prevented the further progress of the instrument. All my attempts, however, to divide the stricture of the gut proved ineffectual; and, after passing a little wind and some blood, but no feculent matter, the patient was put into bed again; had an opiate ordered him, and a warm-bath. On the following day, we found him tolerably free from pain; he had passed a better night than we could have expected, and was free from sickness: still he had had no stool, although he had taken several doses of castor-oil, and had had emollient clysters administered to him.

"It now became evident that no relief was to be expected from the use of medicine. Death, in his present state, was inevitable; and we therefore determined to propose to him, as a last resource, an operation, which certainly was hazardous, but which his desperate condition appeared to justify. This operation consisted in making an incision through the parietes of the abdomen, in the left iliac region, so as to expose the colon near its sigmoid flexure, and to make an opening in that intestine for the purpose of establishing an artificial anus in that situation. We did not know, with any degree of certainty, what might be the nature of the obstruction of the rectum, for it was situated beyond the reach of the finger; but it was possible that the obstruction might be caused by the pressure of a tumor or of an abscess, or by some other cause not of a permanent nature, which might be removed in the course of time; and, in that case, the feces would return to their natural channel, and the artificial anus would close. But, even though so desirable an event should not happen, and though Mr. Lowe might have to submit, for the remainder of his days, to the

* The instrument I employed can scarcely be called a bistoury, being rather a flat trocar about six inches long, which was introduced within the cannula, till it met the obstruction; the blade was then projected by means of a spring, and used as a bistoury.

loathsomeness and inconvenience of an artificial anus, still his life, which was most valuable to his numerous family, was worth preserving on any terms; and he expressed his willingness to submit to any operation which might give him a chance of life.

"The possibility of such an operation had occurred to Dr. De Lys and myself two years before, in attending a child born without an anus, and in whom it was found impossible, by means of a trocar, to reach the rectum. It struck us, in this case, that we should be justified in forming an artificial anus, in the manner I have described, as the only means of saving the life of the child. I accordingly performed the operation: a considerable quantity of meconium was evacuated; and, during the three weeks that the child lived, the feces passed freely at the wound. The child sucked and slept well, and seemed free from suffering, but died apparently from marasmus, the wounded gut adhering firmly to the wound of the parietes of the abdomen, and without any appearance of inflammation of the bowels or other viscera. In this case, though ultimately unsuccessful, the operation had undoubtedly prolonged the life of the child, and seemed to justify us in recommending it in the present instance. We knew, from the various cases recorded by surgical writers, that wounds of the colon are, *ceteris paribus*, less dangerous than those of any other intestine; and therefore we apprehended little or no danger from the actual injury done to the colon by the operation, and so far considered the colon to be the fittest situation for an artificial anus. Besides, it was evident that an artificial anus in the colon, near its sigmoid flexure, would interfere less with the functions of nutrition than if it were formed in any part of the small intestines.

"This operation we thought it right to recommend as a last resource; but, as the symptoms were not yet very urgent, it was deemed prudent to delay it till the increase of danger from the continuance of the obstruction should, if possible, still more fully justify us in proposing an operation so formidable, and of which the success must evidently be very doubtful. We did not choose, therefore, to recommend the operation that evening; but gave the patient to understand that, in case every other resource failed, we should, when we next saw him, think it our duty to propose his submitting to an operation.

"On the following day, February the 5th, we received a letter from Mr. Short, stating that Mr. Lowe had passed a very restless night, had vomited several times, was very much troubled with the hiccup, and complained of considerable soreness all over the belly; and was, he added, very anxious for our arrival, having made up his mind to submit to any operation that might

be deemed advisable. We had, for some days past, looked forward to the probability of having to perform this operation, and had therefore, as much as possible, considered every circumstance connected with it, and had agreed that the most eligible spot for making the external incision that was to expose the colon, would be the left iliac region, about an inch above the anterior superior spine of the ilium, and between one and two inches in front of that process. I accordingly made an incision nearly three inches in length in this situation; and, after cautiously dividing the abdominal muscles and the peritoneum, I exposed the colon covered by the omentum: the omentum was with some difficulty pushed aside by the finger, as it felt adhering to the intestines; and, after confining the colon with one stitch at each end of the wound, I made a longitudinal incision into it, about two inches in length. There was an immediate gush of a considerable quantity of liquid and highly fetid excrementitious matter, and a great deal of wind escaped from the wounded intestine; the belly became soft, and, though Mr. Lowe complained of uneasiness from the wound, he said the pain was considerably diminished. He was ordered to be put into a warm-bath in the course of a few hours, and Mr. Short was requested to stay with him all night.

“On introducing the fore-finger into the cavity of the abdomen before opening the gut, we ascertained that the bowel thus exposed was of considerable size, so as to give us reason to presume that it was the colon, of which we could distinctly feel the longitudinal band. The outward incision had been purposely made as small as possible, from a fear that the bowels, being excessively distended, would burst through the wound with great violence. We had been so much troubled by this accident, in operating on the intestines of dogs, that we dreaded it exceedingly; but Mr. Lowe bore the operation so calmly, that no such accident occurred, and we were enabled to make the most deliberate examination of the gut, before proceeding to lay it open. After the wounded intestine had been plentifully evacuated, spontaneously in the first instance, and then by an injection of warm water, I introduced my fore-finger up and down the gut, and the direction of its course upwards and backwards towards the left kidney, and downwards towards the sigmoid flexure of the colon, confirmed me in the opinion that I had succeeded in exposing the colon.

“On the 6th of February, the day after the operation, Mr. Lowe had, since we left him the day before, passed through the wound several dark-coloured liquid stools, and a good deal of wind. The belly was soft and free from soreness; but he complained of excessive pain in his loins; his pulse was quick, and his tongue furred. An aperient mixture of rhubarb and mag-

nesia was prescribed, and he was directed to take an opiate at bed-time.

"On the 7th, Mr. Shoot informed us that Mr. Lowe had complained of considerable soreness of the belly; and that, his pulse having become sharp, he had been induced to take about twelve ounces of blood from his arm. The pain had been relieved by the bleeding, and the pulse, though quick, was become soft. He still complained of the most intolerable pain in the loins; he voided urine, however, very freely. The wound looked well, and there had been a free discharge of thin feculent matter from the artificial anus. There was a constant rumbling of wind in the belly, which however felt very soft, and was not sore to the touch.

"On the 8th, as the stools had not been voided so freely since yesterday, and as the belly, though not sore, was rather tense, it was thought right to administer a clyster of warm water through the wound of the colon: the liquid passed very freely and without pain, but brought away no feculent matter. The pain in the loins continued unabated: he was troubled with an almost incessant hiccup, and with considerable thirst; the tongue was dry and furred, and the pulse quick. A purge of calomel and rhubarb was prescribed, and an opiate at night; and he was directed to take occasionally, whenever the hiccup was particularly distressing, a table-spoonful of a medicine consisting of the camphorated mixture, with paregoric elixir and sulphuric ether.

"On the 9th.—The laxative had operated freely: he had taken an opiate at bed-time, but had passed a very restless night. The hiccup continued most distressing.

"On the 10th.—No material alteration had taken place in any respect. The gut adhering very evidently to the wound, I thought it right to remove the ligatures, which were become loose. He still continued very restless at night.

"The 11th.—Mr. Lowe had passed a rather better night, but was exceedingly dejected, with a look of considerable anxiety in his countenance. Hitherto he had had no appetite, nor could he be said now to have a natural one; but he complained of a sense of sinking at his stomach, which was relieved only by taking food very frequently, in small quantities. His diet, for a long time, and especially since the obstruction had become so inveterate, had been liquid and of the lightest and most nutritious quality, and his beverage of the mildest kind, such as whey, toast and water, or imperial. Since yesterday he had, at his own request, feeling exceedingly languid, been allowed to take a little wine and water. His pulse was very quick and weak; his tongue clean, but dry, with intolerable thirst and clamminess of the mouth.

"The 12th.—Mr. Lowe appeared better, and had passed a more quiet night. He had voided several stools, some similar in appearance to meconium, and others light-coloured and greenish, like the stools of an infant. The hiccup continued very troublesome. He had voided, *per anum*, a great quantity of a very dark-coloured fluid, which had the appearance of blood in a putrescent state, mixed with a small quantity of thin excrementitious matter. He was ordered to take an aperient mixture with tincture of rhubarb.

"The 13th.—He had passed a very disturbed night, and, though he had had a little sleep, he did not feel refreshed by it, and thought he had not slept at all. He appeared more anxious and dejected than ever, and had lost all hope of recovery. His pulse was very quick and feeble, and his skin cold and clammy. He was excessively thirsty, had no appetite, but still continued to take small quantities of brandy and water or wine and water. He had voided several stools similar to those mentioned in yesterday's report. The state of Mr. Lowe was now such as to leave no hope of his recovery; still, as the belly was rather swollen, and the stools very unnatural, it was thought right to repeat the tincture of rhubarb, so as to keep up the action of the bowels, and promote the free expulsion of wind and feces, which always gave him relief. The gut since yesterday had become everted, like a prolapsed anus, about three inches in length; but was easily replaced, and supported by a soft compress with gentle pressure.

"The 14th.—As Dr. De Lys and myself were proceeding to Solihull, we were met by a messenger from Mr. Short, who informed us that Mr. Lowe was in a dying state. We sent to request that Mr. Short would take the earliest opportunity after the decease of Mr. Lowe, to apply for permission to examine his body; but neither Mr. Short's solicitation, nor our personal application to Mr. Lowe's relations, could obtain for us the permission we so anxiously wished for."

Thus, it appears that, of four instances, the formation of an artificial anus, by making an opening into the colon, has been completely successful in two: the one in a case of imperforate anus, and the other in a case of insurmountable obstruction from disease; and, in the other two, the degree of success was so considerable, that their unfavourable termination will perhaps be regarded as a probable exception than anticipated as a general result. Reverting to the case of my patient, Mrs. White, it may be worth while to observe, that the inconveniences of an anus in this situation are not such as to have caused her any regret for having submitted to the operation: on the contrary, so far from her having any reason to lament this circumstance, I believe myself that it has afforded her a moral, as

well as a physical, advantage; for she is now at no loss for an interest, and is provided with something to think of for the rest of her life.

Bath; December 12th, 1820.

A singular Case of a large Encysted Ossification arising within the Spleen, and attached to the Left Lobe of the Liver, accompanied with Hydrothorax and other Morbid Appearances. By R. W. BAMPFIELD, Esq. Surgeon.

PATRICK M'CORMICK, ætatis 53, was received into the parish infirmary of Covent Garden, on the 31st of August, 1816. In giving a history of his complaints, he stated that he had been a porter and corker to wine-cellars for several years, and had been frequently tipsy. During seven or eight years he has been sick in the morning after eating, and frequently vomited: during the last two years he almost invariably experienced sickness, and pain about the large extremity of the stomach, after eating or drinking, which terminated in vomiting. The appetite was lost, the stomach flatulent, the ingesta turned sour on his stomach, and he felt an unusually uneasy sensation in it, which he firmly believed to originate from the presence of some cork, which he thought he had swallowed in his occupation of corking bottles. His bowels have been generally costive. He has five times fallen down in "fits," while carrying weights in the street. A short and difficult respiration has gradually increased, more especially for the last three years, on going up any ascent, or after any great exertion. For five years he has been affected with a winter cough and pain of the left side. He has been a stout athletic man, but has been seven or eight years gradually declining in strength and flesh, and subject to œdema of the feet.

On the 31st of August, he complained of severe orthopnoea, and inability of making a full inspiration; of a constant fixed pain of the left side, which greatly impeded respiration, and was referred to the false ribs. He could lay down on the affected side, or on the back with the head and shoulders raised; but he felt uneasiness like suffocation, and an anxious respiration, when he turned on his right side. He was affected with a cough, alternately dry and moist, but hard. The pulse was unusually hard and strong, beating about eighty-four strokes in a minute. His bowels were obstinately constipated; pyrexia was present; vomiting was frequent; he had anorexia; his cheeks were of a slight purple hue, and his expression of countenance denoted anxiety and suffering. The quantity of urine did not deviate from the healthy, but more than one-half of it coagulated by heat alone.

Mittatur sanguis ad remissionem doloris. Saline purgatives were prescribed, but every dose was rejected from the stomach, which rendered it necessary to commute the fluid for the solid form of purgative, and calomel and jalapæ pulvis were administered in doses repeated at proper intervals; but they did not operate copiously until two days had elapsed. The bleeding and purging procured considerable relief from pain. The blood drawn exhibited a thick coriaceous buff, and was cupped. A large blister was applied to the left side; and saline medicines, with acetum scillæ et ipecacuanha, were given. Low diet was ordered.

The hydrargyri submuriæ, taken as a purgative to the quantity of ten grains only in two days, excited a free salivation; which was not intended, but had the happy effect of enabling the bowels to act subsequently without any assistance from purgatives.

It will, perhaps, be only necessary to state generally, that the breathing and pain were in some degree relieved by the medicines above mentioned, and a succession of blisters to the left side. The pulse continued hard and strong, varying from 72 to 84. The orthopnœa and incapability to make a full inspiration continued. Latterly, he could only lay on his back with composure, with his head and shoulders more and more raised, until they reached the vertical position; and, finally, he could only breathe with his body inclined forwards. He every day vomited nearly the whole of what he had swallowed, more especially if taken in considerable quantity. At his own request the bleeding was thrice repeated, and afforded a transitory relief: the blood always exhibited the same inflammatory appearance.

On the 20th of September, the *Tr. digitalis* was prescribed instead of the squills; but it occasioned such a constant sickness and vomiting as induced me to omit it, and resume the use of squills: he, however, thought himself so much better after the use of digitalis as to sit up. To relieve the stomach affection, bitters with alkali were given for a day, at two distinct periods, but they instantly aggravated all the symptoms of hydrothorax; and, when the extract of gentian with soda exsiccata was given, their effect was so injurious as to induce the patient to suspect some very narcotic drug had been administered, and he desisted from their use after taking ten grains of the extract and five of the soda.

On the last days of September, and until he died, the pain of the left side, (for he never complained of any in the right,) the orthopnœa, frequent vomiting, inability of lying down, and the œdema of the lower extremities, increased. On the 1st of October, he could only breathe with difficulty by inclining

himself forward in bed; his respiration was quick and anxious, and his pulse about 106. On the 2d and 3d, he could only perform respiration by resting his arms extended on the back of a chair, or on a pillow laid on the table, and inclining his body forward: his lower extremities swelled hourly, became quite anasarcaous, and of an enormous size before death.

The body was opened by Mr. Coulthred, surgeon, Borough-road, and myself, on the 4th of October, the day after he died. The thorax was first examined. The pericardium contained about 3iiss of a serous fluid. The heart was larger than usual, and contained six coagula of blood, formerly termed polypi. Its fibres were flaccid, and of an amber colour. The coats of the arteries arising from it had a similar appearance, were soft, and had lost their usual elasticity. In the left cavity of the thorax there were four pints of a serous fluid. To the pleura costalis were adhering several pieces of coagulable lymph, transparent and of a gelatinous consistence, and some loose portions floated on the surface of the serum. The left lung was of a purple colour, not more than one-third of its natural size, and was attached to the anterior and posterior parts of the pleura costalis by membranous bands and close adhesions. The right cavity of the thorax contained two pints of fluid similar to the left, and similar portions of coagulable lymph. The right lung was larger than the left, but adhered by similar means to the whole length of the mediastinum. It was also of a dark-purple hue.

The cavity of the abdomen contained some serous fluid, in which were floating pieces of coagulable lymph, similar to what were met with in the lungs. The spleen exhibited a very singular morbid appearance; the other viscera were healthy. From the centre of the substance of the right end of the spleen arose a spherical bony tumor, resembling in size and figure the head of a fetus of seven months old, which at its right hemisphere was attached to the left lobe of the liver, which it had stretched and elongated. The long diameter of the tumor is three and a half inches, the short three inches; its largest circumference about eleven inches. The average thickness of the bone is one line, and it is covered, in many parts, with cartilage of various degrees of thickness. The tumor is hollow, and contained seven ounces of a fluid, chiefly serum, in which were floating small particles of a cetaceous substance, shining like mica. Its external covering, or periosteum, appears to be an elongation of the peritoneal covering of the spleen, where it is not enveloped by the substance of that viscus; and its internal lining appears to be a secreting membrane proper to itself. One-half of its external surface is covered by the substance of the spleen; the other by peritoneal membrane, and, generally

speaking, by cartilage; the left lobe of the liver was united to the latter portion of the tumor, or right hemisphere, by adhesion of its peritoneal membrane, and by membranous bands.

The tumor adhered to the peritoneum behind, by which it was fixed in its position posterior to the stomach.

The right portion of the spleen is irregularly distributed over the section of the tumor imbedded in it, and gradually diminishes in thickness, until its tenuity is so small as to be lost in the membrane covering the right section of it. The spleen would probably be of its natural size if separated from the bone. The right end of it, and the portion covering the bone, were of a natural colour; the left end was of a dull flesh colour. The preparation is in Mr. Brooks' museum. From my own researches, and the observations of some learned physicians and surgeons who have seen this morbid appearance of the spleen, it is inferred that there is not a similar one on record.

The laws of nature, in the organization and growth of morbid as of natural parts, are equally concealed from human wisdom, and, perhaps, can only be known to nature's God; yet, from the little knowledge we possess on this subject, we may venture to offer a speculative opinion that this tumor was, probably, in its origin, a membranous cyst formed in the right portion of the spleen, the layers of which were gradually converted into cartilage and bone in its progressive growth, during which it irregularly separated the substance of the spleen, spread it on its surface, and, on reaching its peritoneal covering, extended it, and appropriated it to itself as a periosteum. That this tumor originated in the substance of the spleen, would seem evident from two circumstances: first, that the section of the tumor imbedded in the spleen is closely connected to its substance, and has no membranous covering; secondly, because the peritoneal coat of the spleen is extended over the section of the tumor exterior to the substance of it, which is also irregularly divided over its circumference.

The immediate cause of death appears to have been an accumulation of water in the chest and pericardium; but, how far the morbid structure of the spleen might have operated as a remote cause of hydrothorax, I cannot offer any reasonable conjecture. The severe affections of the stomach may be attributed to the mechanical pressure of this rude mass, more particularly when the stomach was distended, and is another proof that very untractable affections of the stomach may be caused by morbid derangement of the viscera contiguous to it. The coagulable urine observed in this case proves that it may be present in dropsy, when the viscera are both diseased and deranged.

*37, Bedford-street, Covent Garden;
October 8th, 1820.*

Observations on the Means of Reducing Strangulated Herniæ; in a Letter to Mr. R. W. BAMFIELD. By Dr. SHERWEN.

I HAVE lately read, with much satisfaction, your paper in the Medical and Physical Journal of September last, on the advantage of long-continued horizontal position in an old and otherwise irreducible entero-epiplocele; which induces me to offer, through your hands, for the same publication, a short account of two cases of acute incarcerated herniæ, in which the successful treatment was perhaps not less owing to position than to the employment of other confessedly powerful means. But, before I enter upon the cases, permit me to remark, that, in my opinion, the great progress medical science, both general and particular, hath made of late years, may in a great measure be ascribed to the rapidity with which medical facts and information are now promulgated and propagated through the medium of periodical publications; and I know of none that has obtained a more extensive circulation than that in which your excellent paper appears. Heretofore, many a valuable fact was often confined to a particular medical man or his immediate medical friends; with one or more of whom such facts may have been buried and lost to the profession at large: whereas, now, no sooner does a valuable observation occur to any respectable member of the profession, or no sooner does such an one imagine he has made a valuable observation, than it finds its way into some periodical publication, where it either meets with approbation or is soon refuted by the keen eye of medical criticism. But to return.

I spent a considerable part of last summer at Enfield, where I was requested by Mr. Jessop, who had been more than twenty years the assistant of my successor Dr. Clarke, to visit a patient of his, then in the second day of a painful incarcerated inguinal hernia, which had resisted all his attempts at reduction. The patient, with an old and ill-adapted elastic truss, had been working hard in the hay-field, and probably drinking freely. His skin was hot, face flushed, and his pulse far from feeble; but, on account of his age, 75, he had objected to bleeding; which was now immediately performed, the tobacco-clyster, without loss of time, administered, and a large dose of calomel prescribed. The patient was now placed, with his head and shoulders resting on a mattress, upon the floor of the chamber, with his legs and thighs upon the bed, as high as the furniture would permit; and, in this position, napkins wetted with a refrigerating solution, which had been previously very properly applied, were ordered to be continued, and repeatedly changed during five or six hours. At the end of that time Mr. Jessop was directed to attempt reduction again; when he had the

pleasure to find the contents of the tumor, about the size of a large clenched fist, slipped up at the first touch. The febrile symptoms, however, continued to run so high that he found it necessary to repeat the bleeding in the course of the night: and from that time the recovery was as rapid as could be expected.

Twenty-three years ago I found the same position produce the same good effect, without either bleeding or the tobacco-injection, but with the advantage of the cold application, in the treatment of a patient of the same age, in Tuckey-street; and I now take blame to myself for not having then communicated the result of the practice. The best apology I can offer is, that it was my intention to have done so through the medium of the *Medical Spectator*; the continuance of which work was impeded, first by a domestic affliction, and afterwards by the dreadful fire in Red Lion-passage, Fleet-street; where the greater part of the first edition of the three volumes was consumed.

In respect to this continued position, it must be evident that if the finger and thumb could be applied, within the abdomen, to that portion of intestine contiguous to the internal ring, the slightest effort would immediately retract a contiguous portion; which the rudest, the roughest, and, let me add, even the most judicious, pressure *ab extra*, with every advantage of scientific relaxation of muscles and ligaments, would in vain attempt. But, what is denied to the finger and thumb,* is perhaps afforded by due attention to this position.

Let one or more folded sheets be tied from one foot-post of the bedstead to the other, at proper heights. Cover these with blankets and pillows; and let the patient be so placed that his body shall rest partly on the neck and shoulders on other pillows in the bed, and partly by the legs hanging over the top of the highest folded sheet; or the legs may be placed over the arms of a sofa, with which most houses are provided.

In this position the whole contents of the abdomen gravitate towards the diaphragm; there is a constant, but gentle and almost imperceptible, tendency to retraction of the confined

* An intelligent medical friend remarks, "I should not be surprised if hereafter some enterprising surgeon of plain good sense (the best of all sense) should adopt your hint; and, by making an opening into the cavity of the abdomen, apply his finger and thumb in the manner you mention, and with facility effect the reduction." May it not be added, will the patient, by such an operation, undergo as much pain,—will he have as large a surface exposed,—or will the internal cavity of the abdomen be more subjected to the danger of inflammation from the admission of external air, than in the usual operation?—will there be as much danger of wounding either the epigastric artery or the spermatic vessels? If none of these dangers will be augmented, but all of them lessened, why should he hesitate to adopt the hint? The practical operating surgeon may probably advance unanswerable objections: none occur at present to the writer,—old adhesions excepted.

portion of intestine from the herniary tumor : and even the efforts to vomit, (which in the common position are injurious,) and the inverted peristaltic motion, become auxiliaries. A few hours of this position, with an assiduous application of the cold napkins, will retard the progress of inflammation, and, with the other well-known means, give nature fair play.

I cannot help taking this opportunity of remarking, that I think our anatomical and surgical teachers are inclined rather too much to urge the necessity of early operation in these cases. It may be difficult to point out the precise and most proper time for decision, as that must always be left to the judgment and experience of the surgeon : every prudent one will, if possible, obtain the sanction and concurrence either of a professional friend or senior practitioner. A bold and dextrous operator, fond of the knife, may be apt to operate too soon : a timid or an unskilful one, too late. Both may, I believe, be often relieved from embarrassment, by attention to the position here recommended ; which another case in point may further explain.

A gentleman, labouring under painful, acute, incarcerated hernia, called in the assistance of one whom I know to be a very skilful and experienced surgeon ; who, after every proper attempt at reduction, urged the necessity of an operation, which the patient resisted on this ground, that his family had been successfully attended by the junior partner, then absent, for whom he would wait. The junior partner arrived in the evening, when, on failure of reduction, the operation was again mutually urged. But it was now found that the previous deferring of compliance on account of the absence of the junior partner, had been merely pretence ; the patient having determined, in his own mind, not to submit to any operation. On repeating the visit next morning, the two surgeons had the pleasure to find their patient in a state of perfect security,—spontaneous reduction having taken place during the night.

I could here add other cases,

*Quæque ipse miserrima vidi
Et quorum pars magna fui,—*

in which some patients have been lost, who, I am now much inclined to believe, might have been preserved, had I more early had resource to the method I am here recommending.

It may, perhaps, be said there is nothing new in the proposal, —other surgeons having been, from time immemorial, occasionally in the habit of placing the patient's legs over the shoulders of an assistant, with his head hanging down. I know that this has often been done ; and by so doing, and sprinkling the tumor with cold water, I have myself succeeded in dangerous cases : but the difference is great between placing a patient

in this position during a few minutes, while reduction by the hands is attempted, with all the abdominal muscles in a state of tension; and the gradual, slow, and easy effect of quietude, and the uninterrupted efforts of nature, whilst the patient is in a state of relaxation and comparative repose.

I have not the honour, Sir, of your acquaintance; but your valuable paper is the parent of mine, which, but for the stimulus of yours, might have continued, *de die in diem*, in a state of intended preparation and publication.

Bath; Oct. 30th, 1820.

P. S.—On this occasion it may be right to republish what I apprehend may have already appeared in the Medical and Physical Journal,—viz. the popular practice in Russia in cases of incarcerated hernia, as mentioned by JOHN CONRAD HILTEBRAND. “A pot, capable of holding several pints, is applied in the manner of a cupping-glass to the abdomen, previously rubbed with oil or soap. The parietes and bowels are thus drawn (with pain) into the pot, and the parts contained in the rupture into the abdomen.” He asserts that many, under very desperate appearances have been thus cured. He might have added, that it was the dictate of plain good sense. The cupping may, with the least inconvenience, be performed by dropping a piece of silver paper wetted with rectified spirit, and lighted, into a large bell-tumbler, or any other vessel properly capacious.

Strictures on Dr. KINGLAKE'S Paper on the Position of Fractured Limbs. By CHARLES W. SMERDON, Esq. Surgeon.

I SHOULD not have expected to witness a physician of respectability coming forward as an advocate of a practice in surgery so nearly exploded (at least in this country), as that of the straight position of fractured limbs. Such conduct in many men might very well be passed over in silence; but the talents of Dr. Kinglake are such as make it appear to me to be a point of professional duty in those who reprobate that practice, to take into consideration the arguments he advances on this occasion. Before, however, I adduce the strictures I have to make on them, I cannot neglect to remark, that Dr. Kinglake does not seem to be well acquainted with the character of the professional acquirements of British practitioners, or he would not have gravely asserted that they “sometimes do irreparable mischief by the absurd practice of aiming at salutarily stimulating the surfaces of the broken ends (of bones) by moving them on each other.” Without fear of contradiction, I will answer for every surgeon who has received a regular education, that such a glaringly “absurd practice” was never conceived by him to be a measure at all plausible.

Dr. Kinglake sets out by saying, that “the earliest periods

of practical surgery must have been those in which theoretical considerations could have but little influence." Theoretical considerations! What, is that still theoretical, which POTT wrote upon and advocated after a practice of more than thirty years, and which has since been sanctioned and followed by all the first surgeons in London? * Again he says, "to have insisted at that time that laying a broken bone in a curved position, would be favourable both to the osseous union and to the eventful straightness of the limb, would have been thought unaccountably strange, and altogether untenable." Dr. Kinglake has unintentionally written this sentence very obscure. Certainly, to place a broken bone in a curved position, is not the way to make it straight. In several places he has very improperly made use of the word curved instead of bent. Now, a curve, in the common acceptation of the word, signifies a part of the circumference of a circle; but, when a joint is bent, it forms an angle more or less acute, but certainly not a curve. But, to drop this sort of criticism, I will contend, that it is absurd to suppose that the science of surgery is to be stopped in its march towards perfection, because our forefathers would have thought its present improvements "unaccountably strange and altogether untenable."

In advocating the bent position, it is not intended to assert that all the muscles of a fractured limb are placed in a state of relaxation; and it is puerile to suppose that so good an anatomist as Mr. Pott intended literally to convey such a meaning. All that can be meant is, that such muscles are placed in a relaxed state by the bent position as are most likely to draw, by their contractions, the fractured pieces of bone from each other, and which are usually placed in a relaxed state by every animal when asleep or in a state of rest: † these muscles are the flexors and the adductors. Suppose that an artist were to pourtray a sleeping beauty, would he place her with her back perfectly straight, and her limbs stretched out to their utmost extent, as

* Without intending to express any opinion on the merits of either mode of treatment, we may remark that Pott's practice is not universally followed by surgeons of the first rank in London. At one of the hospitals here, the straight position of the limb, in cases of fracture of the thigh-bone, is generally adopted by the whole of the attending surgeons.—EDIT.

† It may be here worthy of remark, that, in birds of flight, the largest muscles are those flexors which bring the wings to the side, when they are stretched out in the act of flying. These muscles, when at rest, are always in a relaxed state, and undoubtedly require more perfect freedom from distention than their antagonists' muscles do. The same may be remarked in the human subject, between the deltoid, the spinati, and other muscles that raise the arm, and those which are intended to bring it again to the side. The one set, for their rest, require perfect relaxation; whilst the others receive the same degree of ease although they are almost constantly stretched out. In short, it is evident that some muscles naturally require, for their rest, a perfect state of relaxation; whilst the antagonists to them receive the same quantity of rest, although kept upon the stretch.

if they were pinioned? Certainly not? He would, I should think, gently curve the back, throw her arms across the breast, and bend the joints of the hips and knees. This position, I believe, is that of the most perfect rest,—that, too, which nature points out as the best adapted for restoring to the muscles that vigour which they may have lost by previous action. Now, is it not reasonable to place muscles in the most natural state of ease and comfort, which are to remain in a fixed position for at least four weeks? and can any other position be better adapted than that which man naturally throws himself into when his muscles require ease and rest? One of the characteristic properties of muscular fibre, is to contract on the application of stimuli. Now, we know that the stimulus of distention is a very powerful agent for this purpose, as we may observe in the action of the heart, the blood-vessels, and the bowels. We have undoubted evidence, likewise, that the voluntary muscles contract powerfully, and remain permanently rigid, when they are violently and suddenly stretched: hence arises the only difficulty which is met with in the reduction of dislocations,—a difficulty which it is sometimes impossible to overcome without the most powerful mechanical re-agents.

In all cases of fractures, the great object of the surgeon is to effect a quiescent state of the muscles of the broken limb; and for this purpose it is his duty, as much as lies in his power, to withdraw from them every stimulus of whatsoever kind that is likely to affect them. Now, if distention be a stimulus to muscular fibre, (which, I presume, no one can doubt,) does he not withdraw it by placing the muscles in a relaxed position? and does he not, by this means, contribute considerably towards the general indications?

The next best means to produce a permanently quiescent state of muscles, is to brace them up moderately tight with a suitable bandage. No one will doubt this who has seen the effect of tight bandaging in chronic rheumatism, where the muscles are principally affected. In cases of lumbago attended with pain and spasmodic twitchings of the muscles of the back, I have seen the greatest relief obtained by applying pads on the lumbar mass, bound on as tight as possible by means of a broad flannel swathe. Nor is the good thus derived evanescent: if the plan be accompanied with perfect rest in a horizontal position, a cure may speedily be expected, without the aid of any other medicines than what are indicated by the state of the bowels.

If we instance a case of fractured thigh-bone, and enumerate the muscles which are relaxed by the bent position, we shall find that these exceed very considerably in power those which are placed in opposition to them. The position here meant is

a slight flexion of the hip and knee, by the placing of a pillow in the ham. First, the *psœ* muscles and the *iliacus internus*, which are inserted into the *trochanter minor*; 2dly, the *sartorius*, the *rectus*, and the *gracilis*; 3dly, the *pectinalis* and the *triceps adductor*, which are inserted into the whole length of the *linea aspera*; and, 4thly, the long flexors of the leg, the *biceps*, the *semi-membranosus*, and *semi-tendinosus*, which are inserted into the head of the *tibia* and *fibula*. These are all muscles of considerable power and extent, and, it is evident, are placed in a relaxed state by the bent position. A view of the skeleton, however, must convince us that some of these muscles, by spasmodic action in the case we are supposing, are capable of doing more mischief than the others. Thus, the three heads of the *triceps adductor*, which, taken collectively, form a very large mass of muscle, are those which, I believe, tend more to occasion deformity in the uniting of a fractured femur than all the rest put together. Their origin and insertion, in the entire state of the bone, point out that they must act powerfully in bringing the thighs together, and crossing them, if required. When, however, the femur is fractured, the action of the *adductor brevis* will draw the broken end of the upper part of the bone from that of the lower part; while this, the inferior portion, having now no fixed position, will be powerfully drawn, by the lower fibres of the *magnus* and *longus*, upwards and inwards.*

Dr. Kinglake goes on to state, "the range of involuntary action given to the muscles connected with a fractured bone when in a relaxed state, is such as operates most injuriously on the stationary position to which it would be desirable constantly to confine the broken ends of the bone." Here, for reasons which I have before stated, I am entirely at issue with him. If distention be a powerful stimulus to muscular fibre, and if muscles are liable to involuntary action by being stretched, whether is it the more rational plan, in order to prevent that action, to relax them, or to place them in a state of distention? Besides, when the rigidity of the muscular fibres is taken off, a bandage can be placed on a fractured extremity with more cer-

* If it be a matter of much consequence to relax the *triceps femoris* in cases of fractured thigh-bone, it is necessary to adopt that sort of position which will secure this point with the greatest possible ease to the patient. It may be questioned if the usual posture of placing the limb on the outside, is the best under all the circumstances of the case. Certainly it is not, if the pelvis be not brought round, as it were, to the limb, so that the patient shall lie completely on his side: otherwise the *triceps* would evidently be placed on the stretch, and the fracture, if an oblique one, be in great danger of being displaced. Perhaps, the best general position is to place the patient on his back; to keep the joints bent, by placing a pillow, well rolled up, in the ham; to rest the limb on the heel, supported properly, and well kept up; and to place the whole extremity so that it may incline a little inward towards its fellow.

tainty of confining the bone by pressing the relaxed and pulpy muscles close around it, than if those muscles retained their natural plumpness and resistance. But Dr. Kinglake seems to have fallen out with all the modern improvements in the treatment of fractured limbs: improvements which, I will venture to affirm, a very large majority of hospital surgeons in this and the sister kingdom are daily in the habit of acting upon.

The preference given to long splints which comprehend the joint above and below the fractured bone, must rank as a very considerable improvement in modern surgery, and which every one competent to judge on the subject, and who at the same time is unbiassed by either prejudice or a love of singularity, will, I believe, allow. In fractures of the thigh-bone, where the muscles are large and powerful, and their range of action of considerable extent, it is very doubtful if short splints be of any other service than that of keeping the patient's mind quiet by a strict adherence to ancient and well-known custom: but I will go a step further, and venture to affirm that they may be productive of harm where there is a disposition in the muscular fibre to spasm, by the very partial and unequal pressure which they must make on the long muscles of the limb and on the bone. Let us instance the vasti muscles, which have their origin from the trochanters and from the whole length of the bone, and are inserted into each side of the patella in the entire state of the thigh: when these muscles act, they extend the leg; but, if the femur be fractured, their contractions will draw the inferior extremity of the bone upwards, and thus increase the deformity. As a remedy for this evil, (as far, at least, as a remedy is within our power,) common sense, I should think, would point out the propriety of making an equable pressure throughout the whole length of such muscles: and where, from the shape of the limb or some other cause, this cannot be done by the splints alone, pads should be introduced to make up for the inequality.

In his treatment of compound fractures, "attended with much contusion, and even threatening laceration of the soft parts," Dr. Kinglake must have been more than ordinarily successful, if he has obtained effectual and speedy cures by "binding up the soft parts with sufficient strictness to keep the broken ends in contact." I agree that it should be the first wish of the surgeon to bring about a union of the external wound as soon as possible; and, where this can be done by Dr. Kinglake's plan, it is but fair to say that a better cannot be adopted. But, in cases where the contusion of the soft parts is very considerable, and where the loss of vitality of such parts may be seriously apprehended, as a consequence of the violent inflammation which generally follows contused and lacerated wounds, I apprehend that such a practice is very likely to produce the evil

which it is intended to avoid. In such wounds of muscular parts the re-action which follows is always in the extreme, and the blood-vessels of the part become excited to the utmost degree, which will continue until nature relieves herself by an effusion into the cellular tissue, as is indicated by tumefaction and a comparative relief from pain. But how can such a salutary process be brought about if the affected muscles be bound up tight by a bandage? If it were necessary in order to deprecate a practice so evidently injudicious, I could here relate a case of this sort of wound, in which the most extensive sloughing was obviously produced by tightly bandaging the part, and by the application of cold evaporating lotions, with the view of bringing about union by the first intention, as it is called. In all such cases it is manifestly the duty of the surgeon to be rather the servant than the master of nature, and to assist her in her salutary operations by relaxing fomentations and local bleeding, rather than to impede her by the practice here pointed out.

Clifton, Bristol; Oct. 1820.

A Series of Cases, exhibiting the Effects of Pressure in Cancerous and other Diseases. By SAMUEL YOUNG, Esq. Surgeon to the Cancer Institution, Gerard-street, Soho; Member of the Royal College of Surgeons, and of the London Medical and Chirurgical Society, &c.

[Continued from vol. xliv. page 364.]

THE circumstance of the existence of fungus under a diseased state of integument in cancerous and other morbid alterations and growth, being frequently mistaken for the presence of matter or pus, was generally alluded to in the last article of this series, when the cases with which we have been favoured by Mr. Trowbridge were reported; and since, in practical application in many instances, much mischief may be produced, or avoided, according to the means employed, a further consideration of the subject in this place may not be unimportant; which, on the contrary, would have been too digressive in the midst of the report of the cases just alluded to.

This state of fungous disease is frequently very deceptive, and, to usual observation and tests, leads to the conclusion that matter has been formed; and, in some cases, the life of the patient will inevitably be lost, if an incision be made for the purpose of evacuating the supposed fluid; while, in all of such instances where an opening is made, mischief in various degrees must necessarily result, without a possibility of any attendant good.

We are very apt to follow a common routine practice with-

out reflection, and consequently without principle; and we probe, and cut, and poultice, mechanically. As a necessary preliminary, a formality not to be dispensed with, a surgeon frequently probes a wound at first sight, for no known purpose of good, and quite innocent of any rational prospect of it; though really guilty of giving unnecessary pain, and running the risk of much probable mischief, to his patient. Such unnecessary interferences and meddlings were called, by my good friend the late Dr. Denman, most justly and appositely, "the impertinencies of surgery."

In following this sort of dull, unmeaning, and unprincipled routine work of practice, if I know several cases, during part of my life, where I have done mischief, I think it is pretty clear I must have done a great deal more of which I have been wholly ignorant; and, as I feel assured, at least in this instance, that I do not stand *alone* with the profession, my reflection and repentance on this subject may prove useful to others.

Poultice, experience sufficiently shows, is only the nurse and feeder of disease in many instances: even in common abscess, its undue employment is a great abuse, and leads to much mischief. And yet how indiscriminately it is used,—even to court suppuration, which might, and (generally speaking, if possible,) ought, always to be avoided; and even in instances where there is an evident growth of morbid structure,—the increase of which it must necessarily favour,—still we see the poultice, in conjunction with fomentations, a common practice.

And then, in heedless uniformity with the rest, and in completion, a lancet or knife is plunged into any part that may afford the appearance of a point, as in common abscess; and thus a door is opened for new, and perhaps irremediable, mischiefs; and the protrusion and increased growth of a fungus, instead of the escape of pus, is the consequence.

In all equivocal cases the great object of good practice is to preserve, if possible, the integument entire, and reduce the action of the vessels of the part. The common routine of fomentations and poultices will, generally speaking, only accelerate disease; in some instances, produce fatal mischiefs, which might have been avoided; and, in all, augment, at least, the evils of the case. I speak most decidedly from facts and my own experience. A lady, for example, about eight years ago, had a small and most troublesome tumor at the angle of the jaw under the right ear. The whole of the covering integument became discoloured and diseased, was drilled through like a cullender with small holes, most exquisitely sensible to the touch, affording but very spare suppuration, generally of a very ichorous and irritating nature; and the entire character of the disease was of a very malignant, though equivocal, nature,

and taking on, in the earlier stages, the appearance of abscess. It was however, after a most obstinate resistance, entirely reduced and removed under the treatment of pressure.

Some time after, the lady being then in the family-way, a small tumor appeared upon, and just about the centre of, the breast-bone. It increased, though not very rapidly, to a considerable bulk; became discoloured at its apex, where it afforded the appearance of a point as in common abscess, as well, ultimately, as the evidence to the feel of the existence of pus. It was poulticed and fomented according to the usual routine of practice; and, finally, an opening was made to discharge the contained matter. This was done in conjunction with a very intelligent practitioner, who has great celebrity as a physician and surgeon in the country where he lives. In making the opening, I found very considerable resistance, not of the integument, but when the abscess-lancet had penetrated, and cutting downwards and upwards to enlarge the opening, as if tough cords or bands were opposed internally to the edge of the instrument. Nothing but a thickish, and rather dark-coloured, blood followed, somewhat copiously, this attempt. My professional friend, just mentioned, then took the lancet from me, and made a deeper incision, thinking I had not sufficiently penetrated into the cavity of the supposed abscess; but nothing followed this attempt but a further discharge of the same sort of blood as issued on the first incision, except here and there a flake of whitish matter, denoting something of a curdy or scrofulous nature. Poulticing, in almost all its varieties, was tried in this case to no effect, except a bad one: the growth of an immense fungus was the result; and which ultimately destroyed the patient after long and great suffering.

During the pregnancy of this lady, the progress of the disease was comparatively slow, and evidently suspended. But, after her delivery, (the child, and especially in contrast to the then state of the mother, being remarkably fine and healthy,) the growth and ravage of the disease became so striking and formidable as to exclude every hope of relief.

It is rather singular (not indeed rather, but, according to my present view of things, strangely and most infatuatedly stupid,) that the treatment by pressure of the breast-disease in this patient never once occurred to me, although I had but shortly before used it, and with success too, to the disease at the angle of the jaw;—diseases similar in both instances, if not precisely alike in their natures. But at that time I was entangled in the common practice, and, although I had applied it with success in a few instances of breast-diseases, the treatment by pressure had not been contemplated by me, even in theory, by any means to the extent of its application that it has been since;

and which, in practical application, the experience and absolute knowledge of its beneficial results, has since been almost daily enlarging and confirming.

Such and similar instances as this case affords, have made an impression on my mind, and resolved it to contemplate and consider things well before a plan of treatment is adopted. Not to do mischief, is no small perfection in our profession, as daily experience shows; and it is incomparably better to do nothing than to do harm.

How far the mischiefs of the chest-sore, in the case just stated, might have been wholly avoided by a different and very opposite course of treatment being adopted, may, in this individual instance, be equivocal. But there can be neither doubt nor question but that the whole of them were accelerated and aggravated by the plan that was adopted; and that, if the integument had been supported by compress, and bandage in conjunction with the evaporating treatment, the disease would have been retarded and modified, at least, if not entirely suppressed and removed. And, further, neither question nor doubt can be entertained, that to have done nothing would have been incomparably better in this case than doing that which was done.

In other cases, however, a plan of poulticing, &c. must inevitably prove fatal; and which might be entirely avoided, as experience shows, by adopting the opposite plan of compression, &c. In the case, for example, of the late Duke of Buccleugh's female domestic, the last reported case in the *Further Minutes*, &c. published in 1818, and so frequently referred to, had the disease proceeded but a few weeks longer, a discoloured point at the top of the breast would have presented all the common tests of the existence of pus; and if, contrary to the history of the case and all good practice, an opening had been made, as we see in common practice most probably it would have been, for the purpose of discharging the (supposed) contained fluid, the patient would have been inevitably lost by the aggravation and ravage of the fungoid disease, which then existed.

I have frequently had to observe, on various occasions, as far as my own observations have extended, in cases where any thing of an equivocal and mixed nature has been shown, as well as in the more decided instances of schirri, the evil results from leeching, fomenting, and poulticing; and up to the present period, on a large ground of experience, the accumulated mischiefs so decidedly resulting and originating from such practice, authorize from me the most unqualified reprobation of it.

Leeches by their bites disease the skin, with, in no instance, any permanent relief, and scarcely with any temporary allevi-

ation, while they hurry on to the formation of tubercular disease; the first to form it, but the very last to yield. Fomentations and poultices, as already observed, only tend to weaken the integument, already too weak to resist disease; and, by giving laxity and facility of circulation, only accelerate the growth of that which ought to be the object of all rational practice to suppress. Take, for example, the first case as reported by Mr. Trowbridge, with the many score of similar cases I have seen, and ask what would have been the result of leeching, fomenting, poulticing, &c.?—The inevitable aggravation of a destructive and fatal disease.

In making this summing-up, I give it with all due respect to practical truth, after the most impartial and correct observation my mind is capable of, authorized by a large, but not a boasted experience; and, if *positively* given, that *positiveness*, let it be understood, is the result of conviction, not presumption,—of errors self-acknowledged and corrected: and, as I feel my errors have been most brotherly in common with the profession at large, I have only to say, let there be a general confession, and let us amend our lives.

I shall now take leave to state a case from the many of a mixed sort,—*i. e.* where common inflammatory symptoms of the part are conjoined with the more specific or morbid progress of cancerous alteration, and where the beneficial results of a judicious practice by pressure were decided and conspicuous.*

Before, however, the detail of this case is given, it will be proper to remark, that the very formidable case just alluded to, of the late Duke of Buccleugh's female servant, has gone through the test of several years in the confirmation of perfect recovery. Since the report of her case, she has married, become a mother, and suckled the child freely with the formerly diseased breast. This information has been the result of frequent communication from the party. Shortly after her delivery, the sister of the patient called upon me, stating that a swelling or hardness had come on, on the formerly diseased breast; of which (although her medical attendant attributed it to the common occurrences in such cases,) they were anxious I should be informed. I merely ordered the acetated ammonia and spirituous lotion to be used, with a request that, if the swelling or hardness did not subside, or any difficulty of suckling should be experienced, that I should be immediately informed of the circumstance. I heard no more of the case then. A twelvemonth, as nearly as I can recollect, has now elapsed; and I have since heard that the patient has been perfectly well, and other patients have been recommended from the same source.

[To be continued.]

* This case will be inserted in the next Number.—EDIT.

On the Treatment of Ulcers of the Legs by Strapping. By A. COPLAND HUTCHISON, Esq. Surgeon to his Royal Highness the Duke of Clarence; to the Westminster General Dispensary; to the Royal Metropolitan Infirmary for Sick Children; Medical Superintendent to the General Penitentiary at Millbank, Westminster, &c.

IF Mr. Baynton's plan of treating ulcers of the legs by adhesive straps were as generally pursued in the different London hospitals and dispensaries as it is in the royal naval hospitals on the coast, I am thoroughly convinced that its great superiority over every other mode of treatment would soon become apparent, and be more generally adopted in practice.

The expense of strapping in these institutions cannot, surely, be the cause of its almost total disuse; for, when the more speedy recovery of the patient under this treatment is taken into consideration, and hence his or her discharge from the institution the sooner effected, the argument of expense must necessarily fall to the ground.

In the application of the straps and roller, there has been nothing left for future observation by the ingenious deceased author of the plan; and therefore it were almost useless to repeat, that it ought to be done with great care and attention as to smoothness of application and equality of pressure. But there is one circumstance, which, however trifling it may appear, is yet of some consequence, both as it regards a freedom from pain to the patient, expedition, and an appearance of adroitness in the surgeon, which, I believe, has not been noticed by any writer on the subject. It is the manner of removing the old straps in order to replace them by new ones.

Taking off adhesive straps one after another, by detaching an end, and bringing that end round the circumference of the limb, is a tedious process; and, if the hairs on the sound skin, which have been shaved in the first instance, have again grown, considerable pain and inconvenience will be produced. In order to obviate this as much as possible, it has been my plan to insinuate a probe underneath the straps, and in contact with the skin, on the opposite side of the limb to the seat of the ulcer: the straps are thus easily detached from the subjacent parts; and, by gently raising one end of the probe so introduced, a pair of crooked surgeon's scissars, passed in the direction of the probe, will very readily divide the whole at two or three snips of the instrument. The divided ends of the straps are, as a whole united body, to be then turned off to the right and left, so that the ulcer shall be the last part uncovered, and the new-formed skin surrounding the circumference of the ulcer will thus be left undetached from its subjacent tender adhesions.

The hint I am now offering to the profession may appear to

some of so trifling a nature, as hardly to merit being recorded ; but it has not been so considered, when seen put in practice, by some physicians of eminence in this town ; one of whom was the patient, and at whose suggestion I give it publicity through your Journal.

While on this subject, I deem it incumbent on me to notice thus publicly, with a view to its being remedied in the proper quarter, the very parsimonious allowance of adhesive-plaster that is annually supplied to ships of war : and it is, at the same time, but justice in me to state, that every representation of mine to the heads of the medical department of the navy, which related either to the good of the service generally or to the comfort of individuals, being seamen or marines, has hitherto been invariably most promptly attended to.

On referring to Form 91 of the Naval-Hospital Instructions, I find that but three pounds of adhesive-plaster are annually allowed to a frigate of the first class ; while ten pounds of Peruvian bark is the allowance for the same period and class of ship, a tenth part of which is possibly never used ; and, on enquiry at a respectable wholesale druggist's, I find sticking-plaster only 1s. 5d. per pound, whilst the best bark is 12s.

Let us now examine what will be the effect of a more liberal or proper supply of this most necessary article. There is not any class of medical men more zealous and humane in performing their professional duty than the surgeons of the British navy ; and it will be admitted, I believe, on all hands, that there is not a class of men who have greater calls on both. To such gentlemen, therefore, it must be exceedingly vexatious to be under the necessity of sending to an hospital patients labouring under ulcer, who might as well be cured on-board, if the surgeons were sufficiently supplied with the means ; and the effective exertions of such men would be thereby equally preserved to the service as if no disease existed ; for, it will be readily admitted, I imagine, by those best acquainted with the treatment of ulcer by strapping and a well-applied bandage, that such patients are, generally speaking, as competent to perform the duties of their office whilst under this treatment, as if they were free from disease. Ships of war would not then be deprived of active hands at a period when they are most required and cannot be replaced, and the country would be spared the expense of an hospital treatment.

Again. Suppose a ship to be called into action with the enemy, and amputation performed on one or more men, how distressing would be the situation of the surgeon, who might almost as well be without instruments—(his head, I was about to say,)—as without adhesive-plaster. Had our gallant little

squadron, after the battle of Algiers, not gone immediately to Gibraltar, where they were so kindly received and liberally supplied with this necessary article, among others, by the army medical officers of the garrison, I can hardly conjecture what would have been the result to the unfortunate sufferers on that occasion.

Ulcers form a great majority of the cases that fall under the treatment of naval-hospital surgeons, from the cause above adverted to ; but, during the several years that I filled the situation of surgeon to men-of-war, previous to being appointed surgeon to Deal Hospital, I find, on a reference to notes, that I had only sent four cases to hospitals ; namely, one case of compound fracture of the thigh, one of fractured cranium, one of dysentery, and one case of gun-shot fracture of the leg. Ulcers occurred in the ships of which I was surgeon, as well as in other ships, but I pursued Baynton's plan of treatment, and procured plaster at my own expense to follow it up with ; and this I believe to be the reason why no patients of this description were ever sent to an hospital from the ships of which I had the professional charge.

I should wish it to be understood, however, that where the malignant ulcer, or, as it is sometimes called, hospital gangrene, occurs on-board ships of war, the sooner such patients are removed to an hospital, away from the crew, the better ; and it is scarcely necessary for me to add, that these are not cases to be treated by adhesive straps : but, with regard to the particular treatment of malignant ulcer, I shall, I trust, have more to say at some future opportunity.

I beg pardon for this digression. I have only been acquitting myself of a public duty, which I have now no other means of doing but through the medium of the press.

Spring Garden ; 9th Dec. 1820.

COLLECTANEA MEDICA:

CONSISTING OF

ANECDOTES, FACTS, EXTRACTS, ILLUSTRATIONS, &c.

*Relating to the History or the Art of Medicine, and the
Auxiliary Sciences.*

*Floriferis ut apes in saltibus omnia libant,
Omnia nos fidem depascimur aurea dicta.*

*Extracts of the Report from the Select Committee of the House of
Commons on the Doctrine of the Contagion of the Plague, in 1819.*

Veneris, 19^o die Martij, 1819.

SIR JOHN JACKSON, Baronet, in the Chair.

Dr. WILLIAM GLADSTONE called in; and examined.

YOU are a practitioner in medicine?—Surgeon to the Naval Asylum at Greenwich.

Are you acquainted with the disease termed Plague in the Levant, and the contagion ascribed to it; and what is your opinion thereof? I was at Constantinople in 1806 and 1807; and, from having been then surgeon of his Majesty's ship the *Endymion*, I there saw some diseases of the plague, and a great variety of Asiatic fevers, highly infectious.

Was the plague at the time raging?—No: there is at all times more or less plague there.

At what time of the year?—It was in December, January, February, and March, prior to the forcing of the Dardanelles.

Will you describe to the Committee the characteristics of the plague?—It is a disease attended by sudden prostration of strength and spirits, great apprehension, and many symptoms of a malignant type. From the little apprehension the Greek physicians I saw have to come in contact with the plague, I was led to feel the pulse of their patients, which I did with considerable apprehension. I do not consider the plague more contagious from contact than through the medium of the atmosphere.

What was the consequence?—I felt no bad consequences.

How many plague patients do you suppose you felt the pulses of?—I saw three, and took opportunities of visiting them several times.

How was their pulse?—Very quick and full, frequently alternating, indicating great arterial disturbance. They were stout men; slaves belonging to the arsenal.

Had they buboes or carbuncles on their bodies?—One had an enlargement in the axilla.

A bubo?—Yes: and two of them died of the plague, I was afterwards informed.

From what you have seen of the plague with your own eyes, do you consider it as contagious?—I consider it as highly infectious. My

opinion is, that it is equally so through the medium of the diseased atmosphere of a sick chamber, as by simple contact, by feeling the pulse.

What do you suppose is the cause of the plague at Constantinople?—Diseased constitution of the atmosphere, and other peculiar causes, such as effluvia and soil, which produce endemic diseases all over the globe; from the same causes as we have epidemic diseases in England; and from the circumstance of that city standing upon hills. Many of the houses are built on ground sloping to the south-west, consequently liable to the whole action of the south-west sun; all are badly ventilated. The streets are very narrow; and they do not, I believe, possess that grand source of health, common sewers.

Are you acquainted with the suburb of Pera?—Yes.

That suburb is chiefly inhabited by Europeans?—Chiefly by Europeans.

Is it true that there is generally less plague in that district than in any other?—Perfectly true.

To what do you ascribe that?—To the houses not being so close, or the streets so narrow.

Is there increased cleanliness in that district?—More, I think, in Pera than Constantinople.

What do you mean by infection?—Disease produced by a contagious state of the atmosphere. An epidemic state of the atmosphere produced by various causes: effluvia, virous effluvia, or by contact with a particular virus.

Have you ever heard of the plague being in Great Britain or Ireland?—Yes; both, during the disease of 1665, in Charles II.'s time. There is an order in council by the honourable privy-council, present the Lord Chamberlain, the Earl of Bath, Mr. Treasurer, Mr. Vice-Chamberlain, and Mr. Secretary Morris, ordering the College of Physicians to suggest measures, and give directions to prevent the spreading the infection of the plague.

Do you consider that was the real Levant plague?—No.

Why?—At that period, as far as I have been able to trace from a variety of old authors, there was scarcely such a thing as a common sewer. The privies were accumulated under every house, probably not emptied for years; and an order was given to empty them once a month. That order originated, I believe, in the College of Physicians, after the spreading of the plague.

As you do not consider that to have been what is properly termed the Levant plague, to what do you ascribe the sickness in 1665?—To the narrowness of the streets, accumulation of filth, and want of ventilation; and, probably, a diseased constitution of the atmosphere at that period.

Then you consider it was not the Levant plague; that it was not imported, but that it originated in England?—Yes; I believe that it originated in England.

Do you found your idea of its not being the Levant plague from any historical description or nosological character of the disease?—I have not been able to trace any decided fact of its importation: a cir-

circumstance of so much importance and general interest, if true, would, I think, have been marked by every medical writer and historian of the time. I look upon the narrowness of the streets, the filth of the city, the want of common sewers, and the state of the atmosphere at that period, to have been the causes; but particularly the want of common sewers.

Was it attended with buboes and carbuncles?—Yes, as far as I am able to learn; in some cases, not generally.

Did you ever see cases of typhus attended with buboes?—Yes, in the first instance, in some inflammatory constitutions.

Frequently?—Not frequently.

Have you seen the carbuncles in typhus?—I have seen an enlargement in the groin: the carbuncle, in general, comes on the back between the shoulders.

Have you ever heard of the plague in Great Britain and Ireland since 1665?—There was a disease in Dublin something similar, but I have not any correct information as to it.

In what year?—I do not know exactly; I believe soon after the plague in London, but I am not sure.

Do you consider that our quarantine establishments have kept the plague from being introduced into Great Britain or Ireland?—No, I do not; I cannot say they have. From having been frequently under quarantine restraint myself, I have made it my business to visit most of the lazarettos between Gibraltar and Constantinople; but the source of disease is more frequently seen among the Greek vessels that carry cargoes to Marseilles, of which there was a great many sailed soon after the blockade by the British squadron under Sir John Duckworth.

If plague-infection was introduced into bales of goods in the Mediterranean, do you suppose that the present conveniences and opportunities at quarantine establishments in Great Britain sufficient to purify them from the infection?—I consider the lazarettos, as far as I am able to learn, particularly inefficient in fitment for that purpose; I mean with respect to ventilation and ballast.

Do you consider that the Levant plague can exist in a British atmosphere?—I think that is very doubtful; but I think there is great encouragement to nurse disease, if any is imported into the lazarettos. There are some of the lazaretto ships, as I am given to understand, where the shingle-ballast has not been shifted for many years; and in many instances fevers have been produced and nursed from this cause, even in our men-of-war. The men-of-war formerly used to be ballasted with shingles: on turning this ballast, it has produced fever in several of the ships; that I have seen myself; but it is well recorded.

What kind of fever?—The usual fever of the station they happened to be on, or the place they were at: not the plague. It is a well-known fact, established among maritime people, that the health of our fleets, our troops in transports, and seamen in merchant-vessels, have all risen in proportion to the degree of perfection at which we have arrived in cleanliness and ventilation.

Have you, in your enquiries, ever heard of a plague case having arrived at, or been seen, in any lazaretto in Great Britain?—Never.

Do you not suppose, that if the infection of plague had been imported in any bales of goods from the Mediterranean, which were opened for the purpose of being ventilated at lazarettos, that if the plague had existed therein, and was contagious, those expurgators would become themselves infected with the plague?—Certainly.

Would the circumstance of plague not having been seen for so many years in Great Britain, or in lazarettos, give sufficient confidence for concluding that the plague cannot exist in a British atmosphere?—I should think it would not give sufficient confidence; it would create great alarm, if known to exist.

You still suppose it might exist?—I think that is a doubtful point.

Is not plague in plague-countries periodical in its beginning and end?—Yes.

What is the state of atmosphere that you conceive compatible and not compatible with the existence of plague?—I look upon it, that in cold dry weather the plague does not so frequently exist. In hot weather, after floods, when the rivers, such as the Nile, have overflowed, and left marshes and ponds, the action of the sun in summer on such marshes and moist ground always produces disease, and frequently in the Levant plague.

What sort of temperature do you consider necessary to the existence of plague?—In the cases of plague I saw at Constantinople, the thermometer stood about the freezing point, from 26 to 30: it was in the winter.

Was the plague prevalent at that state of temperature?—No, it was not; but there were always some cases there.

There were cases even during that state of the year?—There are always some cases at Constantinople.

Was you at Constantinople, or any other part of the Levant, at any period of the year when the plague was raging violently?—No.

Do you happen to know what the state of the atmosphere is, in which it is proposed to act most violently?—I believe a high temperature, from 66 to 76 and upwards.

Is there any reason, when there is a hot moist air in this country, and the temperature rises to that point, that the plague should not exist in this country as well as in any other?—I do not think the summer-heat ever rises so high in England as in those latitudes.

As it appears that it existed at Constantinople when the temperature was nearly at the freezing-point, is there any reason why it should not exist in the same temperature in any other country?—In most other countries, and particularly in England, the houses are better ventilated, more cleanly, and well drained.

Then you mean to state, that the probability of its existing in England depends on ventilation, and the cleanliness of the people of the country?—Not entirely, but in a great measure.

Do you consider the plague in its character as infectious or contagious?—I consider it equally infectious through the medium of the

atmosphere of a sick chamber as from simple contact, having experienced it so far as having felt the arm of a patient under plague.

As you appear to consider the plague as connected with the state of the atmosphere, when the atmosphere is in that state that it admits of the existence of plague, do you then consider it an infectious disorder: I mean, when the air is in that state as to produce a liability to infection by the touch?—I think it is impossible to come in contact with a plague-patient, without inhaling the atmosphere of his chamber; and that we are more susceptible of infection by the membranes of respiration than those of the fingers.

Do you confine it to the sick chamber, or to the general atmosphere?—The surrounding atmosphere of the chamber: the atmosphere of a sick chamber is more infectious than any other.

From any experience that you have had of the plague, have you seen instances of its being communicated any other way than by the touch?—Not in any way; I have never seen an instance of its being communicated.

You have stated, that you were present when there were cases of plague?—When there were plague-patients at Constantinople, I made interest to view them with the Arabian physicians.

State the circumstances of your visit?—I saw him approach the patient, and feel the pulse, without the least fear; and, upon the second view, but with some apprehension of contagion, I was induced to do the same.

In what stage of the disease?—One patient had been ill a week, and was recovering; the others had been ill two or three days; I understood they died: the one of the week's illness I understood to have recovered.

Were buboes and carbuncles on these?—One in the axilla.

Were these all the cases that came under your observation?—These were all the cases that came under my eye that were decidedly plague: I saw a great variety of Asiatic fevers.

Do you attribute its not being communicable by contact, to the state of the air?—I consider that the plague is not more liable to be communicated by contact, than by inhaling the atmosphere in a sick room.

Then you believe it is equally communicable by both, either by contact or inhaling?—It is impossible to approach to contact without inhaling the atmosphere of a sick chamber.

What length of time was you at Constantinople?—I was there three or four months.

Do you consider these points of metaphysical temperature of air so useful and decisive in the investigation of disease, as the plain fact whether the disease has ever existed or not for many years?—No, I do not.

Then you consider the plain fact where it has not existed, the best criterion?—The best criterion; but I do not consider we have arrived at sufficient knowledge with respect to air, so as to ascertain these facts perfectly.

As you touched the plague-patients and felt their pulses, and did

not receive the disorder, would not that rather convey the idea that the plague was not contagious?—That would depend upon the state of the atmosphere, and on the state and constitution of the individual who had touched the patient; and whether there was a predisposition to receive the disease or resist it.

Did you make the subject of the plague your study while you was in these places?—From being frequently under the restraint of quarantine, I thought a good deal upon the subject, and read a good deal.

Did you pursue that by inquiries in the country?—By inquiries at the places we touched at. I made a point of visiting the lazaretto when I was on quarantine, and permitted to do so: where there was no lazaretto, I made a point of getting information from medical practitioners.

Were these practitioners Greeks or Turks?—In Constantinople there were Greeks, Armenians, and Arabians; some few Italians at Pera.

Were they people who entertained the same opinion on the subject of predestination as the Turks?—Most had not; some had; that is an opinion they do not avow.

I would ask whether they stated to you that they considered there was any danger in the use of the clothes of the persons who had died of the plague?—Yes; certainly.

Then you infer, that the plague may be conveyed by clothes, or other things than the touch?—Certainly.

Have you any means of knowing what length of time packages may be conveyed, supposing the air not to operate upon them, or any clothes or materials used by persons having the plague, before they lose the power of communicating the infection: state, either from your own experience or information?—That entirely depends on the ventilation of the packages.

If the packages are not subject to ventilation, you are of opinion that the plague may be conveyed?—It depends on the state of the atmosphere. What I conceive as to cotton is, that it depends on the state of the cotton when it is brought on-board. The Turks are a people very superstitious: when a person dies of the plague, they put the corpse into a shell, and sometimes the buboes discharge matter on carrying a corpse to the grave; this matter comes in contact with the dust: or, if that dust should afterwards be mixed with cotton, and that cotton comes to England and meets with a diseased atmosphere, I cannot answer for the long conveyance, without ventilation, destroying the infection.

You have no reason to conclude it could not?—I have no reason to conclude it could not. I think a lazaretto, properly fitted up with ventilating apparatus, so as to cause a current of air to be constantly percolating through the cargoes, the vitality of any contagion that might be conveyed to England must be soon destroyed by that means.

Are not animal substances, such as goats' skins, particularly susceptible of infection?—They are; goat-skins and hare-skins, and Turkey carpets.

Do you think that the bales of goods which are closely packed and come from Constantinople, admit of such a ventilation, without being completely opened, as to give security?—No; it depends much on the state of ventilation at the lazarettos. I am of opinion, that the airing process might be as efficiently performed as it now is, in a much shorter period, by a different fitment, attending to the state of the ballast and hold, which in every ship is important, but in lazarettos most particularly so.

You stated, that you thought that one of the things that contributed to the plague was the stagnant water?—It always contributes to disease.

Do you know that the plague is frequently prevalent in Egypt?—Very well.

Do you happen to know at what period the Nile rises and falls?—I believe it begins to fall about August. I have not a perfect recollection of the history of the Nile.

Do you not know that the Nile rises during the summer in hot weather, and subsides during the winter?—It subsides during the autumn, I think.

Is plague prevalent in Egypt during the winter and autumn?—It is more prevalent in summer than in winter.

That is, it is prevalent in Egypt at a time when stagnant water is not found?—Yes, at all times.

After what you have stated, I should like to have your opinion, as a physician, whether you would take on yourself to advise, on your own responsibility, the relaxation, or the material relaxation, of any of the precautions in this country to prevent the communication of the plague?—That is a serious question. Though I have not a doubt but what the quarantine might be with safety considerably diminished, under certain regulations of ventilation and fitment, in lazarettos; yet it is a thing of so much importance, that it ought to be proceeded in with the greatest caution.

Do you think any distinction might be taken between persons and goods in quarantine?—Yes, I do.

In what period do you think persons would be safe from Smyrna?—As much quarantine in general as the men-of-war perform; three days. At Malta, coming from the Morea, I remember bringing a passenger who was very anxious to go on-shore, and they let him out in three days.

Did you ever hear of inoculation for the plague?—Yes, I have, by Dr. White, whom I knew well: the patient died under the disease. I am not sure it was the plague, or whether the disease he died of was from inoculation.

Do you make a distinction between ships of war and merchant ships?—Certainly.

Do you consider it more likely that the infection should be received in the internal part of a bale, or the outside?—That depends on the state of the bale of cotton; the state it was embarked in, whether moist or perfectly dry.

Is it not likely to exist in the internal part of a bale?—Certainly, infection is.

And therefore, if it does so exist, according to your doctrine, would it not communicate plague, unless properly purified and ventilated?—That I consider very doubtful; but I should be very desirous to avoid the risk, by having it ventilated.

Do you believe that the plague can be imported into Great Britain?—I should think it possible, certainly.

Do you think it likely?—No; and I think the vitality of the disease might be soon destroyed, if it was.

And therefore you consider it could not long exist in England?—I think it could not long exist in England.

If the air was in a state which admitted of its existing at all, and it was once communicated, what reason is there to believe it would not be generally communicated: why would it not spread?—The disease would be weakened, in the first instance, in the lazarettos, by ventilation and fumigation.

I am assuming a case where there is no lazaretto?—I see no reason why it should not spread, except that the English people are more cleanly, better ventilated in their apartments; and the common sewers and drains carry off all filth, which is a great cause of the spreading of the plague in other countries.

If it got among the lower orders of people, who are extremely crowded together in large towns, might it not then?—Certainly; but they would be immediately separated.

Do you think the plague more infectious than the small-pox, or measles, or typhus?—Yes, perhaps more infectious than any other disease: I have never seen any infection from it; I have from all the other diseases named. In the hospital I have now charge of, I had, four years ago, about eighty-two cases of measles of a mild character. I was desirous of two girls being infected; I placed them in the wards; neither of them took it. I changed their linen; they both took it, and passed the disease mildly.

Did you ever know cases of typhus in which there were buboes as in the plague?—In high inflammatory constitutions, I have seen an enlargement of the groin, probably a sympathetic enlargement, in the first stage of the disease.

Do you consider the buboes a distinctive character of the fever called the plague?—I should think so.

Is the typhous fever, such as we have it in England, a common disorder in those countries where plague prevails?—It is not so prevalent, but it is more fatal; the bilious remittent fever is more common.

The typhus exists distinctly from the plague, though not so common?—Distinctly, though not common.

Do you think that different regions, according to their different circumstances of climate, soil, and construction of cities, produce among the people, at certain seasons of the year, fevers of a particular character?—Yes, certainly.

Do you think that one case of such fever being contagious, and the

contagion transported to another country, where the circumstances are different, they would establish themselves in the other country?—No; only partially, if at all.

Is that your general opinion as to the probability of the plague being transported from the Levant, and communicated to other countries?—Yes; although, if it was imported into England during a diseased condition of the atmosphere, it might do great mischief before the vitality of the disease was overcome.

But you think it could not long survive its importation?—Not long.

And so you think of all other fevers?—All others that are not endemic of the country.

All other fevers characterized by particular circumstances?—Yes.

Do you suppose the atmosphere of England has been applicable to the receiving or generation of plague for the last one hundred years?—No; this country, and every part of the world inhabited, has been more cultivated, underwood near cities has been cleared away, and swamps drained, which has contributed much to rendering the disease milder.

Does not plague occur near the Mediterranean, where quarantine-laws are rigid?—Yes.

Do you suppose the plague to be introduced into these places, or generated in the places themselves?—I have never been able to ascertain as a fact the introduction of plague into any place: I have heard it attributed to men and to goods, but never of my own knowledge.

Have you ever heard that the plague which prevails so generally upon the coast of the Levant, has been spread eastward over the continent of Asia?—I believe it has made some progress, but not always eastward.

I should be glad to know your opinion, founded on your general knowledge, why the plague should not proceed eastward through the continent of Asia by land, as probably it is communicated westward, by persons and goods transported on-board a ship?—I consider that as entirely depending upon a diseased state of the atmosphere in these places; and, as to its being communicated westward by persons and goods, I never knew it so communicated.

You must have heard that the plague is frequent in Aleppo, and that the caravans proceed regularly with goods in bales from Aleppo eastward through the continent of Asia: have you ever heard of the plague being communicated by these caravans to the eastern country?—Never.

Why should not the plague be carried in bales of goods transported to Asia eastward, as well as brought by goods or persons on-board ship westward?—I see no reason why it should not.

Does your knowledge of the practice of countries eastward enable you to say whether the transportation of goods by caravans is calculated in any way to prevent the communication of the plague?—No; but I have seen the unloading of a caravan; the goods are not so closely packed in caravans as in Levant ships. You are aware of the mode they adopt in ships: the cargoes are screwed down; they often

raise the beams of a ship in forcing the goods down; and consequently they are more liable, from their close stowage, to retain infection, if infection is embarked.

But the plague might as well be acquired by a caravan as brought by a ship?—Certainly.

You say you was present at the unpacking of the caravans?—Yes.

Where?—At Constantinople.

From whence did they come?—I have seen one, I believe from Aleppo. I was only present at the unpacking of one, and I am not aware where it came from: they pick up goods, the same as our wagons, every where on the road, I was told.

Do you know that they come through infected countries?—I am not aware of that.

Are you not surprised that the expurgators of goods in lazarettos in Great Britain have never received the plague?—I should conclude, from its not having been imported.

Then you suppose it has not been imported since the establishment of lazarettos?—Yes, I suppose so.

Do you think the epidemic disease under certain circumstances may become contagious?—Yes, it certainly may become infectious.

Do you suppose that the plague has been imported prior to the establishment of lazarettos in Great Britain?—I never heard that it has been ascertained as a fact.

If it had been imported, would it naturally have occasioned the plague in the community?—Not naturally: it might have been destroyed by the climate, or by management.

How long do you consider the infection of the plague may be latent in bale goods?—I cannot exactly state what length of time it may remain latent in bale goods.

Do you know that there is no ascertained account of the introduction of the plague by importation, previous to the quarantine-laws?—Not any that I am acquainted with.

Do you think that the concurrent testimony of all the old historians was merely vague report?—I am not competent to decide; I cannot speak as to that question.

Would you not rather be governed by modern facts than historical reports?—Certainly I would.

Do you consider the typhous fever a species of the plague?—Not exactly, but partaking of a highly malignant fever.

Does not Cullen call the plague a high state of typhous fever?—Yes, a high state of contagious typhus; but he never saw it.

CRITICAL ANALYSIS

OF

RECENT PUBLICATIONS, IN THE DIFFERENT BRANCHES
OF MEDICINE AND SURGERY.

"I would have men know, that, though I reprehend the easie passing over of the causes of things by ascribing them to secret and hidden vertues and properties; (for this hath arrested and held asleepe all true enquiry and indications;) yet I doe not understand but that, in the practical part of knowledge, much will be left to experience and probation, whereunto indication cannot so fully reach: and this not only in *specie*, but in *individuo*. Yet it was well said, *Vere scire, esse per causas scire*,"—BACON.

An Historical and Practical Treatise on the Internal Use of Hydro-Cyanic (Prussic) Acid, in Pulmonary Consumption and other Diseases of the Chest, as well as in several Complaints attended by great Nervous Irritation or acute Pain: with full Directions for the Preparation and Administration of that Medicine; and a preliminary descriptive Account of the principal Diseases in which it has been employed, illustrated by numerous Cases. By A. B. GRANVILLE, M.D. F.R.S. F.L.S. M.R.I. Physician in ordinary to his Royal Highness the Duke of Clarence; Member of the Royal College of Physicians in London; principal Physician to the Royal Infirmary for the Diseases of Children; Physician-Accoucheur to the Westminster General Dispensary, &c. 12mo. pp. 417. Second Edition, greatly enlarged. Longman and Co. 1820.

THE present edition, it is said in the Preface, "may be considered as a new work, and the fourth attempt which the author has made, in the course of five years, to establish the claims of a new and powerful remedy to the attention of the medical profession in this country. To the original facts and observations respecting this important subject contained in the first edition, consisting of less than one hundred pages, the author has been enabled to add, since the period of its publication and rapid circulation in 1819, a vast mass of information collected from various sources, rendering it incumbent upon him to new-model the work, and extend it to its present size. He trusts that, in so doing, he shall not be taxed with having unnecessarily swelled his *pamphlet* into a *volume*; and that, when reference shall have been made to the additional matter of practical utility contained in the present edition, the scope he had in view in publishing it will not be mistaken."

The success of the author's exertions will be best shown by the relation of the fact, that about eight or nine quarts of prussic acid for medicinal purposes have been sold at Apothecaries' Hall since April 1819; and Mr. Garden, the chemist, says that he has disposed of forty pints since the same

period. This quantity, for a medicine which is administered, at most, in the dose of from sixteen to twenty-four drops in the twenty-four hours, must be regarded as very considerable. There are circumstances, which it is unnecessary for us to enumerate, that render it becoming in us, on this occasion, to withhold the expression of the eulogies which reviewers, for their own self-complacency and satisfaction, are accustomed to indulge in, in their critical exordia, when they consider them to be merited; but, if any book ever had the less justice done to its deserts from the want of such panegyrics, this treatise will not, if we are successful in our attempt to give the reader of this article any thing like an adequate idea of its contents: for, the number of interesting facts which it presents, the assiduity with which they have been collected, and the perspicuous and methodic manner in which they are related, need only be contemplated in order that they may obtain the tribute which is due to the author, and an acknowledgment of the claims of the work to the attention of medical practitioners. We proceed immediately to our analysis, without any further prelude, as any remarks which we may have to make respecting the value of the remedy which it is the object of this treatise to place in its proper point of view, will be most properly brought forward when we come to consider its particular application.

After some preliminary reflections, comprising an account of the scope and objects of the present work, the author treats of the chemical history of prussic acid: this, being a matter of interesting curiosity, rather than of practical importance to strictly medical readers, we may pass over without adducing from it any more than an account of the constituents of this acid, as they have been determined by Mr. GAY-LUSSAC. According to this chemist, it is formed of carbon and azote, acidified by hydrogen. To the peculiar gaseous compound of carbon and azote, the base of the acid, he has given the name of *cyanogen*, (from *kyanos*, blue, and *gennao*, to produce,) which he has found to be composed of 46.19 parts of carbon, and 53.81 of azote, in 100; and the *hydro-cyanic acid*, of those same proportions of carbon and azote, with the addition of 3.90 of hydrogen: so that the acid is composed of equal volumes of cyanogen and hydrogen. This combination is, however, gaseous; the liquid commonly known by the name of prussic acid, is a solution of this gas in *water*. Mr. H. C. JENNINGS (the inventor of the mariner's compass which is safe from magnetic influence of the ship in which it is placed,) seems to be the only chemist who has obtained pure liquid prussic acid, by means of immense pressure, as we stated in a former Number of this Journal, (November 1818.)

The second section of this work treats of the modes of preparing the hydro-cyanic acid for medicinal purposes. Those which have been hitherto recommended, are the processes of SCHEELE, as improved by LA PLANCHE; and of VAUQUELIN: each of which, Dr. Granville says, is perfectly good for medicinal purposes. That of Vauquelin appears to be most readily practicable by persons not well versed in chemical operations: it is as follows.

"Into a solution consisting of two ounces of cyanuret of mercury and sixteen ounces of water, pass as much sulphuretted hydrogen-gas as will serve to decompose the salt, leaving an excess of the gas. Filter the liquor to separate the sulphuret of mercury formed, and treat the filtered liquor with an excess of subcarbonate of lead. Shake the bottle until the excess of sulphuretted hydrogen be absorbed. Filter once more, and the remaining liquor will be diluted hydro-cyanic acid, of a proper strength for medicinal purposes."

The following is the formula which is employed at Apothecaries' Hall, and which was supplied by Mr. BRANDE.

"Prussiate of mercury (cyanuret), ℥j; muriatic (hydrochloric) acid, ℥j; water, ℥v. Draw off four pints, and rectify through chalk."

We regret that we cannot supply our readers with a point of information that they must here desire,—that is to say, an account of the precise proportions, in regard to strength, of the two preparations just described. It is the former to which Dr. GRANVILLE alludes when speaking of the doses, &c. in the course of this work; but no great inconvenience will be experienced by those who may choose to employ the preparation from Apothecaries' Hall, as the latter does not vary considerably from being double the strength of that obtained by the former, or Vauquelin's, process.

Some observations ensue on the presence of prussic acid in animal substances, when a few facts are noticed which have been thought to show that it has been found in the human body, under certain conditions, during life, though it does not naturally exist in animal substances in its proper form, but only in its elements. In several vegetable matters it exists naturally; as in bitter almonds, and various kernels having a similar taste to them; peach-flowers; peach and nectarine leaves; the cherry-laurel, (*lauro-cerasus*); the bark of the *prunus padus*; *uva ursi*, &c.: the whole of which possess the odour, as well as the taste, which is so strongly manifested by prussic acid.

The fifth section treats of the physiological experiments made with hydro-cyanic acid. Of the highly deleterious effects of this substance on animal bodies, when administered in certain

quantities, every medical reader must be well-informed; and it cannot be unknown to them that fatal consequences have not unfrequently ensued from the use of laurel-water, laurel-leaves in custards, &c. cherry-brandy, noyau, ratifie flavoured with kernels, and even from bitter almonds when eaten in considerable quantities. The colleges of London and Edinburgh have even deemed it prudent to reject the old black-cherry stone water of the shops, (formerly so favourite a remedy with the people in convulsive affections in children, and especially in those attending the eruption of small-pox,) from their Pharmacopœias, because of the danger attendant on the popular use of a medicine containing prussic acid. It was the striking similarity of certain qualities of taste and odour possessed in common by all those substances with the same qualities in the prussic acid, that led, by analogy, to the use of the latter as a medicine. Cherry-water and laurel-water had been for some years commonly used as remedies, the latter especially in Italy, when the prussic acid was first administered. Chemistry has since shown that the vegetable substances above enumerated contain prussic acid.

Several physiologists have endeavoured to ascertain the precise way in which the system is affected by prussic acid: that is to say, whether that substance acts by producing excitement or asthenia, what part of the body is especially and primarily affected by it; and how death is produced, when it acts as a poison: but none of these questions has been satisfactorily explained. Some have supposed, in the first place, that it acts as a direct sedative; but Messrs. DUVIGNAN and PARENT say, "we feel convinced that the prussic acid acts immediately on the nervous system, of which it excites the action in a manner very prompt, but transient;"* which seems more probable than the former opinion, for it is hardly possible to conceive that there is in nature a *direct* and *positive* sedative to an animal body. The notion of MANZONI and FANZAGO respecting its agency, is not unworthy of attention: they think that it acts as a stimulant to the part to which it is applied, whilst its influence on the rest of the system is sedative. We have pretty good evidence that a blister often acts in this way, by concentrating vital action in one particular spot; and it is not unreasonable to suppose that other stimulants may act in a similar manner. With respect to the parts of the body especially acted on by prussic acid, the opinion of MAGENDIE seems to be most probable: he thinks that it lessens, suspends, or annihilates, (according to the quantities in which it is administered,) the functions of *animal life* (of BICHAT), without acting directly

* American Medical Recorder, vol. ii. p. 464.

on those of nutritive or organic life. The latter become extinguished when the acid is administered in a large dose; but this effect equally ensues from certain degrees of injury to the organs of animal life, in whatever way inflicted, and when it is clearly evident that the deleterious agent has acted *immediately* on those organs alone. But, though we may, as physiologists, feel very desirous to have these questions decided, we may be pretty well satisfied, as medical practitioners, with the knowledge of the facts comprised in the following observations of the author.

“The prussic acid, when administered to a patient labouring under a disease of vascular or other excitement, appears to exert an immediate influence on the nervous system; it gradually diminishes irritability, checks a too-rapid circulation, and calms many of the symptoms of fever. If a dry cough be present, it promotes expectoration in the first instance, and subsequently stops the cough itself. The spirits, before exalted, soon feel the quieting impression of the acid; they become subdued; the speech, the countenance, even the expression of the eyes, assume a character of unusual meekness: there is a relief from pain and actual suffering; sleep comes on undisturbed, respiration is soft, and the pulse more tranquil than at any other period of the complaint, having lost the throbbing heat of irritation. In some few cases, these sedative effects are so much more considerable, that the patient expresses himself as if only ‘*half alive*.’ On those occasions, there is an *apparent* entire prostration of strength, great lowness of spirits, and unwillingness to move, speak, or take food; (relative) life seems (almost) suspended, yet the head and mind remain clear and intelligent; there is a total absence of pain; neither does the patient complain of any symptom of local or general irritation; the heat of the skin is natural, and the pulse, in the midst of this dead suspense, continues its course steadily and quietly. This state of things lasts from twelve to twenty-four hours, when it ceases, and every organ is gradually restored to its former integrity of function. It should, however, be borne in mind, that such instances of great depression are extremely rare; and indeed seldom occur except where the dose has been too large, or the acid has been injudiciously administered; or from some peculiarity of constitution.”

We pass over, for the present, some general observations on the efficacy of this medicine in relieving cough from various causes, hectic fever, asthma, chronic inflammation in various organs, pneumonia, pleurisy, menorrhagia and dysmenorrhœa, hæmoptysis, and some nervous diseases,—to the section on the poisonous effects of the prussic acid, and the means of opposing them. We shall hereafter have occasion to notice the most important observations relating to the subjects previously enumerated.

Respecting the poisonous effects of the prussic acid, we shall only notice some observations made by Mr. COULLON regarding

himself, and by Prof. HUFELAND, for the purpose of showing the different effects of different doses: though we suppose almost every medical practitioner must be familiar with the history of observations of this kind.

Mr. Coullon says, that, on taking an increased quantity of diluted prussic acid, which in the dose of twenty or thirty drops had not produced any considerable effect, he found the liquid to possess an extremely bitter taste; an increased secretion of saliva took place, and nausea in a slight degree occurred two or three times. The pulse became more frequent than ordinary, but returned to its natural standard in less than two hours. He experienced, for a few minutes, a sense of weight, and a slight pain, in the head about the forehead. Oppression about the chest, alternating with a slight pulsating pain, was also felt for about six hours. Prof. Hufeland relates the case of a robust and healthy man, aged 36 years, who, when about to be seized as a thief by the police officers, took a small phial containing a strong solution of prussic acid in alcohol from his pocket, the greater part of which he swallowed. He staggered a few steps, then sunk on his knees, and expired without appearing to suffer pain. Not the slightest trace of respiration or pulse could be discerned. A few minutes afterwards, a single and violent inspiration took place, which was repeated in about two minutes. The extremities were cold, the breast and abdomen still warm, the eye-lids were half open, and the eyes prominent and glistening: the face was not distorted, but rather like that of a person asleep. The corpse exhaled a strong smell of bitter almonds.

Dr. Granville says, that his own examinations of animals poisoned by prussic acid developed no appearances of organic lesion after death; and he says also that the muscles are not excitable by galvanism, when the dose has been very considerable. ORFILA seems to have remarked nothing apparently attributable to the acid in such circumstances, but congestion of blood in the veins and vacuity of the arteries, which is observed in cases of sudden death from several and diverse causes. He however says, that the muscles are excitable by galvanism for some time after apparent death. The blood seems generally to have remained fluid. None of the appearances in the bodies of animals poisoned by prussic acid, are peculiar to this state. The odour they exhale like that of bitter almonds, will however indicate the probability of its having been the cause of death. The most remarkable circumstance in the examination of bodies poisoned by prussic acid, when in a state not much diluted, is that it cannot be detected in the stomach, although the animal had died almost instantly after having swallowed it. It appears to be absorbed with great rapidity. We recollect having

remarked that every part of the body of a dog manifested the odour of the acid for some days after its death, though it had been killed by the application of the poison (in a very concentrated state) to the nose.

The best antidotes to the deleterious effects of prussic acid, are diffusible stimulants. For the reasons above mentioned, any attempts to obviate them by chemical decomposition of the poison in the stomach must prove unavailing. Dr. Granville says, "Hot brandy and water, with, perhaps, some liquid ammonia,—or the latter in combination with camphorated spirit, properly, though sparingly, diluted,—or oil of turpentine, are, of all the means employed, by far the best and most effective." CHAUSSIER recommends a coffee-spoonful of oil of turpentine in a cup of strong coffee every half hour, for three or four times.

The ninth section treats, in a particular manner, of the history of the use of prussic acid as a medicine. As this is a subject of curiosity rather than practical utility, we shall immediately pass on to the second part of the work, which treats of the cases in which the prussic acid has been administered as a medicine. The first in order are cases of "consumption." On this subject the author says, in some preliminary general remarks, "Although it may yet appear, and I confess to me it is, problematical, whether a case of consumption, far advanced in the last stage of its melancholy career, has yet been cured in a decided manner by the prussic acid. There is no doubt that in hundreds of other cases, where the disease had not yet committed great ravages on the lungs and the system generally, that medicine has proved the means of arresting the progress of the complaint, and averting the impending fate of the patient." So far the author seems to be borne out by the evidence he adduces in this work; and it is such as will, probably, lead us in our own practice to supersede, in many instances, digitalis, tartar emetic, and ipecacuanha, by the prussic acid: but, that this or any other medicine will prove the means of cure of *tubercular* phthisis, is a thing which does not come within the circle which we regard as the boundary of probabilities.

There seems to be more of the humoral pathology comprised in Dr. Granville's opinions than is generally admitted at the present day. On speaking of the several species of consumption, (in the manner of a slight sketch only, as a description of diseases, beyond the principal traits which characterize them, does not make one of his objects,) he states one of them to be "owing to a vitiated state of the animal fluids, no matter from what cause, whether scrofulous, scorbutic, syphilitic, or otherwise." This species, he says, "includes what has been called the tubercular and strumous consumption." Alluding to the

incipient stage of this disease, Dr. Granville says, "If the prussic acid be given during this stage, even where a constitutional tendency to this dreadful malady exists, and before the suppurating process of the tubercles be formed, a perfect recovery may be expected. This is well illustrated by the following cases." The cases alluded to are ten in number. As our scepticism respecting such powers in this remedy, and the improper prejudice which is, perhaps, founded on that scepticism, may prevent our seeing in the cases referred to such evidence as the author considers them to present, we deem it prudent to transcribe some of the cases in question, rather than to attempt to give an abstract of them, in which we might, perhaps, omit the signs which are really characteristic of *tubercular* phthisis. Our limits will not permit us to transcribe the whole series; we shall, therefore, select those which occurred to Dr. Granville's own observation. We shall, by choosing these, be most likely to do justice to his statements; and we cannot help, besides, observing, that most of the others are remarkable for the extremely imperfect and unsatisfactory manner in which they are detailed.

"CASE IV.—Master Blackwell, aged ten years, was sent to sea as a midshipman, in hopes, chiefly, of his overcoming a certain predisposition, as it was then supposed, to strumous consumption. For some time after the constitution seemed to improve; but about three years ago, having spent a very severe winter on the Newfoundland station, the symptoms of pectoral disease became so pressing, that, on the recommendation of the medical officers, and indeed of Captain Buchan himself, his commanding officer, he was sent home. On his arrival in England, I was requested by the late much-lamented Sir W. Farquhar, then ill, to visit him; and my report to that eminent physician was altogether unfavourable. Every mean usually employed in such cases was resorted to, in the first instance, to no purpose. The boy was wasting daily; the cough and night-sweats had manifested themselves in a decided manner. There was no expectoration approaching to purulency; nor was the abdomen enlarged or distended. The bowels yielded to proper medicines, and the secretions appeared healthy. The pulse 125 or 130; the skin dry; the respiration difficult; the cheeks flushed. The prussic acid was administered with carbonate of potash. In about three days the disorder seemed arrested. He continued the same medicine another fortnight, and every symptom, except debility, disappeared. He is now in excellent health, and has been so for upwards of two years. The almost instantaneous good effect which the acid seemed to have, from the first, on the cough, induced the mother, who had been troubled with that symptom during the whole winter, to take, without my knowledge, the same mixture with the acid which I had prescribed for her son; and it was only after her cough had wholly subsided that she acquainted me of the step she had taken, though she knew not of the mixture being any thing else than a common cough-mixture.

“CASE V.—A servant of Mrs. E——’s, of Upper Seymour-street, was sent to me for advice, in the month of May, 1819, in consequence of a complaint in the chest, which had been, more or less, troublesome for several winters, but which had become more urgent at the time of his consulting me. He was then in the twenty-second year of his age, tall, slender, of a very fair and delicate complexion, with blue eyes, light hair, white pearly teeth, long pale fingers, &c. He complained of pain immediately under the sternum, a little to the right; coughed occasionally, without any expectoration; breathed with difficulty, and as if heavily oppressed in so doing. The pulse was frequent, small, irregular. There was a clammy moisture on the skin; yet the palms of his hands were burning with heat; his cheeks were marked with spherical flushes, which contrasted singularly with the bright-blue tint shining through a pearly-white skin, about and under the eyes. The tongue was of a purplish colour. The appetite good, eating usually three meals a-day; but the body wasted notwithstanding. The bowels rather relaxed; the abdomen soft, and of a natural size. Being questioned about any chilliness after meals, he answered in the affirmative. The patient had been occasionally attended by the family apothecary, and had been twice bled, upon which every symptom seemed to grow worse; until, at last, his mistress resolved to send him to me for further advice. Considering his case to be one of incipient strumous consumption, I prescribed the acid, which the patient himself used to procure at Mr. Garden’s in Oxford-street. During the first week the cough subsided, as well as the perspiration, but the pulse continued frequent and small. I then gave, alternately with the acid, a mixture with myrrh and carbonate of potash, which seemed to strengthen him; and, during the five weeks that the patient continued to call on me, I could plainly perceive the good effects produced by the medicine. At the end of this time, the cough had entirely left him; the pulse had been lowered to about 80; and he considered himself so much better that he discontinued his visits. I understand that he applied for some time longer to the chemist for the acid, and that, when he had last been there, he appeared in perfect health.

“CASE VII.—Miss C—— had from her childhood exhibited signs of scrofula, though born of healthy parents. The glands of the neck; occasionally, those of the groins; and, in one instance, those under the left arm had become swelled, painful, hard, but had never suppurated. On some of these occasions there was a slight cough present, which was attributed either to teething or to worms. Her appearance, however, denoted perfect health, and no uneasiness was felt respecting her. Towards the critical period of fifteen, after no inconsiderable sufferings, the glandular swellings disappeared almost suddenly; and in a few months afterwards menstruation became fully established. The young lady was now considered as perfectly healthy, and no farther attention was paid to her. As soon, however, as the formalities of etiquette required her presence in the gay world, and she was surrendered to the caprice of fashion, it was discovered that she could not well bear the fatigues of the night, and that neither the prolonged hour of rising on the following days, nor the measured

and choice diet, nor the drive in the open landau, were sufficient to stay the wasting effects which such a mode of life had visibly produced. A *slight* cough again made its appearance, accompanied with a *slight* pain in the chest, and a *slight* oppression in breathing. She was *slightly* affected when walking fast, or ascending the stairs; and she felt, though *slightly*, every variation of the atmosphere. The symptom which first alarmed the parents, was emaciation; and this seemed to proceed so fast, that Miss C——, from being an object of pride, became soon an object of pity and grief, to her friends. A physician was called in, who thought that the young lady suffered from diabetes, and ordered a treatment accordingly. A second was soon afterwards consulted, whose more particular study was supposed to be the unravelling of mysterious complaints, and particularly of the chest; for by this time a disease of that part was strongly suspected by all. His opinion confirmed the fears of the friends, but his treatment did not allay them. In the month of June 1819,—that is, eighteen months from the first time of taking the alarm, the patient was supposed to be in a consumptive state, and under these circumstances I was requested to give my opinion. Mine could not but be in unison with that of the medical gentleman who preceded me; and I strongly recommended the use of the prussic acid. Indeed, my opinion had been asked specifically as to whether that medicine was admissible in the case of Miss C——. To describe all the symptoms under which this young lady laboured at the time of her first beginning the use of the acid, would be to repeat almost every symptom which has been before mentioned in the outset of this section. Suffice it to say, that she was considered as being in the first stage of tubercular consumption. I visited the patient occasionally from June till August, and derived great satisfaction at the amendment which I thought I could perceive in her general health. My subsequent absence from England prevented my following the case through its various stages; but, on my return from the continent in November, I had the pleasure of learning, by letter, that my patient, then in the country, had, during the autumn, lost her cough and oppression on the chest; that she had gained a degree of *embonpoint*; and that, at the approach of the bleak season, she had not experienced, as she had done the preceding years, the ill effects of cold and wet weather. The acid had been discontinued for three months, and resumed in February of the current year; when, after a fortnight, it was altogether abandoned, the young lady being in all respects free from complaint. In this case the acid never produced either sickness or dizziness. It seemed to act gently on the bowels for a few days, and not longer; and was administered, both when it was first taken and when it was resumed, for the first week, in doses of one drop every two hours in a common camphor mixture. This mode of prescribing the acid I have found to be preferable to that of limiting the use of the medicine to three times a day; when, if the whole quantity be pushed, from necessity, to eighteen or twenty drops, the patient is forced to take six of them at a time; a dose which seldom fails to produce sickness, and seems to irritate rather than to act as a sedative.

"CASE VIII.—A lady above sixty years of age, the grandmother of several children, had been troubled for several years with a difficulty of breathing and cough, which, on reaching the period of menopause, became very troublesome, and excited some alarm. In the early part of her life she had enjoyed excellent health, but signs of an unfavourable predisposition to glandular obstructions and enlargements had manifested themselves at various periods, and have since remained stationary. Several glandular swellings were shown to me on the second visit, in February 1820, occupying various parts of the body, but particularly the neck, the bosom, and each side of the chest. Remedies of all descriptions had been resorted to without effect: some indeed, which had been immediately directed to the removal of the glandular swellings, proved pregnant with mischief, and were soon abandoned. With the cough there was dyspnoea, and a scanty thick expectoration. She complained of fever at times; and her pulse, on my first seeing her, was full, hard, and above a hundred strokes in a minute. The skin dry; the tongue also dry, roughish, and of a pale tint. Slept uneasily, and at interrupted intervals. Her bowels had for a long time been confined, so as to render the use of daily medicines necessary. There was no decided pain in any part of the chest; but much uneasiness, impossibility of making a deep inspiration, palpitations and flutterings, with a kind of spasmodic attack at times, which rendered her existence truly miserable. An opinion had long before been given, that this lady was labouring under some tubercular formation in the lungs,—an opinion which external appearances, and the symptoms already described, seemed greatly to uphold. Occasionally the cough, expectoration, fever, and morning perspiration, with a hard pulse, would gradually become so alarming as to induce the attendant practitioner to prescribe bleeding. This operation was about to be again performed when I saw the patient, and it was determined, in preference, to give the prussic acid a trial. From the notes of my first visit, I find that her pulse was hard and frequent; that she had had several attacks of chilliness and fever; that she slept ill, and expectorated a sort of grumous yellow and greenish matter, which, when thrown into cold water, went to the bottom; and, when acted upon by boiling water, did not shrink, so as to appear smaller in its volume.* I ordered the prussic acid in the dose of ten drops for the twenty-four hours, powders composed of tartarized antimony and supertartrate of potash, and some aperient pills.

"The relief she obtained from this treatment became evident in a very few days: the cough soon afterwards ceased altogether, and with it the expectoration. I first saw my patient on the 15th of March, and by the 30th I discontinued my visits. She is now in her former usual state of health, and acknowledges the great benefit she has derived from the medicine in question."

In the *confirmed* stage of consumption, "when the suppurative stage of the tubercles has fairly begun," says the author,

* I beg to suggest this among many other distinctions between pure mucus and decidedly formed pus.

“ the hopes of recovery, from the effects of the prussic acid, become every day more faint, until at last nothing but palliative effects, and those of short duration only, can be expected. Still there can be no doubt that, even in the advanced period of this complaint, alleviation of symptoms, improvement, and even recovery, can, in some few instances, be looked for; since cases have occurred where the life of a patient, under the most unfavourable circumstances, has been redeemed by the action of the medicine in question; and in some other cases by other medicines also, as Dr. LAENNEC has proved in his recent and excellent work on Pectoral Diseases.”

How far the cases noticed by Dr. Laennec were benefited by any medicines that might have been employed, is a matter of doubt; but, that much alleviation has ensued from the use of prussic acid in the latter stage of tubercular phthisis, is abundantly shown by the cases adduced by Dr. Granville; and this alleviation seems to have been much greater and more frequent than what has been derived from any other medicine. The first case of this series is, indeed, related by Dr. MAGENDIE as an instance of the *cure* of tubercular phthisis in its *second* stage: the degree of confidence in this must depend on the degree of credit that may be thought to be due to Dr. Magendie's *assertions* that it was such a disease; for he has not, it would appear, thought it necessary to satisfy the public that such was probably the case, by any historical evidence. We shall transcribe it as it is detailed by Dr. Granville.

“ A lady from Lyons, now residing in Paris, of a constitution eminently bilious, after having experienced several misfortunes, was, in 1814, attacked by all the symptoms which characterize phthisis in its first stage. Circumstances not allowing her to attend to her health, she neglected it, until the month of January 1815, when, the disease having made great progress, she consulted Dr. Magendie. He found her labouring under all the symptoms of the second stage of tuberculous consumption, with a cough returning incessantly, and a slow continued fever preying upon her and undermining her existence. The prussic acid was recommended, and taken in the dose of from six to ten drops in twenty-four hours, diluted. The medicine was continued for about two months. From the first day the cough diminished, the patient slept; and, without increasing the dose beyond ten drops in the twenty-four hours, all the symptoms of the disease disappeared; the breathing became natural, the cough, expectoration, and sweats, ceased. In short, the lady was perfectly cured, and has never since experienced any symptoms which indicate the least disposition to a relapse. Her lungs, only, have become very sensible to the influence of atmospheric variation.”

On alluding to the foregoing case in his pamphlet published in 1819, Dr. Magendie says that the subject of it continues to evince perfect health.

A case follows by Dr. Granville, in which it is satisfactorily evident that consumption in its latter stage, in a young man, "though not cured, was so far palliated as to render life tolerably comfortable." How long this state of amendment continued, is not mentioned. This case is succeeded by that of a young lady, who was considered by her medical attendants to have "phthisis pulmonalis;" and Dr. Granville says, "a description of all her symptoms was forwarded [to him] in confirmation of that opinion." Those stated to be present by Dr. Granville are, pain in the chest, cough, restlessness at night, and great prostration of strength. We must rely on the accuracy of his observation and judgment, that the rest of the evidence necessary to characterize phthisis pulmonalis was present. This patient rapidly recovered so far under the use of the prussic acid as to be able to leave this country for Malaga, (which place Dr. Granville strongly recommends as a residence for consumptive patients,) though a few weeks before she took the acid "she was threatened with imminent dissolution;" and she has recently returned to England, "to all appearance in excellent health."

We must not omit to notice the remark with which Dr. Granville concludes the history of this case, lest our former transcriptions should convey erroneous ideas of the views he entertains of the powers of the remedy under consideration. Dr. Granville says, "Where there is disorganization of the lungs, no cure can be expected; but life may be prolonged, and rendered comparatively comfortable:" and he elsewhere (pages 241 and 242,) adduces similar remarks; showing his disposition to confine his inferences to what is strictly warranted by the evidence of observed facts.

After two other cases, extraneous to the author's own observation, in which some alleviation appears to have resulted from the use of the prussic acid, Dr. Granville takes occasion to make some remarks on the occurrence of vomiting from the influence of this medicine. He says,

"When the prussic acid produces nausea, or even vomiting, (and it is the same with regard to dizziness, which it will induce in some few individuals,) those symptoms take place on the first or second day after the exhibition of the medicine, unless indeed the dose be greatly augmented; and in such cases I strongly recommend that it should be abandoned immediately, for there is no chance of its ever again agreeing with the patient. Of about three or four hundred cases of the exhibition of this medicine, to which my personal experience extends, five or six have occurred in which the acid, evidently from a particular idiosyncrasy, produced sickness at the stomach on the first day it was taken; and in no other was any nausea excited, when it had, to all appearance, agreed with the stomach for the first week. This peculiar

effect of the prussic acid on the stomach of some few individuals, on its first exhibition, is wholly independent of the quantity taken."

Two other cases, which occurred to the observation of the author, are next related; in one of which, especially, a degree of alleviation was produced by the prussic acid that we should not expect to witness from any other medicine hitherto employed. A case is then related by Mr. RUDLAND, of Dartmouth, which seems to us to have been chronic bronchitis, consequent on acute bronchitis, and not "phthisis;" but there was a family predisposition to consumption, and the symptoms in this patient (the mother of several children) were very alarming, and such as often precede real tubercular phthisis consequent on bronchial inflammation. This patient entirely recovered her health under the use of the prussic acid, after, as it is said, "the most approved therapeutical means were adopted and rigidly followed for many weeks, without any obvious benefit."

Several other analogous cases are related or referred to; and then the author treats of its use in another species of consumption, which is that "subsequent to catarrh, pneumonia, pleurisy, bronchitis, and hæmoptysis." The diagnostics of this, in contradistinction to those of *idiopathic** tubercular consumption are first discussed; and then several cases illustrative of the powers of the prussic acid in the species of consumption arising from the causes just enumerated, are detailed. This is the part of the work which we have perused with the greatest degree of satisfaction; and we feel convinced that, in its application to the cases of the kind here alluded to, the prussic acid will prove a highly valuable addition to the materia medica; and with such impressions we very earnestly recommend the cases here related by Dr. Granville to the perusal of our readers. Several instances are adduced of the recovery of perfect health from states which were considered as hopeless by practitioners of eminent talents and very extensive experience. Besides the result of his own experience, Dr. Granville here, and elsewhere, brings forward that of others; amongst which is a very interesting account of a case of "phthisis trachealis" by Mr. TODD THOMSON, in which the efficacy of prussic is shown in a very favourable point of view.

Dr. Granville considers, in a distinct manner, consumption "occurring during pregnancy, or immediately after parturition, also after long suckling, or at the period of ablation." He thinks, with some former authors, that "pregnant women

* We designate it thus to distinguish it from the tubercular consumption which seems to ensue from bronchitis as an effect of the latter affection, as we showed in our exposition of the doctrine of Dr. BROUSSAIS.

are sometimes attacked with phthisis from some cause originating in the peculiarity of this situation ;" but he has not thought it conformable with his object in this work, to bring forward his arguments for this opinion. The results from the use of the prussic acid in these cases are detailed ; but, as they are analogous to those derived from this medicine in the other forms of consumption, we need not stop to particularize them.

The last species of consumption enumerated by the author, is " from a single abscess or vomica, the effect of accidental inflammation of the pulmonic texture, or of the membranes, in consequence of local injury." He states that he has hitherto had no opportunities of trying the prussic acid in this variety of pulmonic affection. According to Dr. Laennec, abscesses of this kind are of but very rare occurrence, as we remarked in our review of his work on Diseases of the Thoracic Organs.

The second section of this part of the work treats of the efficacy of prussic acid in " pneumonia, pleurisy, and other inflammatory complaints."

That so powerful a sedative, and one so rapid and almost constant in producing its effects, as the prussic acid, should be very efficacious in those affections, might be readily preconceived : the author has not, therefore, thought it necessary to give many cases of the kind alluded to in detail. The indications for the application of the remedy must, also, be sufficiently obvious, and do not materially vary from those which have commonly led to the use of tartar-emetic, digitalis, and colchicum, on analogous occasions ; like which remedies, too, the prussic acid seems to be eminently qualified to obviate the practice of excessive blood-letting in many instances, when, without them, it might be thought necessary. Distilled laurel-water, as we have frequently shown in late Numbers of this Journal, is very commonly employed in Italy in consonance with those views. The foregoing remarks are equally well applicable to the subjects of several of the third section, which treats of the use of prussic acid in " catarrh" and " spasmodic cough : " the fourth, on its employment in " hectic—and sympathetic cough ; " and the fifth, in whooping-cough. For the details of the evidence on these points, we must refer the reader to the work : we shall only remark, that, as far as regards the two former series, the evidence is such as shows that the prussic acid may very well supersede, on many occasions, any one remedy previously employed. This seems to be especially the case in respect to severe catarrh, as it appears epidemically at certain periods. On speaking of whooping-cough, the author draws a picture of the conduct of some practitioners in public institutions, which we trust is not strictly applicable to any one. Many men may think that they have, and they undoubtedly have,

treated whooping-cough successfully by antiphlogistic means; and it is as unjust to attribute to "high-mettled fancy" their use and praises of such measures, as it would be to attribute to such qualities the author's praises of the prussic acid, which would itself never have been employed, if the remark that those who use it "must certainly have forgot their Cullen, or know more than that celebrated physician," were justifiable. The sentiments we have just transcribed cannot, however, have been intended to be seriously expressed; for they inculcate nothing less than that no man can hope to be better informed in pathology than Cullen was, or treat whooping-cough better than he did. Dr. Granville says,

"I will, however, state, once for all, that the whooping-cough, in itself, is *never* an inflammatory disease,—for no traces of inflammation have been found in the respiratory organs of those who have fallen victims to it; and that, when the complaint has been very violent, and has lasted a great length of time, and then only, tokens of inflammation have been found in the brain, as the result of strong and often-repeated spasms of the organs of respiration, producing a great determination of blood to the head.*"

Without information of the stages of the disease in which the dissections mentioned in the note were made, the evidence of the want of signs of inflammation of the respiratory organs is hardly worthy of attention. If it is to be argued that whooping-cough is not originally an inflammatory affection, because no signs of inflammation are to be found (supposing the anatomical examinations to have been made in an accurate and sufficient manner) after death, in certain stages of the disease; it might just as well be argued that a gall-stone, passing through the choledochus, has not been the cause of the pain and vomiting which the patient thus circumstanced has experienced, because the stone is not to be found in the duct after death. It would seem, too, that the accuracy of the observation of the persons alluded to in the note just transcribed is not very satisfactorily shown, when they say that "the wind-pipe was found constantly *healthy*, although lined with a frothy, *blackish*, and *adhesive* mucus." We never yet saw an instance where the body of a child who had died in the early stages of whooping-cough was

* "In the first volume of the *Memoire Scientifiche e Letterarie dell' Ateneo di Treviso*, published in 1817, there is a paper containing the result of extensive pathological inquiries, made by several physicians and surgeons of that town, into the nature and seat of various complaints. On the subject of whooping-cough, which had been epidemical in 1816 at Treviso, a table of twenty-three anatomical examinations of children after death is given; from which it appears, that in all of them there were more or less signs of turgidity of the blood-vessels of the head, or serous effusion; that no symptoms of disease occurred in the chest, except in a few individuals, who presented an incipient ptilogosis, or plethora, or serous effusion in that cavity. The wind-pipe was found constantly healthy, although lined with a frothy, blackish, and adhesive mucus."

examined, without signs of inflammation in the larynx or some part of the trachea being manifest; and many practitioners of our acquaintance give testimony of a similar kind. Whether the inflammation is essential to the disease, is at present, we think, a matter of doubt, not a point that can be decided by an absolute negative. It is possible that whooping-cough only destroys life when it produces some of the diseased states which other practitioners regard as essential to it, and that it is really, in itself, as Dr. Granville seems to think, "a spasmodic affection." That the prussic acid possesses great powers in curing it, appears to be well proved by the cases here brought forward, and we cannot neglect to congratulate the author on his successful and judicious application of the remedy to so severe and often fatal a disease. We shall transcribe the two series of cases first in order, (that we may not err by an improper choice,) for the purpose of exemplifying those remarks, and of showing, at the same time, the mode of administering the remedy.

"Two children of Major Fitzgerald fell ill with the whooping-cough, in June 1819, at some distance from town; and, having been early consulted respecting the best mode of treating them, I lost not a moment in suggesting the use of the prussic acid. I also requested to be informed of any change that might take place during its use, and of the apparent effect of that medicine on the children. On the 23d of the same month I received a letter, stating that, "on the eleventh day after taking the prussic acid, my little patients were almost recovered; that the youngest had ceased to cough the last three days; while the eldest coughed still, but a loose, easy, and common sort of cough; and that, although there was still a whoop accompanying it at night, it was but a feeble one." 'The disorder,' the letter continued, 'was dying away, as they eat heartily, and the blackness under the eyes was quite gone, as well as the fever, langour, restlessness, &c.' I recommended a change of air, and this advice was followed almost immediately. The cough soon afterwards left the eldest girl also; and they both returned to their home quite recovered. A relapse, however, occurred in the latter place; and the prussic acid was again had recourse to, and continued until no trace of the complaint remained behind.

"Four young children, three boys and a girl, of Wm. Hamilton, Esq. were attacked, almost simultaneously, with the whooping-cough, in the month of May last; the two youngest with fever and symptoms of pyrexia, to which vomiting soon after succeeded. After opening the bowels with suitable medicines, and giving some cooling powders to those who had fever, I proceeded to administer the prussic acid to all, at first in an almond emulsion, and next in a camphorated mixture. The effect of the medicine was not the same in all four. One of the patients ceased to whoop almost immediately. The two next continued to cough for some time longer; and a fourth, the youngest but one, seemed scarcely to feel the influence of the medicine,—for he continued, even after the complete recovery of his bro-

thers and sister, to cough, whoop, and vomit. His general health, too, appeared to suffer from the prolongation of the complaint. The girl was the first who got well; for at the end of ten days she was scarcely ever heard to cough, and then in the ordinary manner only of people affected with catarrh. She had no relapse. Three of her brothers got well next,—that is, within the third week of taking the acid; but a parotid and tonsillar swelling, with fever, occurred to one of them at the same time, which required the application of leeches, and the adoption of a brisk system of evacuation by the bowels. The prussic acid was continued notwithstanding; the patient taking a teaspoonful of the mixture, containing about one drop of the acid, every three hours. At the end of six weeks his recovery also was complete.”

A considerable extent of forcible evidence of the very successful agency of this remedy in the same disease, is also brought forward from other practitioners, by whom it has been communicated to the author.

Analogy, on the facts already noticed, led the author to employ the prussic acid in asthma; but hitherto, he says, he has had but few opportunities of applying it to this disease. When we consider the various causes from which asthma will arise, and how frequently it is a consequence of considerable organic lesion of some of the thoracic viscera, we cannot expect that this remedy will be very generally of much utility here; but it seems probable, as the author argues, that it may occasionally be beneficial, and three cases in which it has been so are detailed in this work.

Hæmorrhages—painful menstruation—and abortion, are other cases in which the prussic acid has been employed with well-marked, and in some cases, in all probability, with peculiar, advantages. We shall not dwell on the successful results here brought forward, lest we should lead some of our readers to suppose that we think this medicine may supersede free blood-letting, and revulsive measures, in the generality of cases of hæmorrhage. They will best appreciate the evidence after perusing it in detail. There is not, however, much reason to fear that the generality of practitioners will become too enthusiastic and exclusive in the use of a new remedy, however powerful: they will regard it as a valuable addition to those already employed, not as one that is to supersede them on all occasions.

“Nervous diseases” and “affections of the stomach,” are comprised together as the subjects of the eighth section. This commences with some good remarks on the affections ordinarily termed nervous; a great proportion of which, the author says, arise from some disorder of the stomach. But, he adds,

“Every complaint which we cannot well class with other well-known affections, we often attribute to the nerves; and, in styling a disease nervous, we are often using a mere name to denote, at one and

the same time, a vast number of phenomena, definite in themselves,—such as tremor, fainting, palpitations, throbbing, flying pains, flatulency, indigestion, uneasy sensations, spasms, constipation, sudden startings, sighing, laughing, a lump in the throat, and the whole long train of ailments which are known to affect, in a more particular manner, the fine lady,—the irritable,—persons leading a sedentary life, especially authors, projectors, ambitious schemers, disappointed politicians, with many more restless and discontented beings.”

Several cases are related of great disturbance of the nervous system from various apparent causes, which were obviously much benefited by the prussic acid; and, in regard to its efficacy in dyspepsia and some other affections of the stomach, we may add to the evidence brought forward by the author, that Dr. Broussais has satisfactorily traced them, as well as hypochondriasis, in a considerable proportion of cases, to chronic inflammation of the stomach.* We select the following case, which is related by Mr. Todd Thomson, as a good exemplification of the utility of the prussic acid in a severe form of the affection just designated.

“T. R.—, esq. of a slender form and gouty diathesis, had long been afflicted with dyspepsia, attended with a peculiar hot sensation of the tongue, which was supposed to depend on acidity of the stomach. The remedies he had employed, and the regimen to which he had confined himself, for some time past, had materially improved the power of the digestive organs; so much so, that he declined the further use of medicines, and considered his health to be as good as it could be expected to be in an individual beyond the middle age of life. Notwithstanding this improvement, however, the heat of tongue still remained, when he was attacked with the epidemic catarrhal cough, already mentioned. He took the acid in doses of two minims repeated every second hour; and, with the cough, the heat of the tongue also rapidly abated, and altogether left him in less than four days. I have not heard that the latter symptom has returned.

“As the state of the stomach affects the tongue by sympathy, perhaps the unexpected effect of the acid in this instance may be ascribed to its reducing the morbid irritability of the secreting surface of the stomach, thereby enabling the juices of the organ to be more slowly secreted, and of a more healthy character. We know that opium and some other narcotics produce temporary relief in cardialgia, arising from acidity; but, after their first effect is over, the morbid irritability of the organ not only returns, but is augmented: if, therefore, the prussic acid produces a more permanent and equally-beneficial effect, its importance, as an adjunct to tonics in the treatment of dyspeptic affections, must be obvious.”

A very interesting letter, by Mr. Thomson, ensues, on the

* See the exposition of the doctrine of M. Broussais in the late Numbers of this Journal.

general utility of the prussic acid, in which the testimony in its favour as "a remedy of the greatest value" is very decisive.

The author briefly hints at the benefits which have resulted from the topical application of the diluted prussic to various parts; but he defers, until he shall have had more experience of its effects, entering fully on this subject.

The therapeutical part of the work concludes with an account of two instances of "phthisis" "cured" by the prussic acid, which have occurred to the knowledge of the author since the former part of the book had passed the press. We shall transcribe the case which was seen by Dr. Granville himself; for we must acknowledge our inability to discern in it evidence of the existence of *tubercular* consumption, (which is the species to which the term *phthisis* is generally applied by pathologists in an exclusive manner, and is the only one that is commonly regarded as incurable by means well known to every practitioner.) By transcribing cases thus at length, we extend this article much beyond what we should do; but we know no other way of avoiding the danger of misrepresenting the contents of the work.

"Miss Munn, aged 17, of a nervous temperament, with dark hair and regular features, the daughter of healthy parents, and herself enjoying generally perfect health, was attacked, towards the latter end of April, 1820, with every symptom of catarrh, difficulty of breathing, pain in the chest, fever, and restlessness, capable of alarming her relations. Her mother, having occasion to see Mr. Clarke of Saville-row, obtained from that gentleman a prescription, in the use of which the patient persisted for some days, without any sensible amelioration of her complaint. She indeed became considerably worse at last; when, to the pain in the chest and slight febrile rigors, were added a considerable expectoration of thick purulent matter, with regular paroxysms of fever, profuse morning perspirations, head-ach, sickness, cough, and a general wasting of the body. In this state she was taken to Dr. Batty, who ordered her to be bled, and prescribed appropriate medicines, declaring her at the same time to be in a consumptive state, and therefore beyond the chance of recovery. To this decision the afflicted parents endeavoured to reconcile their own and the poor girl's feelings; while every succeeding week seemed to confirm the correctness of that physician's opinion. On the 26th of June, Miss M. was brought to me for advice, when she presented the following symptoms, which I copy from my note-book: A general emaciation to a very great degree; eyes sunk and without animation, marked by an under and broad streak of a leaden hue; nose pointed and sharp; the *alæ* playing strongly and quickly during respiration, which was heavy, difficult, and accompanied by a considerable wheezing noise. Soreness and a sense of perpetual tickling along the wind-pipe; pain deep-seated in both sides of the chest; great tenderness, on pressure at the pit of the stomach; dry and arid skin; foul tongue; hot breath;

pulse 115, small and wiry; excessive debility; spirits much dejected, the patient bursting into tears at each question put to her on the subject of her complaint. The mother reported, moreover, that she sleeps for a few minutes only at a time, owing to an incessant hard cough; that she wakes in the morning bathed in sweat; expectorates a large quantity of thick grumous matter in the course of the day; that at night she is attacked by a regular paroxysm of fever, when she sinks on her bed powerless and exhausted, to rise next morning to a repetition of every symptom.

"This picture afforded nothing very promising. I concealed my impressions from the alarmed patient, and cheered both her and her mother with the prospect of some amelioration from the use of the hydro-cyanic acid, the nature of which medicine had, somehow or other, come to their knowledge. The form in which I prescribed it was that marked No. 7; and additional instructions were given respecting every other part of the treatment, which it is needless to repeat in this place, as they differed but little from what is usually recommended in similar circumstances. The beneficial effects of the acid became visible almost immediately. At the end of the first week, the cough, and consequently the expectoration, had diminished greatly; sleep became prolonged and refreshing; the morning perspirations ceased; the pulse was reduced to 100 beats in a minute, and assumed a healthier character. The dose of the acid, which had been limited to a drop and a half every three hours at first, was now increased to two, and she was desired to persist in it for another week. This she did with so much benefit, that the mother, considering her now as quite safe, discontinued the prussic acid, and merely attended to my instructions as to her general health. I saw the patient again in a fortnight, and the change which had then taken place in her appearance seemed to be so favourable, that I could scarcely believe it to have been the work of so short a period. I enjoined great care and quiet, and ordered some tonic medicine, contenting myself with hearing from her from time to time. Towards the end of September last, the accounts were that she was enjoying perfect health, and had done so for several weeks before. Of the truth of this assertion I had the means of satisfying myself in about a fortnight afterwards, when I found Miss M. free from every, even the most distant, symptom of complaint. It is proper to observe, that Miss M. has not yet menstruated, and that she is small of her age."

The following is the formula referred to in the foregoing history:

"R. Mucilaginis acaciæ, f. ʒ iij; aquæ rosæ, f. ʒiſs; syrupi capilli veneris, f. ʒ ij; acidi hydrocyanici med. m. xvi. F. M. L. A. *Cochleare unicum medium, si vis, singulis horis excipiatur.*"

The work concludes with a section on the "mode of prescribing the prussic acid," and a list of formulæ. The substances with which it may and may not be combined, are particularly discussed. It is not decomposed by mixture with any vegetable substances. It is decomposed by most of the

oxides usually employed in medicine, particularly by those of mercury and antimony. It may be given in conjunction with the carbonates of potash and soda; "forming one of the most successful modes of prescribing it in cases of spasmodic and whooping cough; and supplying the means of administering it in fevers under the form of an equally pleasant and elegant preparation." We refer the reader to the work for the author's particular considerations on its pharmaceutical relations to the other substances of the *materia medica* in ordinary use; only remarking, that the *protoride* of mercury (a preparation which GUIBOUT says cannot be obtained, and that what has been called a protoxide is a combination of deutoxide with metallic mercury finely divided,) is said by the author to be the only preparation of that mineral with which it may not be administered. It is of considerable importance in the administration of this medicine, Dr. Granville says, that the mixture containing it should be newly prepared every day, as it seems, on ordinary occasions, to suffer decomposition on being kept for a longer time than twenty-four hours.

Illustrations of the Efficacy of Compression and Percussion in the Cure of Rheumatism, Sprains, and Debility of the Extremities. By WILLIAM BALFOUR, M.D. Author of "*Illustrations of the Power of Emetic Tartar in the Cure of Fever, Inflammation, and Asthma, and in preventing Consumption and Apoplexy.*" 8vo. pp. 36. Hill and Co. Edinburgh; and Longman and Co. London. 1820.

THE fate of the practice of "compression and percussion" in the treatment of rheumatism, exemplifies, in a very forcible manner, the difficulties which attend the bringing a new remedy into general usage, when that remedy differs in a considerable degree in its nature from the means which may have been previously employed. Dr. BALFOUR, and some others who have adopted his practice, have published well-authenticated results of such a kind as should satisfy the most sceptical person that its powers are of a very extraordinary order; and, what is of more importance, that it will remove some of the most afflicting forms of disease, when all other known means have either failed, or when they cannot be resorted to with the slightest hope of advantage from them. We often, in the treatment of a disease, know not how far spontaneous efforts in the system have contributed to effect the salutary changes which we witness during the administration of our remedies, and we may frequently doubt whether or not the latter have had any share in producing such results; but these doubts cannot be entertained respecting the greater part of those consequent on the remedy under consideration, in the principal part of the cases above alluded to; for, when we see a person

who has been unable to move a limb at all, or not without extreme pain, for several months, perform the natural actions of it with comparative ease and facility after the application of "compression and percussion" to it for a few minutes, it is not possible to avoid believing that it is these means which have effected the change. Yet such a remedy is almost totally neglected by the generality of practitioners, although several years have elapsed since sufficient evidence of its utility was promulgated. Dr. Balfour, however, has sufficient enthusiasm and resolution to incite him to pursue, steadily and vigorously, the good cause he has undertaken, in spite of the difficulties which passion, prejudice, and apathy, have hitherto opposed to it. But he has, perhaps, not acted in the most prudent manner, in replying to, or noticing in any way, the indirect insinuations, malignant misrepresentations, and calumnies, which have been aimed at him and his practice. He should regard them as the natural outcries of impotent malice and vexation; and he may feel assured that no person whose opinion is worthy of consideration will fail, sooner or later, to appreciate them as they deserve. We think he violates what is due to himself in noticing them at all: certainly, the showing of indignation is very unbecoming in him, and the manifestation of anger is still more improper. Men who are weak, and mean, and malicious, will be vexed, and lie, and calumniate; but they must not be censured because they expose their evil dispositions,—they can't help it. On such occasions as these, a wholesome lesson may be taken from the gray *houghnham*, *Gulliver's* master, who, after having enumerated all the natural vices of the *yahoos*, said, "Yet he no more blamed them for their odious qualities, than he did a bird of prey for its cruelty, or a sharp stone for cutting his hoof." What seems to have hurt Dr. Balfour most, is a feebly-insinuated imputation of *empiricism*: but how can that conduct be termed empirical which evinces a desire to explain and teach all the means of the practice employed, and which has explained them sufficiently well to enable any person of common ability to use them with success? We turn away from these absurdities: it is enough for us to indicate that they have had their influence in preventing a fair trial of the efficacy of the practice; and to show that other causes than the failure of it to effect what we might have been led to expect from it, have prevented it from being generally resorted to.

This pamphlet comprises histories of fourteen new cases, including that published by Dr. GRATTAN in the first volume of the Transactions of the Dublin College of Physicians. The first was a case of very severe rheumatism affecting the right thigh of a woman 26 years of age, and which had obliged the patient to keep her bed for two months previous to the time when Dr.

Balfour was consulted. Dr. POOLE, of Edinburgh, had been attending her for six weeks, and had, as he stated to Dr. Balfour, "prescribed in succession all the remedies he had ever read or heard of as beneficial in rheumatism, without the patient deriving the slightest advantage from any of them, with the exception of her appetite being somewhat improved."

"The patient was compelled to lie night and day upon her back, with the limb extended and equably supported on the bed. In no other position could she suffer it for a moment, without the most exquisite pain; every attempt to move it occasioned the most frightful screams. It is impossible, indeed, to conceive a human being in a more distressed and helpless condition than was this woman. There was no external appearance of disease in the limb, but the very idea of any thing touching it was intolerable to the patient. Notwithstanding, I was convinced, from ample experience, that nothing but compression and percussion could be of avail. I proceeded therefore, under the regulation of the patient's feelings, to apply the former, first with my hands, and afterwards with a bandage. I beg leave here to state that, as friction was totally inadmissible in this case, so my practice, in the cure of rheumatism and complaints allied to it, is quite different, not only in principle, but in the mode of application, from that of Mr. Grosvenor of Oxford. I have, moreover, had patients who had been previously under his care, and who say the same thing.

"At first, I proceeded with so much caution and delicacy, that three-quarters of an hour were required to go through the operation every day. For the first eight or ten days little progress seemed to be made. An evident amendment, however, was now observable. The parts could be handled with much more freedom. I now applied percussion to the sole of the foot, in order to give a tremulous motion to the whole of the limb. This accelerated the cure greatly. At the end of a fortnight I began to lift the limb from the bed, and to bend and extend it alternately. Formerly, when the limb was in any degree elevated, the whole limb was seized with tremor, attended with agonizing pain: I instantly checked these tremors, by drawing a bandage very tight round the ankle. This may appear extraordinary, but it is not the less true. Tremor of the inferior extremities can be checked at any time, by grasping firmly the tendon of the heel. The cure now went on rapidly; and, within a month from the time I was called in, the patient was on her legs."

Dr. Poole continued to attend with Dr. Balfour during the treatment above designated, and, on its termination, addressed a letter to the latter, in which, after having described the extreme severity of the disease in the strongest terms, he, on speaking of its favourable issue, says, it was, "to my unqualified conviction, justly attributable to the scientific, safe, and obviously efficacious, operation which happily superseded my practice."

The second case was one of rheumatism affecting the muscles of the arms, of, probably, about three months' duration. The

patient, the captain of a passage-smack, was so debilitated that he could hardly walk ; and he had lost in the last three months " forty-five cups of blood." He could " neither put on nor take off a stitch of his own clothes." " I applied compression and percussion," says Dr. Balfour, " to the parts affected for a short time, when he felt so much relieved that he put on his clothes without aid, with the exception of the last throw necessary to send the arms home into the coat. Next day he brought his steward with him to receive instructions, as he was determined to accompany his vessel to London the day after." On meeting accidentally with this patient somewhat more than two years afterwards, he told Dr. Balfour that " his directions were implicitly followed by his steward ; that the pain and rigidity of his back, shoulders, and arms, were soon removed ; that he was quickly restored to the perfect and permanent use of them ; and that, with the exception of a trifling flying pain occasionally, which he could easily check, the disease had never returned." The author met, on the same occasion, with Mr. Simpson, surgeon to the 56th regiment, who told him that, on his book on Rheumatism having been sent to Malta, where he was, by the direction of the Army Medical Board, he, after perusing it, determined to give the practice it inculcated a trial as soon as opportunities permitted. " I did not need to wait long," he added : " cases of both rheumatism and sprains occurred ; all of which I treated according to his [Dr. Balfour's] method, with immediate and perfect success. My professional brethren were astonished at the rapidity of the cures ; and the practice received the particular approbation of Dr. Warren, inspector of hospitals."

We shall transcribe nearly in detail another case, chiefly because it shows the benefits of the same practice in a form of disease of which we have not yet spoken. It is that of " Miss L—, a young lady of a fine form and stature," who " was seized in the summer of 1817, when residing in the country, with a numbness and partial loss of power of the left inferior extremity, accompanied at times with a prickly sort of pain which frequently attends paralytic affections. Having failed of obtaining relief in the country, she came to Edinburgh for further advice." Her friends advised her to put herself under the care of Dr. Balfour ; but this was resisted by a relation, who was prejudiced against him by the family-surgeon. But, as the patient derived no benefit from the measures employed, she determined to have the opinion and aid of Dr. Balfour ; whose narrative of the subsequent history of the case we shall transcribe.

" The disease consisted in a partial loss of sensation and power in the left inferior extremity, from the middle and back part of the thigh downwards. The patient could walk, but without confidence. In

throwing her weight on the limb, she was always afraid of coming down; and her foot could not grasp the ground. There was no pain in the limb, unless a prickly sensation, to the extent of about a hand-breadth in the thigh where the disease commenced, might be called pain. This prickly sensation was excited by firmly grasping the part.

"I told my patient that percussion was the only remedy from which I could expect any benefit in her case; and that it must be applied daily, and continued till a cure should be obtained. She assented, delighted with the prospect of being cured by any means. I proceeded to the operation; and the first application diffused the nervous power through the whole limb. A mixed sensation of pleasure and pain darted through it to the very point of the great toe. From that moment I entertained not a doubt that I would be able to effect a cure; but I warned my patient to expect that it would be very gradually accomplished. I continued my attendance every day for a short time, applying percussion over the whole limb and sole of the foot. This I did on the principle I have often mentioned, that, wherever a stimulus is applied, to that place there is an afflux of blood and nervous energy. If this principle is admitted, and it is self-evident, the objection raised by some writers to the application of percussion in cases of lameness and debility of the extremities from gout, must fall to the ground. Percussion must be admitted to be stimulant, and therefore cannot be repellent. No application can attract and repel at the same time. Those, therefore, who attempt to frighten gouty patients from the use of percussion, as calculated to repel the disease to internal parts, have other objects in view than truth or the welfare of their patients. Such writers merely cavil,—they do not reason; and, instead of instructing, only betray their own ignorance.

"In the course of ten days, an improvement was perceptible in my patient's walk. She trusted her weight with more confidence on the limb, and planted her foot with greater firmness. And, having by this time learned to apply the remedy equally well as myself, I left her in the conviction of ultimately recovering the full power of her limb.

"A short time after I left off visiting her, she returned to the country, and I neither saw her nor heard of her for about twelve months. Standing in the middle of the street one day, in conversation with other two gentlemen, I observed a young lady crossing from the pavement towards us, with a firm and equal pace; smiling as she approached, and holding out her hand to me. I gave her my hand, but did not recognize her till she spoke. As her whole manner indicated happiness, I was not afraid to say, 'And how are you?' 'Perfectly well!' she replied with emphasis. 'Did you continue the operation of percussion?' 'Every day since I saw you; and I have used no other remedy whatever.'"

There are hardly any grounds for choice amongst the rest of the cases related in this pamphlet, in respect to the manner in which they render manifest the extraordinary and peculiar efficacy of Dr. Balfour's practice; and we have, we think, adduced what is sufficient to elicit the attention of our readers to the whole.

CRITICAL ANALYSIS

OF

RECENT PUBLICATIONS, IN THE DIFFERENT BRANCHES OF
MEDICINE AND SURGERY,

In the Literature of Foreign Nations.

Παρίδες ἄρα
Ἀνδράσιν, ἔ παῖσι τοῖς ἄνδρες, ἀγαλλόμεθα.

Ueber lebende Würmer im lebenden Menschen; ein Buch für ausübende Aertze. Von Dr. BREMSER. 4to. seit. xii. und 284. Wien, 1819.

—i. e. *On living Worms in living Men; a Treatise for the Use of Practical Physicians, &c.*

[In continuation from vol. xlv. p. 427.]

THE sixth chapter of this work treats of the medicines which have been employed for verminous diseases. Amongst the immense number of those which have, at various times, been proposed as remedies of a specific character, there have been but few which have preserved, amongst the generality of practitioners, the reputation they had once acquired. The versatility of opinion on this subject is attributed by the author, 1°, to the reputed remedies having been employed without, at the same time, any regard being paid to the cause of the generation, or the presence, of the worms, and without efforts to remove it; 2°, to their having been often used where the presence of worms was only suspected from some general symptoms; or where, on the expulsion of worms, the morbid phenomena have been attributed to the presence of those animals, when they really proceeded from some other cause; 3°, to the accidental evacuation of worms, during the treatment of some other diseases, having been considered as the effect of the medicines employed. Some attempts have been made to settle these doubts, as far as the immediate effect of several medicines on the life of the worms themselves are concerned, by exposing the animals, when out of the body, to the influence of the supposed remedies: but it is obvious that nothing very precise, or applicable to practice, can be drawn from experiments made under circumstances so different from those which concern the medical practitioner.

The author divides the remedies which may be administered to human beings for the treatment of verminous diseases, into the following classes.

1°. *Mechanical anthelmintics.*—To this class may be referred the filings or powder of tin; cowhage; pulverized charcoal; and raw carrots, proposed by ROSENSTEIN. Dr. BREMSER does not doubt that these substances have produced the expulsion of worms; but he does not favour the use of them, unless combined with other remedies; because, though they may facilitate the removal of the animals, they have

no power to remedy the disorder on which the existence of the worms depends; the removal of which is the principal indication in the cure. Notwithstanding these objections, the filings of tin, and especially cowhage, will be found very useful in practice, for their efficacy in causing the evacuation of worms. On speaking on this subject to two physicians who have practised at the same Dispensary (where diseases of children are witnessed to a great extent,) for several years, one of them said that cowhage alone will generally prove a remedy for worms in children, without any other medicine, either combined with it or subsequently administered: the other said he always gave at the same time calomel, or some other medicine, with the view of restoring the healthy state of the intestines. The cowhage is most conveniently administered in the form of an electuary.

2°. *Specific anthelmintics*: of which he enumerates,—*a*, cold water in large quantities and almost constantly repeated doses, proposed by ROSENTEIN and PALLAS, to be drank after having taken a purgative. Dr. Bremser supposes that the cold water causes their expulsion by the shivering which it produces, on being drank in large quantities, the disagreeable impression of which on the worms causes them to leave the intestines: he also thinks that the animals absorb some portion of it, by which they become tumid, torpid, and incapable of resisting the afflux of the quantity of water drank by the patient. Salt water, he says, acts still more efficaciously.

b. *Valerian root*.—This forms the principal ingredient of the well-known vermifuge of STORK; and is, he considers, a useful remedy in many cases, especially where the nervous system is much affected.

c. *Onions and garlic*, are reputed remedies, but are not at all certain in their efficacy. Several instances are, however, recorded, in which they seem to have been efficacious, for tænia especially. BRERA recommends a liniment made of an ounce of bruised garlic and a drachm of camphor dissolved in six ounces of sulphuric ether, to be applied to the abdomen; especially in conjunction with some others that will be hereafter described.

d. *Semen santonici*, and the seeds, or rather the mature flowers, of the *tanacetum vulgare*.—The author remarks, that the fine powder of those substances soon loses great part of its virtue: he thinks the seeds only grossly powdered are more efficacious, in which form they seem also to act beneficially in a mechanical manner. BRERA believes these medicines to possess considerable efficacy.

e. *Conserva helminthochortos*, has been sometimes used with apparent advantage in quantities of from a scruple to half a drachm, or rather of an infusion to the extent of two drachms in four ounces of water, daily.

f. *Chenopodium anthelminthicum*, (Jerusalem oak.)—The seeds of this are frequently used in America. CHALMERS says, that it is the active ingredient of the anthelminthic which has been so famous in North America.

g. *Angelica bark*.—An ounce of this is boiled in three pints of water down to one pint; one or two ounces of which are to be taken every morning. This medicine is apt to produce griping; but it pro-

duces the expulsion of worms in large quantities when they exist in great numbers.

h. Grana tilia (linden-seeds), are rather purgatives than proper anthelmintics.

i. Spigelia anthelmia and *marilandica*.—The former of these has been long employed in America: the latter was praised by BERGIUS as the best of vermifuges; and Dr. LINING, of South Carolina, favours its use. Both produce narcotic effects, and at the same time copious evacuations from the bowels. They are violent medicines, and may be spared without much loss, according to Dr. Bremser. The *spigelia anthelmia* has, however, been strongly recommended by BROWN, ROSENSTEIN, and some other physicians; and BRERA says, that he has frequently prescribed it with the greatest success.

k. Surinam root, or the *geoffrea surinamensis*, (worm-bark tree.)—SCHWARTZE says, a very efficacious tincture may be prepared by digesting two ounces of the root with four ounces of rectified spirit of wine and two pints of water for six days; the mixture is then reduced, by evaporation by a gentle heat, to one pint. For two days, the patient is to take two table-spoonfuls in the morning fasting, and repeat the dose twice after intervals of an hour; on the third day, he is to take the rest, by tea-cupfuls; on the fourth, a purge of jalap and calomel. The author does not think that much reliance should be placed on this medicine. Brera, on the contrary, says that he has employed it, and known it employed, many times, with efficacy against ascarides, both vermiculares and lumbricoides. He says, it is more beneficial when combined with valerian.

l. Cevadilla, *veratrum sabadilla*.—The seeds were first employed, internally, by LOESER, and afterwards by SCHMUCKER, in cases where NOUFFER's remedy had failed. They should be cautiously employed, as they possess very acrid qualities. Children but rarely bear more than three or four grains, daily. Schmucker obtained great success in his treatment; but it should be remarked, that he used at the same time calomel and jalap. He thought clysters of a decoction of the seeds with milk, beneficial, in conjunction with other means, in cases of ascarides vermiculares.

m. The green rind of walnuts, in the form of infusion or extract, has been much esteemed in modern as well as in ancient times. It may be given in doses of one or two drachms, in infusion or decoction; or two drachms of its extract may be dissolved in half an ounce of cinnamon-water, and from fifteen to thirty drops of the mixture given to children twice a-day.

n. Assafetida, is sometimes used, in the form of a pill.

o. Camphor, was particularly recommended by PRINGLE and MOSCATI for the *ascarides lumbricoides*. It may be given in doses of from a grain to a scruple, in pills, with or without assafetida, or infusion of valerian. It would appear, from the observations of PALLETA, that this substance is particularly noxious to the worms; and that, when administered by way of clyster, in children who could not be made to take it by the mouth, it has occasioned the worms to retire from the lower to the upper part of the intestines; where they have,

nevertheless, been destroyed, when the use of the remedy has been vigorously pursued in the same manner. It is thought, too, that this medicine will prevent the development of any ova which may be left in the intestines. Brera says, that he has always used it with the greatest success.*

p. Polypodium filix mas, has continued to possess, down to the present day, the reputation it had in the time of GALEN. It is said by some authors to be almost a certain remedy against the *tænia lata*, (*bothriocephalus*;) but not, according to Dr. Bremser especially, so certain against the *tænia cucurbitina*, the prompt regeneration of which it does not prevent. It is the principal ingredient in the famous remedy of NOUFFER, said to be an infallible specific against the *tænia*. It may be given in doses of from one to three drachms to an adult, in powder or infusion.

q. Petroleum.—This has been said to be commonly used with success in Egypt; given in doses of twenty or thirty drops, for three mornings in succession; after which time a purgative is administered. LARREY, however, says that *tænia* is but rarely seen, and that petroleum is unknown, in Egypt. It has been, however, a famous remedy at Montpellier.

r. Oil of turpentine.

s. Cajeput oil; recommended by RUDOLPHI.

t. Dippel's animal oil, taken in the dose of from three to ten drops, three times a-day, in broth, is an efficacious anthelmintic, according to Rudolphi.

u. Chabert's empyreumatic oil, is, the physiologist just named says, preferable to the foregoing. It is prepared in the following way:—Take fetid oil of hartshorn, one part; oil of turpentine, three parts; mix them well together, let them stand for four days; then distil the mixture in a sand-bath, and draw off three-fourths of the liquor. It is to be kept in a phial with a glass stopper, tied over with a piece of bladder.

v. Fluid mercury.—Water boiled over mercury is an old domestic vermifuge. Experience has, however, proved that the water takes up only the impurities, or a little of the oxide which exists on the surface of the quicksilver of commerce. Fluid mercury has no specific action on worms.

w. Muriate of barytes, has been much recommended, (since its internal use was advocated by CRAWFORD and CLARK,) in verminous diseases, by several physicians, as one of the most powerful remedies against *ascarides vermiculares*. Great caution is required in its administration.

The many remedies which have been proposed for external application, are considered by the author to have but little utility; though, he thinks, some of them may act beneficially by their influence on the nervous system, and especially on the abdominal plexuses. In this opinion many other experienced practitioners are at variance with

* See the xxvth volume (p. 25) of this Journal, for a curious instance of the efficacy of this remedy.

him. We know a physician to a public institution, where his practice for some years has been very extensive in the diseases of children, who says that the external application of oil of turpentine to the abdomen, is but little less efficacious than its internal exhibition in a great proportion of cases; and FRANK, WEIKARD, and ALIBERT, have regarded the same mode of using the anthelmintics in general in as favourable a manner.*

The third class of anthelmintics consists of *Purgatives*.

The chief use of medicines of this class, is to expel the worms destroyed or collected together by the foregoing remedies. Several authors have recommended, especially, the sulphate of soda and of potash, muriate of soda; tartarized antimony; calomel; muriate of barytes; the fat oils, especially nut and castor oil. Dr. Bremser prefers senna and jalap to any of them. Calomel, he thinks, is a vermifuge only in consequence of its being a purgative.

4°. *Tonics*.—These may be employed to restore the strength of the intestinal tube, that may have been destroyed by the remedies employed for the removal of the worms. The author recommends particularly bitters and iron. Professor Brera is disposed to consider tonics as of considerable utility for the immediate relief of the verminous disorder itself, which he regards as a state of asthenia of the digestive organs. He advises the administration of them with purgatives in many instances.

Dr. Bremser says nothing respecting the use of emetics, which have been very strongly recommended by some physicians. Prof. Brera strongly reprobates the practice, as he does the exhibition of violent purgatives, which, although they may occasionally produce the expulsion of worms, often tend to favour their re-production, by increasing the morbid state which is the cause of their presence in the intestines.

The agency of culinary salt as a remedy against worms, is not noticed by Dr. Bremser. On this subject we refer to a paper by Mr. MARSHALL, published in this Journal for May 1818; and to HEBERDEN's paper in the first volume of the Transactions of the College of Physicians. We shall notice the oil of turpentine hereafter, in a particular manner.

So far our observations have related principally to the remedies for verminous disorders in general; but remedies which are efficacious for one species, ascarides for example, are often of no utility for others, as for the tæniæ. This leads us to the seventh chapter, in which the

* Brera recommends the following liniments, for friction on the abdomen, as of great utility, especially in children.

1°. A drachm of ox-gall, and the same quantity of Castile-soap, to be made into a liniment with a sufficient quantity of oil of tansy.

2°. Two ounces of ox-gall, half an ounce of socotrine aloes, and the same quantity of powder of colocynth, are to be digested for twenty-four hours, in a warm place, in a sufficient quantity of gastric juice or saliva, and the mixture to be made into a liniment with lard.

Alibert says, that the gastric juice (if such a fluid exist) is devoid of utility; for that he has obtained equally advantageous results from the application of the remedies without it.

author discusses the particular treatment of the several species of worms.

1°. The *tricocephalus dispar*.—We stated, in a former part of this Review, that there was no evidence of the existence of this worm in the living body, for that no practitioner had mentioned having seen them expelled during life. Dr. Bremser, however, we now find, says that it has once occurred to him to remark a worm of this species, which was evacuated from the anus with a multitude of *ascarides lumbricoides* and *vermiculares*. Until more instances of this kind have been witnessed, this species of worm cannot be regarded as an object of medical treatment.

2°. The *oxyures vermiculares*, are commonly productive of distress by the irritation which they excite, though sometimes thousands exist together without producing the slightest inconvenience. The most common effect from them is an insupportable itching about the rectum, which is joined in children with much nervous disturbance. The destruction of these worms is not, in general, very easily effected: not only in consequence of the great rapidity with which they are reproduced, but also from the mucous secretion of the intestine serving to defend them against the means employed for their destruction. Dr. Bremser has found very useful, though often only as a palliative, the following mode of treatment:—In the first instance, in order to remove the worms from the upper part of the large intestines, he directs to be taken, morning and evening, a coffee-spoonful of the electary marked No. 1, in the collection of formula which will be presently adduced; with which he joins a sufficient quantity of jalap to purge the bowels gently. Besides this, he orders two clysters daily, prepared according to formula No. 2, taking care that the lower intestines have been evacuated previously to their administration. In patients of a very irritable habit of body, a spoonful of fresh ox-gall should be added to each clyster. The patient should endeavour to retain the clysters as long as possible. This practice is to be continued for several weeks. A clyster of olive-oil will procure immediate relief from the pruritus occasioned by these worms.

Professor Brera says, that we may sometimes relieve the irritation produced by these worms, when they are situate near to the anus, by passing into the rectum a piece of bacon tied to a string: this is to be withdrawn after a little while, when a considerable number of *ascarides* will be found attached to it. The practice may be repeated until all the worms are brought away.

Clysters of decoction of Surinam-root, assafoetida, the *veratrum sabadilla*, aloes and lime-water, salt-water, and *semen santonici* (especially recommended by Mr. CHARLES CLARK), are also occasionally efficacious in bringing away those collected about the anus: but, when there is much irritation of the intestine, castor-oil and mucilaginous liquids are preferable for this purpose. Tobacco was once strongly recommended with the same intention; but Heberden stated that it was injurious rather than beneficial, and it does not seem to have been much employed.

The more efficacious of the remedies administered by the mouth,

generally employed, are the spirit of turpentine, camphor, muriate of barytes, cowhage; and, in certain cases, especially when the functions of the digestive organs are much deranged, calomel; with, or without, various tonics, as the muriatic tincture of iron, and bitters, or assa-fœtida. The latter medicines are generally especially useful after the worms have been evacuated, to prevent their re-production.

3°. The *ascarides lumbricoides*.—In the choice of the numerous remedies proposed for these worms, it is of essential importance that those should be preferred which tend not only to destroy them, but to remove the causes of their generation, or their presence, in the intestines. Dr. Bremser, when he suspects them to exist, puts in practice the following measures. He orders a child to take, morning and evening, a dessert-spoonful of the electuary No. 1: after three or four days, the intestinal evacuations begin to be more frequent and more copious than ordinary, and commonly much mucus, and perhaps a few worms, are expelled. Where the latter effects do not take place, he directs the dose of the medicine to be increased, or to be taken more frequently. If, with this addition, the desired amendment is obtained, he advises the electuary to be continued, moderating the dose, so as to produce only some augmentation of the evacuation of feces and intestinal mucus; administering also, from time to time, the mild purgative No. 3. According to the author, it is a matter of indifference whether or not worms are evacuated during the use of these means: sometimes they are not passed until all the signs of the verminous disorder have disappeared. In patients of a leucophlegmatic habit of body, in order to prevent a prompt relapse, he orders for some time a few drops of the mixture No. 4.

In respect to diet, the patient should abstain from the extensive use of farinaceous substances, legumens, and of fat meat; and should eat, in preference, dry bread. Dr. Bremser has found this treatment so efficacious, that he has not had occasion to employ any other means.

The measures which have most generally been found efficacious against these worms, do not differ materially from those proper in the treatment of *ascarides vermiculares*; only, as the *lumbricoides* inhabit higher parts of the intestines, medicines administered by the mouth are more likely to be successful, and clysters less so, than with the latter species. We have nothing of importance to add to what we said when treating of the *vermiculares*, and in the express review of the particular remedies. We may, however, mention here that the cowhage is in no cases so generally efficacious as in the treatment for *lumbricoides*.

4°. The *tænia lata* and *cucurbitina*.—The symptoms in the case of the presence of either of these worms, do not differ remarkably from those which arise from the *ascaris lumbricoides*: the expulsion of some of the joints of them is the only means of ascertaining, with certainty, their existence. In general, the ordinary vermifuges fail to produce their evacuation; and hence it is that such a multitude of medicines have been tried. It is still more difficult to prevent their regeneration. Dr. Bremser says, that he has never known the latter species of *tænia* expelled by the ordinary remedies. He has had, he says, in the course

of ten years, more than five hundred patients affected with *tænia cucurbitina*, amongst whom two were children of about a year and a half old. Only four of all those patients were obliged to have recourse a second time to the use of the remedy which he employed; and in one only had the worm been re-produced at the end of two years. The following is his mode of treatment, which he considers to be as efficacious in the cure of the *tænia lata* as in that of the *cucurbitina*. He commences with the electary No. 1, which is to be taken as directed when speaking of the *ascaris lumbricoides*. On the quantity prescribed being consumed, the patient is to take, having his mouth full of water at the instant, two coffee-spoonfuls of the oil No. 4. This medicine is much more ungrateful to the smell than to the taste. It is better that the patient should not rinse his mouth afterwards, to remove the particles of the oil adhering about it: he should only have his mouth previously full of that liquid in order to facilitate the passage of the oil to the stomach. He may, however, chew a piece of cinnamon or some cloves. If this dose should produce irritation or head-ach, it should from the first be diminished; and, when it excites nausea if taken fasting, it may be administered an hour or two after breakfast. The ardor urinæ which sometimes supervenes on the use of this remedy, may be removed by some oily or amygdaline emulsion. By proceeding in this way, the patient, at the end of ten or twelve days, will have taken from two to three ounces of the medicine. He should then take the purgative No. 3; and after this resume the use of the oil. Generally four or five ounces should be consumed; and, where the worm has resisted the influence of other medicines, six or seven ounces may be requisite. It will be prudent, however, to continue the treatment for some time longer, partly to remove the disposition to the generation of worms, and partly to prevent the development of the ova in the intestines which may have been left by the worm which has been expelled. All secondary treatment is superfluous, excepting in the cases where there is a predominant disposition to the formation of mucus in the intestines and the generation of worms; against which the drops No. 5, may be advantageously employed. The diet during the treatment should be subjected to similar restrictions to those mentioned as proper in cases of *ascarides*. The worm, under this treatment, generally comes away in pieces, sometimes almost like strings of mucus; but the proper form of the animal can commonly be recognized by careful examination. When no more portions have been evacuated by the end of three months, the author says we may be certain that the whole has been destroyed. When new fragments appear at the end of two or three years, as is sometimes seen, it must be considered that a new worm has been generated.*

* The following are the formulæ alluded to in the foregoing observations:

No. 1.—R. Sem. cinæ s. tanacetî, contus. ʒj.

Pulv. rad. valerianæ, ʒij.

— jalapii, ʒss. ad ʒij.

Potassæ sulphatis, ʒj ss. ad ʒij.

Oxym. scillæ, q. s. ut fiat electuarium.

Being satisfied with this mode of treatment, and not placing any confident reliance on the other means which have been proposed as remedies for *tæniæ*, Dr. Bremser passes them over almost unnoticed: we shall, therefore, deviate a little more than ordinary from the running-base with which we have accompanied the author, and give an *arbitrio* to fill up the vacancy which he has left in the piece.

We should first remark that several cases have occurred in which very long *tæniæ* have been evacuated whole, after the use of cold water in the way recommended by Rosenstein; and still more when various mineral waters have been thus employed. It is, probably, from their having been drank in the way just designated, that the waters of *Chad's Well*, at Battlebridge, were a few years since so celebrated a remedy for *tænia*.

The most powerful, and most constantly efficacious, of all the remedies ordinarily employed for *tænia*, is the oil of turpentine; the introduction of which into practice is due to Dr. FENWICK. The utility of this medicine is, however, too well known to English practitioners to render it necessary to dwell upon it here.

The remedy of NOUFFER, the formula for which was purchased for a large sum of money by the king of France, and which, towards the middle of the last century, was so much celebrated in France, consisted principally of the *polypodium filix mas*: but, according to the report of the French physicians who were appointed to investigate its effects, it is of no utility against the *tænia cucurbitina*, though generally a remedy for the *tænia lata*. Dr. HARRENSCHWAUD had previously employed the same remedy in Germany, and had acquired great reputation for the success of his practice. Brera, however, relates some cases in which the administration of this medicine was followed by the

No. 2.—R. *Herbæ absynthii*
Rad. *valerianæ*, ā ʒj.
Sem. *tanacetii*
Cort. *aurant.* ā ʒ ss.

To be reduced to a coarse powder, and mingled together. Two table-spoonfuls are to be put in two pints of boiling water, and the mixture suffered to stand in a close vessel for twelve hours; it is then to be strained through cloth, and will serve for two clysters; to each of which a spoonful of fetid oil of hartshorn is to be added.

No. 3.—R. *Pulv. jalapii*, ʒj.
Fol. *sennæ*, ʒss.

Potassæ sulphatis, ʒj. M. f. Pulvis, in tres quatuorve partibus dividendus. The patient is to take the half of one of these powders every half-hour, or every hour, until the purgative effect is produced.

No. 4. is the vermifuge oil of CHABERT, the mode of preparing which was stated when we passed in review the different remedies for worms. Dr. Bremser is the first person who proved that this medicine might be safely and effectually administered to human beings: it had previously been given only to animals.

No. 5.—R. Tinct. *aloes comp.** ʒj.
Tinct. *martis. pom.* ʒj.

Elixir *vitrioli mynsichti*, ʒ ss. M. To be taken in doses of ten, twenty, thirty, or more drops, three times a-day, in a glass of water or wine.

* This is according to the *Pharm. Austriaco-provincialis*, and is the same with the *elixir proprietatis* of PARACELSUS, which does not differ materially from the *T. aloes comp.* of the London and Dublin Pharmacopœias.

evacuation of the armed *tænia*; but it seems that this occurred when the worms were young, and not able to attach themselves firmly to the intestines by the crotchets in their head.

The oleum ricini, taken in doses of two or three ounces, obtained much estimation in the practice of ODIER of Geneva, as a remedy for the *tænia lata* only. It appears occasionally to kill the worm, and at the same time produce its expulsion from the bowels. Brera says, that it is sometimes efficacious against the armed *tænia*.

Tio, in filings especially, as already noticed, has acquired much credit as a remedy for both the species of *tænia*. The reputation of this medicine seems to have declined of late years, particularly in England. We have, ourselves, known very severe colic produced by it; but we should, at the same time, state that we have frequently known it produce the evacuation of *tænia* of both species.

An apothecary of Berlin, named MATHEU, acquired much reputation for many years for the treatment of *tænia*, and received great honours from William III. in consequence of his successful practice. His remedy was a combination of tin-filings, male fern, santeny seed, jalap, scammony, gamboge, and sulphate of potass.*

Another remedy, which has been proposed with much confidence, is the pomegranate root, given in the form of decoction; and the evidence in its favour is such as to place it amongst those most deserving of trial. (See vol. xxxii. of this Journal.)

Some caution is necessary in the management of the case when the worm is partially expelled. It might appear that the animal might be wholly withdrawn by pulling at it; but, generally, either a great deal of pain in the intestines is produced by the attempt, or the worm is broken. Instead of attempting this, Brera advises that a thread should be tied around some part of the expelled portion: the worm retires a little into the intestines when this is effected, but soon afterwards passes wholly from them.

In the eighth chapter, Dr. Bremser treats of worms which inhabit other parts of the body of man than the intestines. These are—

1°. The *filaria dracunculus*, *filaria medinensis* (GMELIN). We must pass over what the author says about the history, and the description, of this worm, as it relates to natural history rather than to pathology; and he does not advance any thing novel of importance, either in regard to the symptoms which ensue from it, or to its treatment. Dr. Bremser is, however, almost singular in the opinion that it is, like intestinal worms, primitively generated in the human body. For particular information on this subject, we refer the reader to the paper of Dr. CHISHOLM, published in the 42d Number of the Edinburgh Medical and Surgical Journal, or to the abstract inserted in the 197th of our own.

2°. *Hamularia subcompressa*.—Dr. Bremser is not perfectly convinced of the presence of this genus in the human body, which TREUTLER had attempted to establish as existing in the bronchial glands, where he

* See the fifth volume, page 398, of this Journal, for a particular account of the mode of using this remedy.

stated he had found a considerable number in the year 1790. But, though so long a period has since elapsed, no other persons seem to have observed them.

3°. *Strongylus gigas*.—This worm has been found but rarely in man: it inhabits the kidneys, and perhaps also the adjacent muscles. It has often been stated by medical practitioners that worms have been passed with the urine; but, in by far the greater proportion of cases, the supposed worms have been lymphatic concretions, or else they have passed from the intestines into the bladder.

Amongst the worms which appear to be nourished by means of general absorption by their surface, there occur in man:

1°. The *distoma hepaticum*, which inhabits the gall-bladder, and perhaps also the liver. Dr. Bremser has never had an opportunity of seeing it in man. This is the common *hydatid*, which is seated in the liver of inferior animals, especially in sheep and pigs.

2°. *Polystoma pinguicola*, discovered by Treutler in fat about the ovary of a woman.

According to the author, the term *hydatid* appertains properly to those vesicles, full of a limpid fluid or more dense matter, which are found in the bodies of man and animals, and which, without any connection with the surrounding parts, are enclosed in a particular capsule appertaining to the organ in which the vesicle is seated. This definition, he says, will enable us to distinguish true hydatids from a varicose dilatation of the lymphatics, or distention of the cellular tissue. Hitherto only the two following species have been discovered in man.

1°. The *cysticercus cellulosæ*.—It inhabits the cellular texture of the muscles and the brain. Its existence in domesticated pigs has been long observed; but WERNER was the first who remarked it in man, and it is asserted to have been present in almost the whole of the muscles of the body. Dr. Bremser has nothing of importance to say respecting the etiology, pathology, diagnosis, or therapeutics, of these worms. Nothing, indeed, seems to be known on the subject.

2°. *Echinococcus*.—RUDOLPHI, as we have already stated, makes two species of hydatids,—those having, and those not possessing, peculiar life; whilst Dr. Bremser maintains that it is only those which are enclosed in a particular capsule, not attached to this capsule nor to the organ in which they are seated, that are real worms; and to these he appropriates the term *hydatid*. None of those animals, of this genus, having a sort of crown with crotchets and a sucking mouth, has been found in man; but distinct vesicular worms, of a spherical figure, have been observed in all parts of the human body excepting the intestines. Dr. Bremser's own observations have led him to agree with several former authors, that hydatids, of the genus *echinococcus*, have been found in the uterus and placenta, both with or without a fetus, in different instances. He speaks particularly of a mass presented to him by Dr. HEIM, in which, after very accurate examination, he was satisfied that peculiar animal life existed. The consideration of this subject, beyond an account of the opinions of the author, is foreign to our object on this occasion, which is to produce a practical

article respecting the treatment of worms; and our limits will not permit us now to enter on a full discussion of it, even were we so disposed.

In the twelfth chapter, the author treats of what he terms "*pseudo-helminthen*," by which appellation he designates those foreign bodies, animal or otherwise, which have passed from the human body during life, or have been found in it after death, and which have erroneously been supposed to be real *entozooa*, or visceral worms. Those noticed by different modern writers, to which the author confines his remarks, are the following:

1°. The *dytrachycerus*, (*cysticercus bicornus* of ZEDER; the *dicercus rude* of RUDOLPHI; the *ditrachicerosoma* of BRERA.—This supposed worm was described by SULZER. Dr. Bremser, however, doubts whether the little bodies in question, observed in the feces of the patient, were worms: he is disposed to think that they were vegetable seeds which had undergone some degree of the germinating process.

2°. The *ascaris stephanostoma*, and

3°. The *ascaris conosoma*.—LENZ thus terms two bodies which BRETSCHNEIDER supposed had been evacuated from the bowels of a young man. They were probably, the author thinks, larvæ of flies.

4°. The *cercosoma*, found by CANALI in some feces, and by him regarded as a new species of intestinal worms. It was the larva of an insect; probably of the *cristalis pendulis*.

5°. The *hexathyridium venarum*.—This worm, supposed by TREUTLER to have come from a vein in the leg of a young man in the act of bathing, who was at the same time affected with *ascarides vermiculares*, was, in the opinion of Zeder and Rudolphi, a worm of the species *planaria*, which lives in water and sucks blood.

6°. *Dyacanthos polycephalos*, observed and described by STIEBEL* as an intestinal worm, appears, from Rudolphi's examination of it, to have been a dried portion of some vegetable-stalk, probably a raisin-stalk.

6°. *Worms in the teeth*, as certain substances which have come from carious teeth, after the use of fumigations, have been called. These, according to the author, are only the seeds of henbane or some other analogous plant, used for the fumigation, which (deprived of their capsule by having been slightly burned previously to their use) have, on escaping from the tooth, fallen accidentally into water, when, by the unequal expansion of the fibres of the germ, the seed has taken a circulatory movement, and been imposed on inaccurate observers and the vulgar as a living animal.

* In MECKEL's *Deutsches Archiv*. 3 B. 2 H. s. 174.

Medical and Physical Intelligence.

TRANSACTIONS OF SCIENTIFIC SOCIETIES.

ROYAL SOCIETY of London.—Nov. 9. A PAPER, by Sir E. HUNT, was read, entitled “*On the Black Rete Mucosum of the Negro, being a Defence against the Scorching Effects of the Sun’s Rays.*” The author began by stating some observations which had induced him to form the opinion that the scorching effects of the sun’s rays are produced, not merely in virtue of their heating power, but by the joint agency of their heat and light. To verify this opinion, he made several experiments which showed that the face and hands may be exposed to a temperature of 100°, or even 120°, without pain being produced, provided the light be excluded: but that if the same, or even an inferior degree, of heat be produced by the direct rays of the sun, the parts are scorched, and blisters are produced. This effect he found to be completely prevented by covering the hand or face with black kerseymere; and the same purpose is attained by the black rete mucosum of the negro. In those cases when a black covering was superimposed, perspiration came on; and the same takes place on the skin of the negro, when exposed to the direct rays of the sun.

The author observed, also, that the eyes of those animals exposed to the strong light of the sun are furnished with a black pigment, apparently for the same purpose; while others, which are abroad by night, and consequently not needing such a protection, are not provided with it.

Nov. 16.—A paper, by Sir H. DAVY, was read, entitled “*On the Magnetic Effects produced by Electricity.*”

On repeating Oersted’s experiments, the author stated that, with a voltaic battery of 100 four-inch plates, the south pole of a magnetic needle placed under the communicating wire of platinum, the positive end of the apparatus being on the right, was strongly attracted by the wire, which was shown to be itself magnetic by its power of attracting steel-filings. The wire also was found to communicate permanent magnetism to steel bars attached to it transversely; while such bars placed parallel to the wire, were only magnetic during their connection with the apparatus.

Actual contact, however, of the steel wires with the platinum, or other metal forming the conductor, was found not to be necessary; for magnetism was imparted to a needle placed transversely to the connecting metal, but at some distance from it.

Sir H. next related some experiments, showing that the magnetic power is proportionate to the quantity of electricity passing through a given space, without any relation to the transmitting vessel; and that, the finer the wires, the stronger the magnetic effect.

The author found that an analogous effect was produced by the discharge of a Leyden phial through a wire; and, by passing the discharge of a Leyden battery of seventeen square feet through a silver wire

with a steel bar transversely attached to it, of two inches in length, the latter became powerfully and permanently magnetic. The same effect was produced at a distance of five inches through air, water, and even through thick plates of glass.

When several wires parallel to each other formed part of the same circuit, each became similarly magnetic to the single wire, and the opposite ends of each wire were in different magnetic states, and consequently attracted each other. When two voltaic batteries were placed parallel to each other, the positive end of the one being opposite to the negative end of the other, and transmitting their electricity through two wires, such wires repelled each other, because the opposite ends were in similar electrical states.

The Copley medal has been voted to Professor OERSTED for his important discovery of the connection between electricity and magnetism.

WE extract the following aphorisms respecting the treatment of *Puerperal Fever*, from the Inaugural Thesis of a Dr. LEGOUAIS, maintained during the last year before the Faculty of Paris; not because they are *new*, for they are merely principles which Dr. ARMSTRONG had previously established in the most satisfactory manner, but because the author was a pupil of Professor CHAUSSIER, and for a long time a resident student at the Lying-in Hospital *La Maternité*. They are, therefore, deduced from very extensive observation, and are, no doubt, the precepts of the excellent Professor whom we have just named. Such coincidences of opinion, especially when the extraordinary acquirements, remarkable acumen of observation, and very rare intellectual talents, of Chaussier are considered, cannot fail to give a great degree of interest to the following paragraphs.

“ 1. Excepting in the cases where there exist some particular and manifest contra-indications to the use of local, and especially of general, evacuation of blood, in the treatment of puerperal peritonitis, this measure should be the first to be employed.

“ 2. In order that bleeding may be followed by success as certainly as we can ever expect in the treatment of diseases, it should be employed in the first stage,—that is to say, in by far the greater majority of cases, before the termination of the first day of the disease; and, moreover, that a sufficiently abundant quantity of blood should be taken away to destroy at once all the violence of the malady.

“ 3. When employed not in conformity with those two essential conditions, sanguineous evacuations, far from being useful, always become very injurious to the patient, by destroying her strength and the resources of nature, without having a durable influence against the disease.

“ 4. In the cases,—and these are the most common instances,—where constipation is one of the attendant circumstances of the disease, another indication, not less pressing for its fulfilment, is joined with that of blood-letting, which is to excite alvine evacuations, by gentle, or even by somewhat active, purgatives, and thus to maintain a sort of

artificial diarrhœa during the whole of the duration of the disease; a duration which, in the greater number of cases where the termination is favourable, does not exceed the space of from two to four days.

“ 5. When a diarrhœa exists from the commencement of the disease, nothing should be done to suppress it when it is moderate: when, on the contrary, it appears excessive, none but mild and soothing means should be employed, such as emollient clysters, mucilaginous drinks, and some opiates; and we should by no means add to the irritation by the administration of astringents.”

WE select the following theorems from the Inaugural Thesis of Dr. RATIER, sustained last year before the Faculty of Paris. They are, in almost every instance, only conformable with some of the inferences comprised in the excellent Treatise on the Blood of Mr. THACKRAH; but it can never be considered uninteresting to witness a similarity of results from the researches of different persons on an important and practical subject relating to medicine. Alluding to the *buffy coat* on the blood, Dr. Ratier says—

“ 1. It is foreign to the state of perfect health, and equally so to bilious, mucous, typhoid, and asthenic, diatheses; to passive hemorrhages; and to nervous and organic affections.

“ 2. It appertains to the state of plethora, but only when it exists with a disposition to inflammations in general, and especially to those of the serous and parenchymatous membranes. It may exist without general inflammatory disposition in the system, when there is a very intense local inflammation.

“ 3. Its presence, and, when it exists, its thickness and density, depend on the intensity of the inflammation, on the largeness of the incision, on the form and rapidity of the jet, and, lastly, on the form of the vessel in which the blood is received.

“ 4. It is always composed of fibrine,—at least of a great proportion of that substance.

“ 5. It always manifests itself in a direct ratio with the intensity of the inflammation.

“ 6. Its presence, in connection with the other symptoms of inflammation, confirms the diagnostics of that affection.

“ 7. The absence of it should not be considered a reason for not resorting to blood-letting, if it be otherwise indicated.

“ 8. Its presence without clearly evident signs of inflammation should rouse the attention of the practitioner, and render him very circumspect if he employs tonics.

“ 9. Some anomalies may occur in respect to its existence, and in some cases, even with all the apparently requisite conditions, it is not manifest; but then we constantly find an extraordinary degree of density of the coagulum.”

WE transcribe the following table, by Dr. JOHN FORBES, of Penzance, from a late Number of Dr. THOMSON's *Annals of Philosophy*:—

Comparative View of Mean Temperature at different Places in Great Britain.

	Time of observation.	Mean Temperature.												Mean Temperature.					Annual range.	Mean variation of successive months.
		WINTER.			SPRING.			SUMMER.			AUTUMN.			Winter.	Spring.	Summer.	Autumn.	Whole year.		
		Nov.	Dec.	Jan.	Feb.	March	April	May	June	July	August	Sept.	O							
Isle of Wight, 10 yrs. (1809—1818)*	a.m. 9	44	39	37	41	44	46	56	62	65	62	58	51	40	43	61	57	50	25	4.66
+ Penzance, 11 yrs. (1807—1817)†	8†	45	42	40	42	43	46	53	57	59	59	56	50	42	43	56	55	49	19	3.16
Skidmouth, 2 yrs. (1813—1814)‡	8	44	41	34	40	43	50	54	60	64	63	59	50	39	44	59	57	50	30	5.00
Exeter, 5 yrs. (1814—1818)§	8	43	37	34	38	41	44	51	57	59	57	53	47	38	41	55	52	46	25	4.16
Gosport, 4 yrs. (1816—1819)¶	8	43	37	39	39	41	46	52	60	62	61	57	49	39	42	58	55	48	25	4.16
London, 3 yrs. (1817—1819)**	8	45	36	38	39	40	43	52	59	61	60	56	48	39	41	57	54	48	25	4.16
Kinfauns, 6 yrs. (1813—1819)††	8	39	34	32	36	37	48	54	54	57	55	51	44	35	38	53	50	43	25	4.16

* Original Journal, by — Kirkpatrick, Esq.

† I have stated the time here at eight, a.m. but the observations were taken at seven, a.m. in all the months except December and January. Of course, some allowance ought to be made on account of the earlier (and colder) period of observation at Penzance.

‡ Original Journal, by Thomas Giddy, Esq.

§ Dr. Clarke, in *Annals of Philosophy*.

¶ Original Journal, by E. P. Pucher, Esq.

** Dr. Barneby, in *Annals of Philosophy*.†† Mr. Cary, in *Philosophical Magazine*.†† Thomson's *Annals*.

†† These two columns are formed from the mean temperature as given in the preceding part of the table.

REPORT OF DISEASES.

THE diseases most prevalent during the last month have differed but little from those we mentioned in our Report for the one immediately preceding; a circumstance easily accounted for by the similar states of the atmosphere during these periods. Rheumatism continues to abound in all its forms, and we have recently seen three cases in which the disease appeared to be translated to the chest: in two of these instances, the attack of pain in the region of the heart, accompanied with much anxiety and disturbed respiration, followed so closely the subsiding of the pain and swelling of the limbs, as to leave no doubt of its real nature. We have nothing to add to what we have already stated regarding the treatment of rheumatism, except that we have tried the *vinum seminum colchici* in eight or ten cases, without being able to perceive any advantage possessed by this form over the one in more general use. Of this, however, we are quite satisfied, that it acts more readily upon the bowels,—rather severe purging having in several instances followed its exhibition even in moderate doses. When this drug does irritate the bowels, the evil is best remedied by magnesia, or a few grains of ipecacuanha, two or three times a-day. We are likewise convinced that it is a mistake to suppose that this operation upon the bowels is in any way connected with its proper effects upon the constitution; and, consequently, that it is best to check it immediately, upon the same principle that we give opium when diarrhœa arises during the exhibition of the blue-pill. Both the forms of colchicum seem to us equally efficacious remedies in those chronic affections of the mucous membrane of the lungs in which the last month has been abundantly prolific.

Measles, which during the two preceding months had been epidemic, are now again disappearing, though some cases still occur. The sequelæ of the disease, however, are by no means overcome, the cough in many proving obstinate, and bronchitis ensuing in some cases.

Some severe cases of diarrhœa, attended with distressing head-ach and nausea, have likewise occurred: these have seemed to be caused by the abuse of malt-liquors.

Bronchitis, especially in children, has been more prevalent, we think, during the last two or three weeks than heretofore during this season; and we have witnessed a few cases of croup; two of which, affecting children of the same family, are very remarkable, in the disease passing into a chronic form, and being attended with paroxysms of such loud croaking that respiration may be heard in another room than that in which the children may happen to be. These paroxysms come on at irregular periods, without evident exciting causes, and continue for about half an hour; the breathing in the intervals can hardly be heard. This affection has been of two months' duration, in its present form, according to the mother's account. We did not see the patients until a few days since. The future history of the cases shall not be forgotten in our Reports.

Whooping-cough is very common. The disease has generally appeared as simple bronchitis in the first instance; and then, after from two or three to ten days, the whooping has come on. We have employed the colchicum in several cases of this affection, with results that make us consider that in this and the prussic acid we possess new and very powerful means of combating that disease.

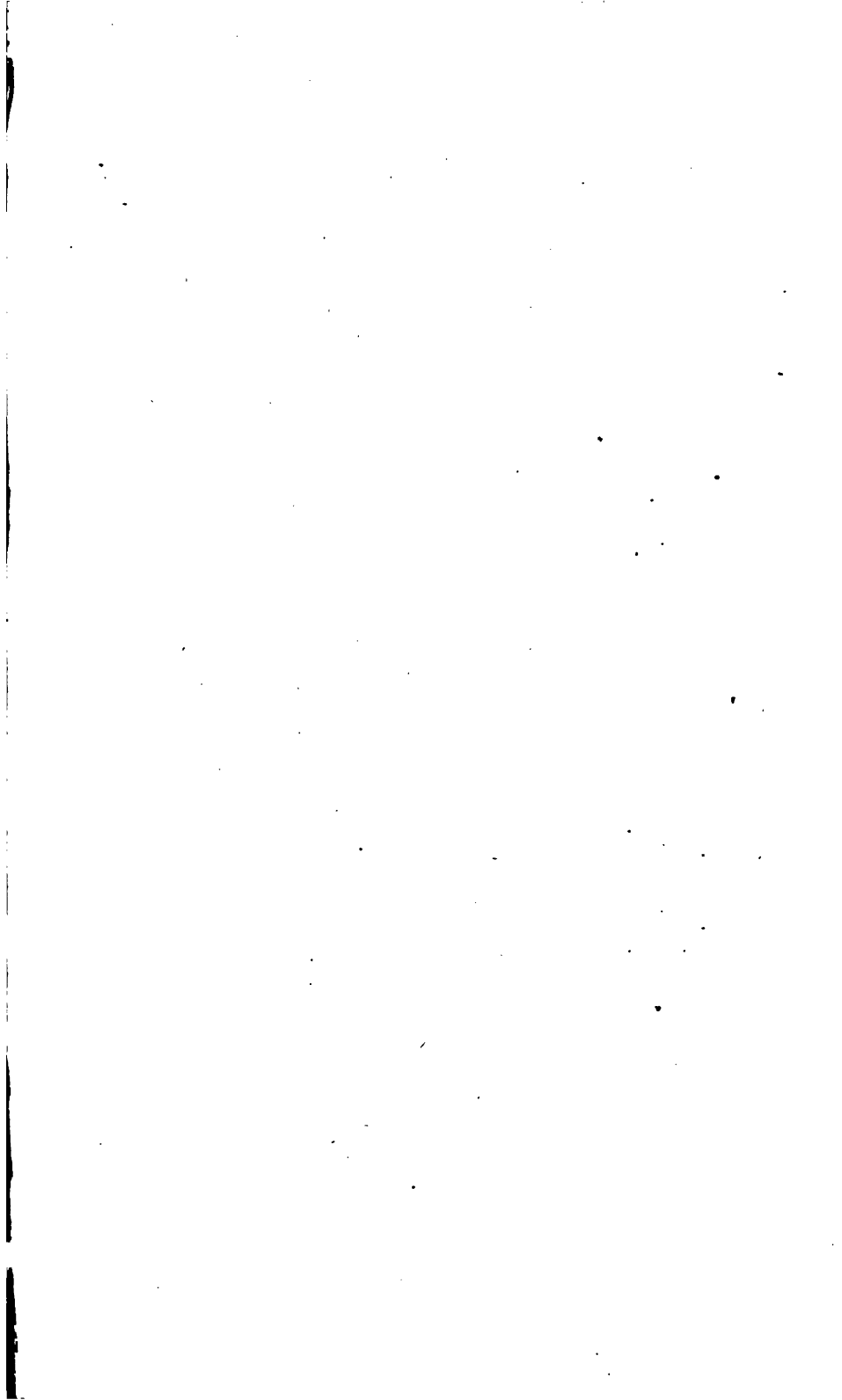
METEOROLOGICAL JOURNAL.

By Messrs. WILLIAM HARRIS and Co. 50, Holborn, London.

From November 20 to December 19, inclusive.

Day of Month.	Moon.	Rain gauge	THERM.			BAROM.		DeLue's Hygro.		WIND.		ATMOSPHERIC VARIATION.		
Nov														
20	○		45 48 36	30.05	30.09	61 60	S	SW	Fog				Cloud.	
21		'03	40 45 39	30.00	29.85	62 60	S	WSW	Fog			Rain	Cloud.	
22		'20	42 45 39	29.63	29.47	62 64	SW	SW	Fine			Rain		
23		'19	43 47 35	29.50	29.59	63 62	S	WSW	Rain				Fine	
24		'05	39 41 37	29.63	29.67	62 60	SE	ESE	Fog				Cloud	
25		'06	41 43 35	29.65	29.62	62 62	SSE	S	Cloudy					
26		'03	40 45 38	29.77	29.95	61 62	SE	E	Fine				Cloud.	
27	☾		42 45 40	29.94	30.02	61 61	E	E	Cloudy			Fine		
28			43 47 40	30.15	30.22	60 60	E	ENE	Cloudy			Fine		
29			42 45 37	30.26	30.30	61 59	E	NE	Cloudy					
30		'02	41 45 35	30.26	30.20	60 60	N	NW	Fine					
Dec														
1			39 45 35	30.16	30.07	60 60	NW	W	Fog			Cloud.		
2			37 45 36	30.05	30.10	60 60	W	WNW	Fog			Fine		
3			39 47 38	30.10	30.06	62 63	WSW	WNW	Rain				Cloud.	
4			40 47 39	30.00	30.01	63 65	W	WSW	Cloudy					
5	●		43 49 39	29.97	29.91	63 61	W	WNW	Fine					
6		'15	45 50 40	30.10	30.09	60 64	SE	E	Fog			Cloud		
7			46 56 40	30.05	30.11	63 65	W	W	Fog			d.		
8			48 52 45	30.16	30.21	61 62	W	W	Fine					
9			50 52 50	30.20	30.12	59 60	WSW	WSW	Cloudy			Fine		
10		'13	52 53 50	30.04	29.98	61 61	SW	WSW	Rain				Cloud.	
11			52 53 50	29.86	29.38	61 61	WSW	SW	Fine				Cloud.	
12	☾	'20	52 55 46	29.63	29.55	63 62	SW	SW	Rain					
13		'52	50 44 30	29.56	29.73	62 60	NNE	NE	Rain				Fine	
14		'13	32 35 36	29.88	29.95	56 57	NNE	ENE	Fine				Sleet	
15			50 33 30	30.00	29.88	57 53	SE	SE	Cloudy				Fine	
16		'09	52 35 35	29.73	29.60	57 60	SE	SE	Cloudy				Rain	
17		'14	35 38 40	29.68	29.93	61 63	E	E	Sn. & Ra.			Rain	Cloud.	
18		'05	43 48 41	30.10	30.20	65 65	SE	S	Cloudy			Rain	Fine	
19	○	'09	46 49 42	30.26	30.34	65 65	SSW	W	Rain				Cloud.	

The quantity of rain fallen in the month of November,
is 1 inch and 40-100ths.





Engraved for the London Med. & Phys. Journal & Published Feb'y 1. 1821 by J. Souter 23. St. Dun's Church Yard.

THE LONDON
Medical and Physical Journal.

2 OF VOL. XLV.]

FEBRUARY, 1821.

[NO. 264.]

NOTICE.

Another of the series of PRŒMIA to the several volumes of this Journal, (which commenced with that to the forty-third volume,) comprising a History of the Progress of Medicine and its auxiliary Sciences for the half-year immediately previous to the period of their production, respectively, was published on the last day of January. One of the especial intentions of those Prœmia, is to present a comprehensive view of the state and progress of Medicine throughout Europe generally, and in the United States of America; an object that cannot be effected in the regular monthly Numbers of the Journal, because of the small extent of space which can be there appropriated to this

TO SUBSCRIBERS.

The increased value of the back Numbers of this Journal, since the publication of the GENERAL INDEX to the FORTY VOLUMES, having caused so considerable a demand, as to oblige the Proprietors to reprint several of the early Numbers, they have determined upon a temporary reduction in the price of all the Numbers comprised in the forty Volumes, (being No. 1 to 238;) which will, therefore, be sold, during the next six months, at 1s. 6d. each: after which time, they can only be had at the usual price of 2s. 6d.

** * THE GENERAL INDEX to the London Medical and Physical Journal, from Volume 1 to 40 inclusive, comprising an Analytical Table of their Contents, arranged in Alphabetical Order, with references to the whole of the cited authorities under their Nominal Characters, &c. was lately published by JOHN SOUTER, 73, St. Paul's Church-yard.*

Persons finding any difficulty in procuring the progressive Numbers of the Journal, the PRŒMIA, or the GENERAL INDEX, will please to address a line to the Publisher, as above, which will ensure a removal of every difficulty.

and its effects fairly appreciated unmixed with other efforts: and especially so, as there was evidently an impaired state of visceral functions with the patient, which very possibly augmented and kept up the painful state of the breast and arm; and which an alterative and active purgative plan would then most probably have relieved. In this case, however, it will be seen that an alterative plan, with active purging and occasional emetics, was pursued for three weeks without the slightest diminution of pain, and without the smallest amendment of any one symptom; but, on the contrary, that immediate relief was obtained upon the very first application of pressure, and the most permanent benefits followed the persisting in its use.



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Original Communications, Select Observations, etc.

A Series of Cases, exhibiting the Effects of Pressure in Cancerous and other Diseases. By SAMUEL YOUNG, Esq. Surgeon to the Cancer Institution, Gerard-street, Soho; Member of the Royal College of Surgeons, and of the London Medical and Chirurgical Society, &c.

[Continued from page 31.]

THE next case to be reported (as noticed in the last published series of this Journal,) is taken from the minute-book of the Cancer Institution; and, although a case of very considerable standing, yet, as a great deal of aggravation had arisen from a comparatively recent injury, it was determined, in the first place, to try other probable remedies, for the relief at least of the increased state of pain, before the treatment by pressure should be adopted, so that the latter should be kept distinct, and its effects fairly appreciated unmixed with other efforts: and especially so, as there was evidently an impaired state of visceral functions with the patient, which very possibly augmented and kept up the painful state of the breast and arm; and which an alterative and active purgative plan would then most probably have relieved. In this case, however, it will be seen that an alterative plan, with active purging and occasional emetics, was pursued for three weeks without the slightest diminution of pain, and without the smallest amendment of any one symptom; but, on the contrary, that immediate relief was obtained upon the very first application of pressure, and the most permanent benefits followed the persisting in its use.

CASE.

June 10, 1819.—Lydia Elms, age 45; Grafton-street, Soho. Violent lancinating and gnawing pains of the whole left breast, but particularly at the nipple towards the inner side, and at the upper part; also about the shoulder and collar-bone. Great pain down the inner part of the arm, attended with swelling. No distinct tumor to be discovered, but an evidently diseased state of structure generally of the whole gland, particularly about the nipple, in the centre at the upper part of the breast, and the side leading to the axilla. Twenty-one years since, had a lump formed at the upper part of the breast, which came to matter and healed: then under the care of Mr. Birch, of Spring-gardens. After this, broke out several times, but ultimately healed. A slight degree of pain, with uneasiness, have ever since attended on the parts. Catamenia not subsided, though somewhat irregular. Health generally but very indifferent. Married, but never had any children.

June 11.—The great pain of the breast has existed for these three years; but, on the 25th of March last, on lifting a chest of drawers, the edge struck the breast, and all the symptoms since have been greatly aggravated; particularly the pain at the inner part of the breast by the nipple. *Pil. alterat. decoct. tarax. mist. purg.*

17.—The alterative pills have been taken every, and every other, night, and the purging medicine very freely used. The decoct. tarax. was rejected by the stomach, though persevered in. The symptoms in no one instance relieved. The medicines to be actively continued.

22.—The medicines have been pursued, but no benefit has yet appeared. The breast and arm still very painful; nights very bad. *Rep. med.*

27. No relief. The medicines to be pursued, with the addition of emetics.

30.—No relief as to the pain of the breast or arm; but a better night on the 29th. After taking the emetic, a large quantity of bile thrown up. The medicines to be continued.

July 4.—No alteration. The stomach rejecting the dandelion decoction, as well as the food. The patient was directed to suspend the breast firmly with handkerchiefs. A light infusion of camomile and cloves, with the alterative pills, were ordered. The dandelion decoction to be discontinued.

7.—The stomach somewhat improved; and the breast a little easier, from the application of the handkerchiefs. Pressure to the breast and side by roller and compress. The last-ordered medicine to be continued.

13.—Pressure in general application to the breast and side has been uniformly and actively kept up. The patient states

herself to be greatly relieved, indeed :—wonderfully so! to use her own expression. The pain down the arm particularly less ; and, since the application of the pressure to the breast, the fingers of the left hand have much more freedom and use, which before were so stiff as to render the hand incapable of holding any thing. The patient can now use her fingers tolerably well, even to hold needle-work ; before, she states her hand to have been perfectly useless.

Rest at nights. General health much improved. Pressure to be continued.

August.—The structure of the breast much reduced in its diseased thickness and morbid feel. Distinct schirrus round the nipple more perceptible. The pain in the arm subsided ; free use of the fingers ; and in every respect very much better. It was omitted by the patient to mention, when giving the history of her case, that, some three or four and twenty years ago, she had a distinct tumor in the same breast, (that is, previous to her going under the care of Mr. Birch,) and for which a removal by the knife was proposed by two surgeons in Buckinghamshire, where the patient then resided.

October.—The diseased state of the breast has greatly subsided. The patient has remained perfectly free from pain ; has a free use of the hand and arm. An improved state of health ; rest at nights ; and a more confirmed and comfortable feeling generally.

This case has been submitted from time to time, with others, to the inspection of those medical gentlemen who favoured the author by their attendance on his Lectures, as already noticed.

December.—The patient's health retains its improved condition. The breast and arm have remained easy. The former diseased structure of the breast very obscure and difficult of detection.

December 1820.—The breast has remained, from under pressure or any kind of treatment, quite easy for many months. The use of the hand and arm strong and natural. The patient's health occasionally in a disturbed state.

Within the last two months, Dr. Pett, of Clapton, had a professional opportunity of examining the state of the patient's breast ; and it may be well to remark, that I have since been informed by a gentleman, in whose family the patient lived as an upper servant, that twelve years back, from the diseased state of her breast, she was then incapable of raising her arm to her head ; and that the use of the whole limb was even then otherwise very much impaired.

Frequent notice has been taken of the treatment by pressure as a palliative, and it has been stated by myself, as well as

others, that, even taken in the light of a palliative in advanced cases, as a palliative merely in a state of hopelessness and disease, that the treatment by pressure will not only be found superior to any other known method of practice; but, in so confined a view even, its application and right treatment will be found a valuable accession in the removal of pain, the prevention of further mischiefs, and the melioration generally of that misery which the advanced stages of the disease so frequently present.

This, however, has been a general statement from experience generally, without being illustrated by any individual cases. The following case from the minute-book is therefore given as an example, and may be taken as a fair average representation of this department of the practice:—*i. e.* the important effects of pressure in hopeless or irrecoverable cases, as a palliative remedy merely.

CASE.

May 21, 1819.—Ann Whitnell, age 55, a full corpulent habit; Wardour-street. A large cancerous mass or tumor occupying the whole space of the formerly-situated right breast, as well as the axilla; immovable on the parts beneath, and extending considerably round the side, and, for several inches below in front, taking the sweep of the ribs. The largely involved and discoloured integument loaded with tubercles. The morbid mass itself ragged and tucked in, and shooting out a considerable fungus.

The history and circumstances of the case were minuted down, from the patient's statement, as follows:

In March 1816, first discovered a drawing-in of the nipple, and, upon examination, found a large and very hard lump beneath. A twelvemonth previous to this, had had a discharge from the nipple of the same breast, but took no further notice of it. Not the least pain attended up to this period, and the patient was never more surprised in her life (as she says) to discover such an unlooked-for alteration. Upon the advice of Mr. Senior, she immediately went the next day to consult Mr. Astley Cooper; who prescribed for her, and ordered a plaster of soap cerate, and the application of three leeches once every week. The patient applied the leeches only once, on account of the irritation they produced, and the difficulty of healing the sores occasioned by their bites. In a few days after she consulted Mr. Travers, who prescribed for her; and shortly after she went to Mr. George Young, who also prescribed for her; but strongly objected to any excision of the tumor. During this time the disease was on the increase, and in the month of July a second tumor had formed on the side of the breast towards the axilla, and for the space of a month previously the

whole had become very painful. The puckering of the breast considerably increased, and the nipple was quite lost. Although the parts had become much less movable, and the integument much more tightly stretched over the tumor, yet no discoloration of the skin existed.

The character of the pain was, sharp dartings and prickly shootings.

In the month of August following, (the disease and pain still on the increase,) she again consulted Mr. Travers; but any trial of the treatment by pressure, of which the patient had heard and proposed to submit to, being discountenanced from the first as useless, and (as represented) as producing insufferable pain, she submitted, and the parts were removed by the knife.

The wound after the operation healed in about three weeks; but the parts never returned to a natural state of feeling, and particularly under the arm much pain was experienced.

In about two months after the operation,—that is, in the October following,—a small white lump was observable in the cicatrix towards the side, which in the progress of eight weeks more became discoloured, and grew to the size of a large marble. (This tumor now is the size round of the large end of a hen's egg, and projects from the diseased mass about two inches or more, covered by a thin crimson cuticle.)

During this time the whole of the cicatrix became diseased and deeply puckered, particularly towards the side, the cavities of which were filled with small hard bodies. The growth of tumor, and advance of morbid alteration towards the clavicle, side, and axilla, now became evident, till it progressively arrived at its present formidable bulk and deeply and extended diseased state.

Within the last three months, the patient's health has become greatly impaired. Total loss of appetite, costive bowels, head-ach, with loss of flesh, and great weakness. Within this time also the right arm has become painful and enlarged, and at the present moment there is considerable swelling at the inner part of the arm above the elbow, and which the patient states is increasing. The patient was sent by me to Mr. Pearson for inspection. She was put for a few days on the alterative plan, and ordered to abstain from animal food.

May 25.—First application of pressure. Large commanding plaster-straps, compress, and roller. The compression produced, very considerable.

Up to the time of the application, the breast and side have continued very painful.

No inconvenience or additional pain experienced at the time the pressure was applied, and shortly after its application the

former pain was considerably less, and the patient became (as she terms it) very comfortable. The pills had acted on the bowels. To be continued, with the dandelion decoction, daily.

May 26.—The breast and side quite easy; but the patient did not pass a good night, which she attributes to the novelty of the bandages. The breathing easy, which formerly had become impaired.

27.—Passed a very good night. Breast, &c. easy.

28.—The night not so good. Breast, side, and arm, quite easy; bowels costive. Magnes. sulph.

29.—A good night. Breast and side easy. The pain and swelling of the arm considerably better; the breathing much improved. The bowels have operated, though sparingly. The applications to the breast have remained, without any alteration, very firm.

This day an additional roller with compress was used, with great firmness. No pain. The patient thinks her general health is certainly better. Medicines continued.

June 1.—Removal of the compress and rollers, but the plaster-straps not touched. Re-application of linen compress and two rollers. No pain whatever produced by the application: on the contrary, the patient expresses herself as quite comfortable as to her breast and side, which have remained free from pain. The arm she also states as much improved, particularly as to the pain, shootings, and soreness from the elbow to the arm-pit, which have now quite subsided.

The size of the arm appears to be reduced to its natural state. The patient's general health is very bad; her stomach particularly deranged, with total loss of appetite. On Saturday experienced a violent pain across the part; but it has not returned, and altogether she thinks herself better to-day.

4.—The whole of the applications removed. Decided improvement has taken place. The entire bulk of the tumor much reduced. The formerly tightly-stretched integument can be taken up in folds. The disease so much reduced at the upper angle of the breast towards the axilla, that the patient can freely move her arm forward; which formerly, she states, was quite impeded by the bulk of the tumor at this part. Her arm also, she reports as quite natural; the pain of which used to deprive her almost entirely of rest at night; and which, through the day, in consequence of its great pain and uneasiness previous to the applications of compressure, she was constantly supporting in various positions for the last four months. In this state of suffering, the greatest relief the patient experienced was sitting between two low chests of drawers, and supporting her arms upwards on their tops. From all this now, however, she is entirely relieved.

The integument of the side, and its tuberculated diseased state in front below the breast, the patient also, from frequent previous examinations, considers much looser and otherwise improved; and which, surgically, is the fact. The large fungus seems to be much in the same state as before the application of the pressure, except that the covering cuticle is certainly less discoloured and otherwise diseased.

The only complaint the patient has made, is from the itching produced at the back, where the ends of the plaster-straps are placed. This has much annoyed her at nights. Although free from pain, and her rest and sleep considerably improved, the patient's general health is certainly inconsiderably changed for the better. She says that she faithfully follows the plan laid down for her; that is, abstaining in particular from any irregular sort of living.

Medicines as usual. Very active pressure employed, by the re-application of long plaster-straps, compresses, and two six-yard rollers. No pain.

June 25.—Progressive improvement of the disease; but the patient's general state of health on the decline.

N.B. The author's own indisposition, which confined him to his bed shortly after this period, gave an interruption to the further treatment of this case: but he has since understood that the patient had a severe bilious fever, from which she recovered, and had afterwards called at the Institution; that she had left her house in Wardour-street, and was supposed to have gone into the country.

The facts of this case, however, sufficiently speak for themselves, in establishing the treatment by pressure as a superior beneficial remedy, viewed as a palliative merely, without dwelling further on its merits, confirmed as they are in every case, upon a fair trial and right application of the practice.

There is one point, perhaps, proper to notice, connected with this case; and that is, the representation to the patient of the pain produced by pressure, previous to the removal of the disease by the knife, and which led to that operation. Of the integrity of that representation, however contrary to the known facts of the treatment in question, I am fully convinced; for at this moment I have a lady under my care, for cancer, with an aggravation of the disease, similar to the last-reported case, where great, indeed unlooked-for, benefits are now being derived under the treatment of compression, who some time since underwent the removal of the breast by the knife, and where Mr. Travers operated. The tight cording and rubbing, &c. of the bandages during the after-treatment of this case by

Mr. Travers, the patient represents as intolerable, and to which, in a great measure, she attributes the nervous irritability she has since laboured under; and which, upon the return of the complaint, greatly prejudiced her against any trial of the treatment by compression, thinking that the same, or rather an aggravated, degree of suffering would necessarily be produced, till the disease had made considerable ravages, before she submitted to the present treatment.

But this tight cording has nothing to do with the right application of compression to any given part; and it only leads to what has been over and over observed, again and again repeated, by others as well as myself, for the last five years, that this cording or bandaging of the trunk by Mr. Travers, or rather the usual bandaging of the London hospital-practice, is not the *new* method of bandaging by which the *new* method of treatment by *compression* is effected; than which nothing can be more opposite the one to the other, and producing in application the most opposite results.

Of the old method of bandaging the trunk in rib and collar-bone fractures and other diseases, I may have occasion to speak at some future opportunity, when endeavouring to afford a further illustration of bandaging the body upon right and efficient principles. It is only to be observed here, that a practice so barbarous, so devoid of all principle, as the old method of bandaging, can never be applied to carry in effect the compression of breast-diseases without producing mischief: a fact which the experience of the last five years has but too amply and practically established; and therefore those who endeavour to treat cases by pressure upon the old system of bandaging, may well fail, and represent the treatment as useless and as producing insufferable pain. But now there is sufficient information before the public, and enough has surely been said on the subject, for such palpable errors to be avoided.

CASE.

Richard Fiest, age 33, (of whose case the annexed plate affords a general appearance, as it existed in the month of December, 1815, when he first came under my care.) In a written statement of his case, this patient states that the disease at first was a small speck between the eye and the nose, the skin of which by accident was broken, by being struck by his nail in washing. This accident happened in 1803. "For this," the patient states, "I placed myself under a surgeon; but, getting no better, I went to another, but to no purpose: it kept spreading until it surrounded the whole eye, attaching itself to the bone in three different places, and growing very large and very painful,

accompanied with almost constant bleeding,* and taking a course down the cheek until it contracted my mouth: all this time suffering without being informed what my complaint was, until told by Mr. Durbridge,† under whose care I then was, but without any success. From him, I was with Mr. M'Donald, Kent-road, all to no purpose, the disease still getting worse to worse. From him, to Mr. Astley Cooper for advice, who said, with an operation I might get well,—without it, never.”

The patient could not, or would not, make up his mind to the operation, and returned again into the country; and in this way twelve years of his life were occupied, till he came under the present treatment.

A list of the different medical gentlemen, five in number, with their residence, for the sake of accuracy and proof, is given by the patient in his written statement; but it will be quite sufficient to state the fact generally, in order to show the nature and inveteracy of the disease, that the patient, previous to his coming under the present treatment, had contended with this malady for upwards of ten years, under the almost constant care of five or six medical gentlemen; till, ruined and worn down, he made a last effort to that source where a fame has been so long and widely established in forlorn and desperate cases, and he applied to Mr. Astley Cooper.

Although the disease had still continued to advance since the patient's application to Mr. Cooper, and a removal of the disease by the knife appeared to me wholly impracticable at the time of his coming under my care, yet, as an operation had been named, I thought it but fair to the practice in question that the practicability or impracticability of any removal by the knife should be decided by other competent judges, before the treatment of pressure was adopted. For this purpose I sent the patient to St. George's Hospital, where I met Mr. Brodie and Mr. Ewbank, who declared, after examining the case, that, from the extent of the involved integument, a thought of any removal by the knife could not then for a moment be enter-

* First, in stating his case to me, has represented this bleeding as even copious at times. His trade was that of a boot and shoe maker, by which he (at Horsham) had brought up his father's family as well as his own, till totally ruined by the expense occasioned on seeking relief for the disease of his face and eye. He says he could not even work as a journeyman, from the pain and bleeding: that, when he attempted it, with his head necessarily stooping, the pain became intolerable, and the blood would run down in a gutter on the floor. The integrity and honesty of his character is vouched for by the most respectable people in his neighbourhood, who have taken a great interest in his case; and it was through some friends of the late Lady Evelyn's that he came recommended to me.

† The patient was told his complaint was cancerous.

tained: at this interview Drs. Pearson and Young were also present.

Although the disease, particularly at the upper part, and of the upper-lid especially, (which was entirely converted into a darkish-brown crustaceous mass, or hard and rather brittle structure in parts, not unlike the diseased growth in Lea's lip, whose case was published in 1815, in the "First Minutes,") protruded so as to obscure all vision on that side; yet, when the under-lid was drawn down, or separated from under the projecting mass, and which was not so changed by disease as the upper-lid, the cornea of the eye then became visible, and the patient could discern objects. Upon any such attempts, the part was subject to bleed largely. In an undisturbed state, however; all appearance of eye or eye-brow, or any trace of the external appearance of the parts, was quite lost; the disease filling up and projecting beyond the cavity of the bony orbit, as generally represented in the accompanying plate.

The disease largely sprang from the inner angle of the eye; was firmly attached to the frontal bone just above the centre of the orbit, as well as at the outer angle, to the temporal bone.

The under diseased lid was almost obscured by the upper part of the disease; and the parts beneath covering the orbit were so morbidly changed and thickened as to obscure any feel of the bone beneath. The integument of the cheek towards the nose was tuberculated, and the corner of the mouth drawn upwards.

On the 16th of December, 1815, pressure was commenced by compress and roller, the bulk of the disease being a sufficient pad of itself, with a small quantity of lint only to fill up inequalities, and afford a general covering for the roller to produce the necessary compression. In the course of a few weeks, a considerable sloughing of the general mass of the disease had taken place; and in two or three months, the parts were evolved, and the eye-brow could now be seen, with detached portions of disease along its course, and the greatly-diseased mass which had once been the upper eye-lid hung beneath.

The disease of the under-lid, as well as that on the cheek, was also improved. A hollow at this time being formed by the sloughing of the disease from the orbit, the pad of a tourniquet covered with lint was now used as a compress.

Under this mode of treatment the disease still diminished. The discharge most offensive and acrid. It excoriated every part of the face with which it came in contact, and entirely destroyed the leather covering of the tourniquet pad in the course of a day or two. The sheet-lead also, which was afterwards used as a case to the pad and to enlarge the compress, was so acted upon by the discharge as to be reduced into small crumbling portions.

In the Easter following, his wife came to town to see the patient, and her first observation was, that "one side of his face was as long as the other." That is, the tuberculated disease of the cheek had been entirely removed by the pressure, and that side of the face, which had formerly been shortened, and the corner of the mouth drawn up in consequence, was now let down to its proper situation and length.

Some of these tubercles of the cheek sloughed out precisely the same as in the case of tuberculated integument in the cancerous female breast, when under similar treatment.

In the month of December, 1816, the disease of the upper eye-lid entirely came away; that is, the whole sweep of the former lid was removed, having only a few lines of diseased integument under the eye-brow, and which came in contact with the eye-ball. In this case, the eye, so far from projecting, was naturally sunk deep into the orbit. There was conjunctival disease at the inner angle.

The removal of the projecting disease, and ultimately of the disease of the upper eye-lid, afforded an opportunity of examining the state of the orbit itself; and the disease was found casing the cavity, as far as the examination of the finger and thumb could go. Under pressure, these diseased portions became detached from the bone; felt like cartilaginous plates of the thickness of a crown-piece, and ultimately disappeared under the pressure, the whole cavity of the orbit being filled up by graduated compress for the purpose. At the outer angle, disease still filled up the space between the eye and the orbit, so that the edge of the bone could not be felt. The fungous disease at the inner angle, the consistence of which was that of a gristly or cartilaginous flesh, had sloughed down, leaving a large cavity which took the first joint of the thumb to carry lint down to the bottom as a compress; the whole of the cavity, of course, being always so filled up.

The very thick fleshy feel of the under eye lid and integument filling up the under portion of the orbit, had greatly thinned away; but, on pulling down the under-lid, two or three crescent-like fleshy funguses were seen projecting upwards.

The integument of the cheek at the lower part had entirely recovered itself from the disease, and both in appearance and feel was quite natural.

After considerable removal of the disease had been effected, the patient was sent for the satisfaction of Mr. Astley Cooper's inspection; who stated, in the note with which he favoured the author in return, "that the disease was greatly improved, though a good deal of the old leven yet remained."

In the spring of 1817, the patient was unfortunately attacked by severe inflammation of the conjunctiva of the diseased

eye,—a complaint very prevalent at the time. In the course of twenty-four hours, the conjunctiva, from being perfectly flat and white on the ball of the eye, rose considerably above the cornea, presenting a thick red ring, as if the eye had been set in red sealing-wax.

A complaint so sudden was considered entirely unconnected with the old morbid organization of the surrounding parts, and was treated accordingly. Pressure was suspended, and the complaint combatted in the usual way. The annular enlargement of the conjunctiva (which, totally distinct from the fungous production of the old disease, had a natural feel, and yielded to the edge of the lancet,) was freely scarified; lotions, with the opiate-wine, employed, as well as the wine itself in drops to the eye; and purgatives, emetics, with alteratives, were given.

The scarification of the conjunctiva did not seem to produce any good effect, but rather the contrary; and the patient always felt better, and the appearance of the eye was certainly improved, when the part was placed under general compression, the applications being kept damped by the lead and opiate wash. The attack gradually subsided; lotions of sulphat of copper were used, and subsequently the concentrated acetate of lead; but, though reduced, the annular enlargement of the conjunctiva did not entirely subside. The treatment by pressure was actively resumed against the remaining original disease.

The formidable portion of the disease at the inner angle had now given way; the deep cavity had filled up with healthy granulations; and skin was formed along the margin, as well as upwards from the sore partly along the nose, till the whole was quite healed, and presented an healthy sheet of skin close in to the very corner of the eye, the concavity of the part being perfectly natural.

In the autumn of 1818, the patient was considered as nearly recovered. The pressure had been discontinued frequently, to try the integrity of the parts either recently restored, or where disease had otherwise been removed. He had also occasionally returned to his home in the country during the treatment; and throughout had frequently been examined by Mr. Brodie and Mr. Ewbank, who had seen the disease before the commencement of the compression. A number of other professional gentlemen, from various parts of the country, had also seen the case during different periods of its treatment.

It may be proper here to remark, that, throughout the treatment, when general compression was employed to the part, neither uneasiness nor pain were produced, but rather the contrary: but, when the pressure was applied specifically to any point or angle, then the uneasiness and pain were fre-

quently considerable; and a blowing sensation, like bellows; on these occasions was produced in the ears. The discharge, after the great sloughing of the disease *en masse* had subsided, became purulent, and lost its offensiveness. But, during the treatment, the same effluvia and discharge peculiar to the disease occurred at any time when a part, placed under specific pressure for the purpose, occasionally sloughed.

In the month of October of 1818, after the author had been absent for several weeks from town, he had the mortification to find that, from neglect and suspension of all pressure whatever, a considerable growth of disease had taken place. A firm fleshy substance projected from the centre of the eye at the under part, obscuring the ball; and at the outer angle a fig-like fungus, of an inch in diameter, and reaching down to the corner of the cheek-bone, with a broad and firmly-attached base, had projected itself.

Upon more minute examination, however, all the part at the inner angle, as well as the sweep of the eye-brow and other material points which had been formerly removed and healed, remained perfectly sound, and to all appearance quite healthy. This circumstance gave a hope that the disease at length might ultimately be conquered, or at least reduced to a very manageable state: and, with this hope, pressure was again actively employed to the new disease that had presented itself. This stage of the disease was frequently seen by Mr. Tegart, jun. of Pall Mall.

Under considerable interruptions of the treatment, the new disease was at length removed; but at the outer angle there was evidently disease filling up the space between the eye-ball and the bone. Pressure was actively and specifically thrown on this part; and the compress, from its shape, pressing on the edge of the bone, very considerable pain at times was occasioned.

During the author's Lectures in July and September in 1819, this case also came under the examination of Messrs. Haden and Leman, Dr. Armstrong, &c. The disease at the outer angle was not then perfectly cleared away; and the serrated edge of the integument under the eye-brow, and immediately in contact with the eye-ball, had rather a suspicious appearance. This part, trifling however in extent, and scarcely the thickness of a wafer, was constantly in a state of friction and moisture; and, from its situation, being immediately under the eye-brow within the orbit, and especially from its thinness, could not without difficulty be compressed. The annular enlargement of the conjunctiva remained as it had been during the last two years and a half, and formed for the eye a sort of lid at the upper part, against which the small portion of integument just

described under the eye-brow was constantly rubbing, upon any motion of the eye, although to the patient it produced no sensible irritation or any other effect. For some months, pressure had only been employed at the outer angle, and that too under various interruptions. All the other parts remained healthy.

Under pressure, at length, the obstinate—or rather, from circumstance of situation, the hidden and defended—disease at the outer angle and interior of the orbit was perfectly cleared away; the bone was quite distinct, and the finger could be passed down between it and the ball of the eye.

Thus this man, with a numerous family, and exposed to extreme difficulties, pursued the treatment by compression, under a variety of interruptions and disheartening circumstances, through a space of four years, till he had conquered a disease which had engrossed seventeen years of his life. It may be right to observe, that, in a few months after the treatment was commenced, the patient was able to work at his trade, from which he had been wholly incapacitated by the disease for so many years previously.

In June, 1820, the patient, with his family, returned into the country; all pressure having been suspended between six and seven months.

In about three months afterwards, I was gratified to hear a satisfactory account of the patient, from a gentleman (Mr. Henry Tredcroft) resident in the neighbourhood, and who had taken great interest in the case from the beginning. In his letter of thanks to me on this occasion, he “regrets he could not get poor Fiest earlier under my care, as the cure would have been sooner effected.” He also states, “I am sure it will add to your satisfaction to know that your kind attention has been bestowed on a person who well deserved it. His good character, added to his misfortune, will always secure from me an interest in his behalf. I shall at all times be ready and most happy to bear testimony to the ability and feeling displayed towards this poor sufferer, if ever I am called upon. I have seen more than one poor person in the last stage of this dreadful disease. I have seen their agony, and therefore feel particularly for those who labour under this complaint. Fiest is now living comfortably with his family; for which he is entirely indebted to you, &c.”

It may be well to observe, in a practical point of view, that no bone, at least none that could be detected, exfoliated during the sloughing in Fiest's case: whereas, in Johnston's case, on the contrary, (the first reported in the present series,) a large portion of the bones of the eye and upper jaw was destroyed.

But it may also be proper to add, that Johnston's case was a disease that sprang from the bone, at least from the periosteum: whereas, in Fiest's case, the disease commenced in the soft parts, and only in progress became attached to the bones. In Johnston's case there was no original disease of the integument, the parts only becoming distended by the growth of the under-tumor. In Fiest's case, on the contrary, the integument was wholly and originally converted and involved by the morbid growth, bearing every characteristic of the cancerous change; yet in this case there was no enlarged absorbent gland to be found in the neighbourhood of the disease, though five times the standing of Johnston's case, who had no one cancerous symptom of the integument, but where a cluster of enlarged glands did exist under the ear, at the angle of the jaw.

The deep-seatedness, as well as proximity, of the disease in Johnston's case, leaves little difficulty in the mind to conceive how the glands should become excited, if not ultimately involved in the same disease, although the patient had not been exposed, as he was, to severe cold and catarrhal affection: while in Fiest's case, on the contrary, it was a disease more of surface and projection, and distant from the seat of such glandular affection.

[To be continued.]

Observations respecting the Nature and Origin of the common Species of Disorder of the Spine: with Critical Remarks on the Opinions of former Writers on this Disease. By EDWARD HARRISON, M.D. formerly President of the Royal Medical and Royal Physical Societies of Edinburgh, &c.

OF all the maladies by which mankind are assailed, none is more afflictive and distressing to relatives, or more deplorable and mortifying to the sufferers themselves, than spinal distortion. Persons so affected become objects of neglect or aversion; and, being rendered unfit to discharge with propriety the several duties of life, they remain burthens upon their friends. To a generous mind, nothing can be more painful and humiliating than to feel that in him the chief purposes of existence have been defeated, and that he is doomed to consume his days in wearisome inactivity, or in unproductive occupations.

That such must be the necessary consequences of this disease will be evident, if we take a survey of the important offices of the spine, and of its influence over the complicated functions of the thoracic, abdominal, and pelvic viscera. Curvatures in the cervical bones produce numbness and paralysis of the arms. Injuries received in any part of the spine induce loss of sensation, and of power of voluntary motion in the lower limbs.

Different internal organs are made to participate, according as the malady is seated higher or lower in the back. Pressure from dislocation of the cervical vertebræ impedes deglutition and respiration, by reducing the diameter of the œsophagus and trachea, as well as by disturbing the actions of their muscles and nerves. Diminished capacity of the chest, the unavoidable consequence of its deformity, gives rise, by displacing or squeezing the enclosed parts together, and preventing the convenient exercise of their functions, to palpitations of the heart, to dyspnœa, cough, and various distressing perceptions, which are followed, in process of time, by organic derangements of the heart and lungs, by consumption, asthma, hydrothorax, and other chronic disorders. When the luxated dorsal vertebræ compress the spinal cord, there is generally a constrictive uneasiness about the stomach, and a depraved secretion in some of the abdominal organs. Bony distortions of the thorax mechanically force the liver, the stomach, and the contiguous viscera, out of their natural places: hence arise painful sensations in them, and irregularity in their functions. The impediments thus created are followed by organic mischief in the parts, which terminates at length in the most distressing complaints. A displacement of the lumbar and sacral bones leads, in the same way, to very obstinate affections of the bladder, rectum, uterus, and external genital members.

Till the true nature of the disorder has been discovered in these cases, the most discordant opinions are advanced by the faculty, and the patients are tortured by the most opposite practices. A malady which is attended with the most disgusting appearances, and produces such a variety of obstinate symptoms, cannot be too soon or too carefully resisted. A vigorous opposition to the very *dawn* of the complaint is of greater consequence, because no effectual means have hitherto been attempted to remove the deformity, arrest its progress, or avert the maladies which proceed from it. In order to show the truth of these remarks, and prepare the reader for a more successful method of cure, I propose to give a short account of the principal writers who have delivered their sentiments upon this disorder, their respective opinions, and the different practices which they have employed to subdue it.

Society is under great obligations to the late Mr. POTT, for calling the attention of his brethren to an investigation of the causes and treatment of one of the most obstinate and distressing complaints to which the human frame is exposed. This subject, though much agitated in the early ages of mankind,* had been unaccountably neglected in modern times. Mr. Pott

* See Galen, Avicenna, &c.

brought it again before the public, in his valuable remarks on Palsy of the Lower Limbs.* So much deference was paid to his appeal, that he had the satisfaction immediately to succeed in rescuing several miserable victims out of the hands of unfeeling and rapacious quacks, who had till then ingrossed to themselves the whole care and management of these unfortunate sufferers. The eminent station of this accomplished surgeon enabled him to treat many patients, and enrich his Remarks with several dissections of persons who had sunk under the complaint.

"The true cause of the disease is," he observes, "a morbid state of the spine, and of some of the parts connected with it; which distempered state of parts will, upon careful inquiry, be always found to have preceded the deformity some length of time." "A morbid state of parts previous to deformity, caries, or curve, must be allowed: every complaint of the living, and every appearance of the dead, prove it beyond contradiction or doubt. All the general complaints of persons afflicted with this disorder will always, upon careful inquiry, be found to have preceded any degree of deformity, to have increased as the curve became apparent, and to have decreased as the means used for relief took place. The pain and tightness about the stomach, the indigestion, the want of appetite, the disturbed sleep, &c. gradually disappear, and the marks of returning health become observable, before the limbs recover the smallest degree of their power of moving."

"That this curvature, which is always from within outward, is caused by the erosion or destruction of part of the body, or bodies, of one or more of the vertebræ; by which means, that immediately above the distemper, and that immediately below it, are brought nearer to each other than they should be; the body of the patient bends forward, the spine is curved from within outward, and the tuberosity appears behind, occasioned by the protrusion of the spinal processes of the distempered vertebræ."

"That, without this erosive destruction of the bodies of the vertebræ, there can be no curvature of the kind which I am speaking of,—or, in other words, that erosion is the *sine qua non* of this disease; that, although there can be no true curve without caries, yet there is, and that not unfrequently, caries without curve."†

These passages might be confirmed by reference to several others. Having extracted them from his last essay on Spinal Maladies, it is reasonable to conclude that they contain a faith-

* *Chirurgical Works of Percival Pott, Esq.* vol. iii.

† Pott on the Palsy, &c. vol. iii. pp. 427, 457, 486, 493.

ful record of his latest sentiments on the subject. Had any subsequent change of opinion taken place, it would doubtless have been recorded in the "Notes and Observations" of his respectable editor and relative, Sir James Earle.

It appears from these extracts, and from the spirit of Mr. Pott's writings, that, in his estimation, caries formed a necessary and essential part of this formidable malady. We shall take another opportunity to prove that paraplegia can appear in its most complete form, independently of caries; and it is equally certain that caries of the vertebræ* is frequently unaccompanied with paraplegia.

We are instructed in the foregoing passages, that spinal complaints affect three different textures,—the bones, ligaments, and cartilages. The last are brought to participate during the progress, and cannot therefore be allowed any share in the original formation. Though Mr. Pott always found disease in the "ligaments,"† and "sometimes in them without any apparent affection of the bones,"‡ he unaccountably overlooked them; and limited his curative indications to the morbid state of the bones. In so doing, he reasoned upon a narrow basis. Admitting his own statement in its full extent, I think we are still bound to pay some regard to the ligaments. It however appears, from later dissections, that the vertebral bones are not always enlarged, or otherwise disordered, even when they are accompanied with a greater or less degree of deformity and crookedness of the spine.§ Many skeletons, showing lateral, posterior, and anterior curvatures, may be seen in the anatomical museums; several of them so exceedingly misshapen, as to leave no doubt of their having occasioned death, by interfering with the functions of the spinal marrow, the thoracic and abdominal organs. In some, the vertebral bones and cartilages show no other signs of disease than such as arise from unequal pressure: they are hard, firm, sound, and, where free from pressure, are of the natural size. In order to form a curvature, the bones and cartilages must necessarily be fuller on the outside and compressed on the inside. The bones are even unnaturally separated in some parts of the external bow, yet nothing like ulceration had taken place.

According to this view of the subject, we must direct our attention to some other tissue to discover the true cause of spinal complaints; and I am of opinion that we shall find it in the connecting ligaments, "which seem to have lost part of their power of holding the bones together."|| These get relaxed,

* Lientaud, Morgagni, *Lond. Med. Observat.*, De Hæn, Stork, Bacchius, Lécat, Aubertus, Blanchard, Hildanus.

† Pott, vol. iii. p. 427, 428, 432.

§ Vol. iii. p. 483.

‡ Vol. iii. p. 482.

|| P. 438.

and suffer a single vertebra to become slightly displaced. The column now losing its natural firmness, other bones begin to press unduly upon the surrounding ligaments; they, in turn, get relaxed and elongated, by which the dislocation is increased and the distortion permanently established. The direction becomes lateral, anterior, or posterior, according to circumstances; but the malady has in every instance the same origin, and requires the same mode of cure.

It appears to me that, when the spinal trunk gets too infirm to maintain the weight of the head and shoulders, the distortion inclines to the lateral form, being encouraged to take that direction from the resistance given by the superior spinal ligament. The anterior or internal curve, as far as my experience extends, has always been confined to the lower cervical and upper dorsal, or to the lumbar, vertebra. The spine is depressed in both instances, appearing to have sunk under and been crushed downwards by an oppressive and overwhelming burden. When it arises, in a weakly constitution, from the exertion of pulling, lifting, or carrying, the protrusion, from the force employed, is nearly backward; and thus is laid the foundation of the posterior or outward curve. This is the simple and real origin of our spinal deformities, except in some rare instances, where they begin in the vertebral or cartilaginous structure. On this principle, we can easily understand why people so often recover from spinal affections, and carry about with them the visible effects for many years. If the primary mischief begun in bone, as Mr. Pott teaches, or in cartilage, according to others, I think the affection would be very seldom cured, either by the unaided efforts of nature or the best assistance of the healing art. "No degree of benefit or relief, nor any the smallest tendency towards a cure, is to be expected until the caries be stopped and the rotten bones have begun to incarn: the larger the quantity of bones concerned, and the greater degree of waste and havoc committed by the caries, the greater must be the length of time required for the correction of it, and for restoring to a sound state so large a quantity of distempered parts."* When any thing like this degree of caries and rottenness occurs in the living body, the matter generated can never be absorbed. In defiance of the best efforts of the constitution, it will most certainly burst forth and lay the foundation of very tedious, if not fatal, drains from the back, groin, thighs, or hips.

The practice of Mr. Pott arose, by an obvious induction, from his own premises. His object was to excite inflammatory action by caustic issues, and thereby induce anchylosis, or

* Pott, vol. iii. p. 474.

union among the morbid bones. That such treatment is useful at an advanced period, after caries is actually formed, may be agreeable to sound practice, though it has never obtained the universal approbation of medical men. Professor RUST* remarks, that numerous observations and long experience have proved to him that issues rarely produce the desired effect, and that they even frequently accelerate the progress of the disease in a late stage. At an early period, while the disorder is confined to ligament alone, the practice of Mr. Pott is highly objectionable, because it prevents the application of other modes better calculated to restore the sufferer to his natural figure and former health. It cannot be denied, nor do I wish to insinuate, that patients have not got well upon this plan. Caustics, by stimulating, encourage the muscles and ligaments to act more energetically, by which they sooner regain their lost tone and vigour. The curative process is further expedited by the rest to which invalids must to a certain degree submit, while smarting under the pain of caustics. In many cases, Mr. Pott found it necessary to do more than employ caustic issues: he actually confined his patients to bed or to an horizontal situation, during the greatest part of the cure, as they could not bear to remain in an upright position.† In all these recoveries, the subjects of them remain through life in puny health, because, the bones continuing displaced, and some of the viscera being necessarily subjected to injurious pressure, the important functions of the spinal cord are imperfectly discharged, owing to the difficulties it meets with from the altered form and direction of the medullary canal.

“The deformity remaining after recovery is subject to great uncertainty and considerable variety, as it depends on the degree of caries and the number of bones affected: in general, it may be said, that where one vertebra only is affected, and the patient young, the curve will in length of time almost totally disappear; but, where two or three are affected, this cannot be expected. The thing aimed at is the consolidation and union of the bones, which had been carious and are now become sound. This is the *sine qua non* of the cure, and this must, in such cases, render the curvature, and consequently the deformity, permanent. The issues will restore the use of the limbs, but not the lost figure of the spine.”

“That, when two or more vertebræ are affected, forming a large curve, however perfect the success may be with regard to the restoration of health and limbs, yet the curvature will, and must, remain, in consequence of the union of the bones with each other.‡

* Professor Rust's *Arthrocacology*.

† *Observations on the Cure of the Curved Spine, &c.* by James Earle, Esq. p. 28.

‡ Pott, vol. iii. p. 478, 491.

It is clearly apparent, from the foregoing quotations, and the candid admissions of Mr. Pott, that he never attempted to restore the back to its natural figure. Such an happy result never came under contemplation; nor could it, consistently with his ideas, form part of the curative plan. Believing, as he did, that the mischief always begun in the bodies of the vertebræ, his efforts were confined to prevent ulceration, to cure it when present, and to join the bones permanently together in their curved form by an immovable union.

Had Mr. Pott attended carefully to the incipient stage, it is more than probable he would have found that the affection generally begins in the ligaments, and passes from them to the bones and cartilages. It is of the greatest importance to keep this distinction constantly in view, because the knowledge of it leads to a marked difference in practice. At this period, the complaint, according to my experience, is more certainly curable than many others; and, what is most encouraging, *the spinal column is made wholly to recover its natural form and powers*. Instead, therefore, of sustaining the miseries of an infirm constitution for the rest of life, and carrying deformity into all companies, the individual returns into society in as perfect health and shape as if he had never been afflicted.* That such has been the pleasing result of the new method, is abundantly proved by experience, and can be confirmed by many living witnesses. Had the disorder, in any of these examples, been attended with caries, very different must have been the result. The complaint, instead of submitting to the means employed, would either have gradually increased, or, had a cure been at last obtained, there would, for obvious reasons, have been no reduction in the tuberosity. The inevitable consequences must have been a mis-shapen form, and its attendant evils.

A cure conducted agreeably to the foregoing principles leaves the patient in good health, and in the enjoyment of his natural shape. Where it has been produced by issues, how different is the result! In such instances, the curvature, and consequently the deformity, remain permanent.

"These observations,"† says Sir James Earle, "and others of the same tendency, added to the many melancholy instances which I have seen after a cure has been effected by issues alone, had long obtruded unpleasantly on my recollection. Prolonging life merely to lengthen out a miserable existence, and enable a wretched being to crawl a little longer on the earth, appears very unsatisfactory, and stopping short, if the idea be

* This subject will be prosecuted in successive Essays, till the author shall have fully explained his ideas of the disorder and its treatment.

† *Observations on the Cure of the Curved Spine, &c.*

indulged, that, by any assistance from art, more may be done. I have often thought it would be a most happy circumstance could we go a step further, and *cure the deformity so well as the disease*; for, besides the disgusting appearance of the crookedness which remained, want of health, debility and inactivity, usually accompanied it: and another very material consequence resulted from it,—namely, that the largeness of the remaining curvature rendered the spine mechanically weak; which, probably, being added to a tendency to the same softness of bones as was the foundation of the original malady, was the efficient cause of the disease being liable to return. This was a very strong and additional reason why the improvement of the form of the spine should be equally the object of our attention with the cure of the disease.”

It does not excite surprise that Mr. Pott, chiefly conversant with the worst and most obstinate cases, was led to believe that the disorder always began in the bones. These were, as he thought, enlarged, and pressed upon the cord. Having been induced, for reasons which he has not fully explained, to abandon this opinion, Mr. Pott did not afterwards attempt to account for loss of motion in the limbs. He contented himself with asserting that it bore no resemblance to the true paralysis of the same parts. Sir J. Earle, in his anxiety to supply the defect, ventured to go no farther than to revive the discarded opinion of his preceptor. I mention this circumstance as a proof that little progress had been made in the pathology or treatment: the great authority of the master seemed for a time to over-awe and bear down opposition.

One of our latest writers imputes the disorder to pressure from an inflammatory increase of bulk in the fibro-ligamentous substances interposed between the vertebræ. He was induced to give up the opinion of Mr. Pott, because bony enlargements of the spine are seldom found in the dead body.* We have little difficulty in joining our testimony to that of Mr. Copeland, in cases of external violence. It is, however, uncertain whether in these instances the inflammation commences in bone, cartilage, or membrane. The species to which I wish to draw attention, makes its attack so insidiously as not to be discovered till a long time after its invasion. It exhibits none of the local characters of inflammation, nor is it accompanied with pyrexia. This, which is the common variety, seizes upon relaxed fibres, or persons accidentally debilitated. I am enabled, from the treatment of many such cases, to assert that they are wholly unconnected with inflammatory diathesis, and very seldom originate in a morbid condition of the vertebræ.

* Copeland on Diseased Spine. Gortz de Morbis Ligamentorum.

In order to understand the true origin and injurious effects of these disorders, we must premise a cursory survey of the operations which the spinal column and its contents are destined to perform in the living animal. The nine or ten pairs of nerves which issue immediately from the brain, seem to be chiefly occupied with conveying perceptions to the sensorium commune. The thirty-nine or forty pairs which arise out of the spinal marrow, communicate the power of feeling and voluntary motion to the limbs and muscles of the trunk and head. They, in conjunction with the ganglions and great sympathetic nerve, supply the sanguiferous system, the thoracic and abdominal viscera, with the principal share of their nervous influence. By experiments performed upon living animals, we learn that these several organs continue to execute their appropriate functions long enough after all connection with the brain has been destroyed by decapitation, to induce us to believe that they derive their predominant energy from a different source. This seems to be the spinal marrow; for, when it is removed, the abdominal and thoracic organs soon cease to act. We are therefore led to conclude, that, although the animal functions are performed by the brainular nerves, those of organic life, or such as are only intended to support the machine, are executed by the spinal nerves. In this view of the subject, a healthy disposition of the spine is indispensable to the well-being of the individual. That the inference rests upon experience we shall be able to prove, by taking a review of the principal symptoms which occur in this disorder. When it affects the superior cervical bones, deglutition is impaired, from pressure made upon the cervical nerves in their course to the pharynx and œsophagus. For the same reason, a dry teasing cough and difficult breathing are occasioned by pressure upon the nerves in their way to the larynx and trachea.

Projections in the cervical bones affect the arms with numbness, debility, spasmodic twitchings, and paralysis. They also produce uneasiness in respiration, with palpitations from slight causes. In the dorsal vertebrae, they induce a girding sensation over the stomach, as if it were tied with cords. There is also indigestion, and oftentimes vitiated appetite. The secretion of bile becomes diminished, the countenance looks sallow, and the patient labours under many symptoms of jaundice. The belly is obstinately constipated, and the rectum refuses its office. The feces are often slimy, whitish, or clay-coloured. He gets feverish, is restless, emaciated, and affected with many symptoms, which resemble consumption. The kidneys secrete little urine, and the bladder loses its expulsive faculty. In whatever part of the back the curvature is situated, the lower limbs are apt to be affected. Slight pressure upon the spinal

cord produces debility and fatigue in walking. The legs cross each other; the patient is liable to stumble, and cannot go straight to any point. Greater pressure occasions muscular spasms, numbness, restlessness, and clammy sweats. A still greater pressure produces inability of motion, and complete paralysis of the limbs.

The numerous, obstinate, and complicated maladies, enumerated above, are truly appalling. They exhibit spinal complaints in a new light, and claim for them an attention which they have not hitherto received. Whether we consider them as giving an unseemly appearance and laying the foundation of delicate health, or regard them as the fruitful parent of many other formidable disorders, the subject is worthy of the most rigorous investigation. Happy will it be for our contemporaries and successors, if a complaint almost universally prevalent in the middle and higher classes can be subdued so as to leave behind it no vestige of its former existence. In order to accomplish the object, we must endeavour to make out the cause by tracing the different and apparently-unconnected symptoms to their true source. Perplexing as the inquiry may seem at first sight, we are inclined to believe that it admits of explanation upon anatomical principles. In the natural form of the back, the nerves meet with no impediment in their passage from the cord through their proper foramina in the several vertebræ. Distortion of the spine produces an alteration in the canal under it, and the vertebral holes are necessarily forced into unnatural directions. In consequence of these changes, the tender substance of the cord gets pressed against the hard sides of its sheath. The nervous filaments which issue from it, having to travel over a longer course, become unduly stretched, and encounter angles and projections in their way through the bony holes of the vertebræ, by which their energies are impaired, interrupted, and morbidly affected. We have already shown that the spinal nerves have been traced to all the abdominal and thoracic organs, to the sanguiferous system, to the upper and lower extremities; also to the trunk of the body, the neck, and outside of the head. When we contemplate this distribution upon anatomical and pathological principles, we cease to be surprised that luxations of the vertebræ should produce such an endless train and succession of perplexing symptoms. The extensive range of the spinal nerves, and momentous offices they are destined to perform in the animal fabric, afford an easy solution of our difficulties. Not that all the symptoms and sufferings enumerated were ever encountered by any individual.

It has been already declared that the symptoms vary according to the situation of the curve. Organs supplied with nerves

from a higher part of the spine than the seat of disease, are seldom affected. In slight cases of pressure, even the thoracic and abdominal members, which derive their nervous influence from below the distortion, are but little disturbed. The symptoms, in all these instances, are chiefly confined to the lower extremities and the viscera which receive their nerves immediately from the spinal curvature. In order more clearly to explain this part of our subject, we must premise, that the neck consists of seven flexible vertebræ; the back contains twelve, and the loins five. From each of these vertebræ issues a pair or bundle of nerves; one portion of which diverges to the right, another towards the left side, and the remainder run into the body. From the os sacrum arise five more pairs of nerves. It follows from this calculation, that thirty-nine pairs, or seventy-eight nervous cords, each including many distinct filaments, originate in the spinal marrow. Having passed through as many proper holes in the vertebræ, they separate into minute fibres, and are distributed over the greater part of the body, to regulate and control the most useful and necessary functions of animate beings. When any of the vertebræ become displaced or too prominent, the patient experiences inconvenience from a local derangement in the nerves of the part. He, in consequence, is tormented with a train of nervous symptoms, which are as obscure in their origin as they are stubborn in their nature: they have therefore been justly denominated the *opprobria medicorum*. A sedulous examination into morbid anatomy having enabled us to disclose the latent sources of other ailments formerly concealed in impenetrable darkness, we may look forward with confidence to a similar result with regard to several nervous complaints, by directing inquiry to the spinal column and its delicate contents.

According to this view of the subject, the obvious indication for the cure of spinal affections consists in restoring the displaced bones to their natural situations, that the spinal cord and its nerves, relieved from injurious pressure and disturbance, may be re-instated in their former abilities. In all the cases hitherto treated agreeably to these principles, the success has been complete. The affected organs to which they run, being no longer under the influence of diseased nerves, gradually recover their healthy state and proper functions.

Mr. BAYNTON, having been frequently disappointed in the cure of these complaints by treating them according to the ordinary modes, was led to devise another course. He has favoured us with his practice in twelve spinal cases: of these, one died; eleven recovered and regained the use of their limbs. The process took up from seven to fifteen months, and the projection was reduced in every instance; in some it is asserted

to have been removed. He imputes the want of success, where his treatment failed, to previous ankylosis of the displaced bones. These cases, partly drawn up by himself, and partly by unprofessional friends, are not sufficiently detailed and circumstantial to enable us to form a clear and satisfactory opinion either in regard to the process adopted, its success in reducing the enlargement of the back, or the degree of constitutional health to which the patients were afterwards restored. In all these particulars the treatise is very defective and unsatisfactory. We are, however, under great obligations to the candid author for making us acquainted with a plan which promises to be extremely advantageous in the management of these and other obstinate complaints. It consists in placing the patient horizontally upon a firm and unyielding mattress, where he is to remain constantly recumbent during the whole process. He is not accommodated with a pillow to support the head; nor is he to be moved in the least, for the most necessary occasions. All fears of the health suffering under this mode have been happily removed, by the successful issue of the different trials. In every case where it has been properly conducted, the patient soon became easy, cheerful, and regained his rest. Appetite and digestion improve under the confinement. In general, there is increase of flesh, and a marked improvement of countenance.

The intention of the plan is to afford the "softened bones of the vertebræ" an opportunity for recovery, and make themselves able to support the weight of the parts above. So long as the column remains perpendicular, it is capable of bearing considerably more weight than after it has become oblique. In order to restore the strength of the spine, the person is to be laid flat, to give the "softened bones of the vertebræ" a better opportunity to recover themselves. Until the enfeebled spine is relieved from its natural load it cannot get tone, because the weight of the head and shoulders prevents the benefit to be expected from any mode of treatment. A system of resting will accomplish cures after the failure of drains and machinery. It may be observed, that Mr. Pott, though he takes little or no notice of the matter, thought it necessary to confine his patients in bed, or the horizontal posture, during the greatest part of the cure, as they could not bear to remain in an upright disposition.* The length of time required to produce the cure, varies, as we have already stated, from seven to fifteen months. At the end of the limited period, the patients were allowed to rise and take exercise, according to their ability. We have not been informed by the author of any relapses, but such occurrences are

* See Sir James Earle's *Observations on the Cure of the Curved Spine.*

asserted to have happened from other quarters. They were probably owing to the want or neglect of proper directions in regard to the care and precautions which should be observed for some time after the reclining method has been discontinued. No means were used to remove the tenderness and pain of the back. These were soon subdued by rest alone. In no instance were external applications wanted. Internally, the liquor calicis muriatæ was administered, to increase and consolidate the ossific process. Besides this remedy, bark, and medicines to obviate costiveness, were also employed. It is difficult to say how long the resting should be continued; nor can we lay down any rules with confidence till we have had greater experience. In the slightest cases, the recumbency must be continued five or six weeks after the removal of all tenderness. Where disorganization of the bodies of the vertebræ has taken place, the rest must be prolonged two or three months after every inconvenient symptom has disappeared.

Such is the plan recommended by Mr. Baynton. It consists, as we have already observed, in placing the patient horizontally upon a mattress, without bolster or other elevation, and continuing the same position till the spine shall have recovered firmness enough to support the head, shoulders, and chest. This is the whole object contemplated, and such are the means employed to attain it. When the process is finished, no measures of precaution are advised to prevent relapses. The patients arise from their couch, and thus ends the treatment.

Mr. EARLE, laudably solicitous to maintain the high character of his deceased ancestor, has for that purpose voluntarily entered the field of controversy with the Editors of the *Medical and Surgical Journal*.^{*} In the review of Mr. Baynton's essay,† they had taken occasion to extol his "humane" method, compared with the severity of Mr. Pott's treatment of spinal complaints. In order to bring the matter fairly before an impartial public, Mr. Earle was led to analyze Mr. Baynton's twelve cases, published in favour of the resting process, and to accompany each of them with his own remarks. He reports that only three of them are of the description alluded to by Mr. Pott; and in them the previous employment of caustic issues had, he thinks, removed the diseased condition of the bones. The merit of the resting-plan was, therefore, limited to expedite the cure; for which intention he thinks it well adapted, where "softness prevails in the bones." In all the remainder, there was either no disease of the back, or it consisted of a muscular affection only. In confirmation of this assertion, he introduces

* Vol. xi. No. 41, *Edinburgh Medical and Surgical Journal*.

† Vol. x. No. 39.

accounts of three cases which fell under his own management. In these the spine was greatly bent, and the lower extremities more or less affected. One appeared to arise from hydrocephalus internus. The second occurred to a military officer.

"On the day after exposure to great fatigue and being wet through for many hours, he had an attack of fever, and, on attempting to walk, he found stiffness in his legs and inability to move them. Soon afterwards he was incapable of expelling his urine, and his feces at times passed involuntarily. That these symptoms gradually increased, until the lower half of his body was completely palsied." He had suffered four years when Mr. Earle was consulted. He was miserably helpless. His lower extremities were cold and benumbed, and he had lost all power of directing his steps. The whole vertebral column was slightly arched in the form of a half-hoop, but no part was unnaturally exuberant. When supported, he could raise his body from its bent shape to an upright position. The form of the back, when reclined, was quite natural. This circumstance, contrasted with subsequent experience, induces me strongly to believe that there was no disease whatever in the vertebral column; and this arching of the body was purely the effect of deficient muscular action, rendering the muscles of the back incapable of supporting the weight of the head and trunk: and, from the result of the examination which I was enabled to make a short time after, in a case which I shall presently relate, I am firmly of opinion that the deficient nervous energy in this case depended either on some morbid affection of the brain or its membranes, which rendered it incapable of transmitting its influence to the extreme parts of the body, or some diseased affection of the medulla spinalis, wholly independent on any disease in the vertebræ."

The third patient was a soldier, whose constitution had suffered from previous residence in hot climates. "In addition to the curved back, he had deficient muscular power in the lower limbs. There was a numbness and irregular action in the muscles of the fore-arms and hands: he was incapable of feeling any minute objects, and would often let things fall from his hands. On dissection, the liver, spleen, and lungs, were found to be diseased. "As I was most anxious to ascertain the state of the vertebræ and medulla spinalis, I did not pay much attention to these appearances, and, having cleared away the viscera, proceeded to make an accurate inspection of the vertebral column; and, no disease being apparent externally, I removed all the bodies of the vertebræ, by sawing through the rings between the articular processes. By this plan, I obtained a complete and very satisfactory view of the spinal marrow;

but still no cause for the symptoms presented itself, and I was left to conjecture respecting the nature of the malady. On examining the head, however, the source of the mischief became apparent. The cellular structure of the pia mater was loaded with fluid, and the tunica arachnoidea was of a milky colour and thickened, especially at the base of the brain, where it was firmly connected with the pia mater, forming together a tough dense membrane. I am aware that to many this may appear an insufficient cause for the violence and extent of the symptoms; but, having paid considerable attention to the morbid anatomy of the brain, and well knowing the infinite variety of symptoms produced in different individuals by the same morbid alteration of structure, I am satisfied in my own mind that all the phenomena were referable to this diseased state of the membranes."

Whatever difference of opinion may arise in other respects, every one will, I think, admit Mr. Earle's cases to be legitimate examples of paraplegia, though certainly not of the variety described by Mr. Pott.

The more we become acquainted with spinal pathology, the stronger is our conviction that paraplegia ought to constitute a generic term, and include under it numerous species. Of these, one, and that comparatively of rare occurrence, originates in a carious state of the bones. In others, it takes place in the progress of the complaint; but in the greater number it never appears, though the health be generally impaired and destroyed by the mischievous effects impressed upon the constitution. We find the most essential signs of the disorder clearly displayed in Mr. Baynton's cases. There is debility, numbness, a pricking sensation, and torpor of the lower extremities, with incapability of directing the motions. In some, the upper limbs were also affected, as well as the internal viscera, giving to the malady a greater range of morbid action.

These phenomena having by all physiologists been referred to the nervous system, I do not think it necessary to produce any arguments in support of an admitted doctrine. The only question for discussion, is the seat of paraplegia. Is it ever in the brain, or is it always in the spinal marrow? In order to arrive at a more satisfactory conclusion, we may premise that none of the cerebral nerves, with the exception of the eighth pair, and perhaps the great sympathetic, descend below the throat; and the spinal marrow is united to the brain by means of the medulla oblongata. The cerebral nerves are principally employed to convey impressions to the sensorium commune, and transmit the determinations of the will to the muscles of voluntary motion. By means of the spinal nerves, the offices of secretion and excretion are performed. They

regulate the circulation of the blood, and impart energy to the extremities of the body. These nerves are chiefly independent of the brain; for, if the head be cut off, in certain warm-blooded animals, and respiration artificially carried on, the trunk will live for many hours: but, destroy the spinal marrow, and the animal dies almost immediately. We are led from these experiments to conclude, that the nerves of the brain and spinal marrow discharge separate and distinct offices in the animal economy, though the brain certainly exercises its control, through the spinal cord, over the voluntary muscles. The involuntary motions are executed by the spinal cord alone, and are wholly independent of the brain.

The difference between the two orders of nerves is very apparent in the paralysis which they occasion. Hemiplegia is confined to one side; paraplegia equally affects both. In the former, the diseased muscles are more flabby, soft, and unresisting, than in the latter. The mental faculties are always disturbed in hemiplegia; in paraplegia they remain free. The latter complaint deranges some of the internal functions; but in the former they are little impaired. By paying attention to these several distinguishing characters, a clear diagnosis may generally be established between these different forms of palsy.*

If we examine Mr. Earle's first case according to the above rule, we shall find considerable derangement within the body, as well as in the lower extremities. I cannot therefore hesitate to consider it as a species of paraplegia, induced by compression of the spinal nerves. From the torpor of the bladder and rectum, together with the involuntary discharge of feces after taking purgative medicines, I am induced to place the seat of disease in the loins or sacrum. I have come to this conclusion because the nerves distributed to the rectum and bladder issue from these parts. Had the disorder occupied a higher station, some of the abdominal viscera would have participated in the complaint. I am of opinion that his fever after exposure to wet and fatigue, proceeded from an attack of inflammatory rheumatism in the loins or sacrum.

The joints, the tendinous sheaths, *bursæ mucosæ*, and in general all parts furnished with fibrous structure, are liable to its invasion. The tendons of muscles are often attacked, but the fleshy bellies seldom or never suffer from this complaint. We find, on dissection, a glairy fluid effused, with increased thickness and morbid enlargement of the surrounding structure.

Rheumatism seizes indiscriminately upon the vertebral joints. The back is frequently the seat, because it is supplied with

* The diagnostic signs will be more fully considered in a future essay.

strong ligaments to provide for the weight and motions of the body. It has likewise the usual articular provisions, and a tendinous expansion, the common posterior ligament of the vertebræ, enclosed within the theca spinalis. Rheumatic inflammation of these organs, by enlarging their bulk and dimensions, occasions compression upon the spinal cord, impedes its operations, and thus induces coldness, debility, numbness, loss of feeling and motion in the limbs, and the various sensations and inabilities complained of in the internal parts. In agricultural districts, rheumatism is of frequent occurrence among the labouring poor, from their being thinly clothed and so much exposed to the changes of the weather. When it invades the back, symptomatic paraplegia often supervenes in the arms or legs. Many patients of this description have been admitted into the Horncastle Dispensary, under my professional care. In some, the paraplegia was entirely removed with the primary complaint: in others, it continued to torment the sufferer during the rest of life.

It is for these several reasons, and from the suddenness of the attack, that I feel inclined to impute the origin of Mr. Earle's first case to lumbago, rather than any other cause. Had the inflammation invaded bone or cartilage, I think its effects would not have appeared so early as the following day: I therefore coincide in the opinion that the vertebræ and cartilages were wholly untouched. The disorder in process of time became chronic, and was then attended with muscular debility. Still I think it began in membranous parts, and extended from them to the muscles. That the articulating organs of the vertebræ were relaxed and preternaturally elongated when Mr. Earle was consulted, must, I think, be conceded, from the arch or "half-hooped form" of the back in an erect posture, and its becoming straight upon lying down. To me it appears altogether impossible to distort the bones of the vertebral column in the manner described, without stretching and hurting some of the articulating connections. So long as they remain firm and unyielding, the vertebræ continue fixed; but, when they begin to lengthen, the vertebræ as certainly show a disposition to move out of the line: I therefore think it much more probable that the affection was situated in the ligaments than the muscles, especially since fibrous texture possesses little inherent contractibility. When once over-stretched, it recovers its lost tone slowly and with difficulty. In this respect it differs from muscles, which admit of considerable contraction and relaxation, without injury to their functions. The bent figure of the vertebral column, and consequent extension of the spinal marrow, would account for the symptoms under which the patient laboured when Mr. Earle saw him; but the commencement

of disease on the morning after long exposure to wet and fatigue, cannot be referred to a circumstance which had not then taken place. The subsequent appearance of the same complaint in other vertebræ, is agreeable to an established law of the animal economy. Rheumatic inflammation passes successively from joint to joint, sometimes leaving the former to invade the latter; more frequently, it attacks the new organ without deserting the old one. It is no uncommon thing for sciatica to proceed to the knees, ankles, and superior extremities. After remaining in all of them for a certain period, it gradually disappears, or degenerates into a protracted rheumatism. It is in this way that we account for the membranes and ligaments of the back becoming so generally and obstinately disordered in Mr. Earle's and Mr. Baynton's different cases.

An objection may be raised against this explanation, because, in the second case, Mr. Earle found no disease of the spinal marrow, after viewing it by "sawing through the rings between the articular processes." It is clear from the statement, that it never occurred to his mind to examine the articulations with any particular attention; otherwise, he would have laid them open with greater care and in a different manner. His eyes and thoughts were exclusively directed to the vertebral bones, their intermediate cartilages, and the spinal cord; and, to get to the objects of research, he incautiously tore and mangled the articulating membranes with a coarse instrument. It could not be expected, after such treatment, that the parts would display any strong signs of diseased structure; and, therefore, his finding no sufficient cause for the symptoms in the spinal marrow, does not convince me that none were present in the contiguous ligaments. The advocates for pressure generally would have examined the bones, cartilages, and membranes which surround the spinal marrow or connect the parts together, under an idea that paraplegia derives its origin from disease situate sometimes in one tissue and sometimes in another.

I have thus endeavoured, in controverting the theory of Mr. Earle, to establish the phenomena on a basis ample enough to afford them support, and more conformable to the principles of the animal economy.

Dr. BAILLIE has also favoured us with his sentiments on paraplegia, in the Transactions of the Royal College of Physicians.* It is much to be regretted, however, that he did not superintend the dissection of a patient, whom he had occasionally visited during life, and whose case constitutes the basis of his paper: had he personally directed the investigation, I am convinced that the spinal marrow and its membranes would not,

* Vol. vi. page 16.

under such circumstances, have escaped the notice of a physician intimately acquainted with morbid anatomy and the general principles of his profession. "This last form of paralytic affection," (the paraplegia), he observes, "has, as far as I can judge from individual experience, increased considerably in this country within the last fifteen or twenty years, although it be very difficult to assign any satisfactory reason for it. Paraplegia in adults has been considered by most medical men as being produced by some disease, either in the bones or ligaments of the spine, or in the cavity of the spine, most commonly at the loins, independently of any disease in the brain. The reason of this general opinion has probably been, that the lower part of the body from the loins downwards is, in this disease, affected with paralysis; and that, in children, a similar disease is often obviously dependent upon a morbid affection of the lumbar or some other portion of the spine. In adults, however, where there has been no accident affecting the spine by outward violence, paraplegia, I believe, depends most commonly, in a great measure, upon a disease affecting the brain itself. This opinion I have entertained for several years, and some other medical men have likewise held the same opinion. It is, however, by no means general; and the chief object which I have in view in writing this paper, is to render this opinion more commonly known, that it may either be established or be properly limited by the future observations of other practitioners."

The brain and its membranes, in the case recorded, exhibited strong marks of disease, though they were not, as I think, sufficiently distinguishing to account for the paraplegic symptoms. We are not informed how the mental faculties and organs of sense were exercised under the pressure of the disease, nor to what degree the feeling and motion of the arms were impaired, or in what manner the thoracic and abdominal viscera discharged their respective offices. These omissions are the more to be lamented, because, if the several functions remained undisturbed, we have little reason for imputing the disorder of the lower limbs to the cause assigned.

"A considerable quantity of water," we are told, "was discharged during the inspection from within the theca of the spinal marrow." When such an obvious source of the disease presented itself, we have surely more reason to refer the paraplegia to it, than attempt to search for its origin in the deranged structure of a distant part. We have the authority* of celebrated names for maintaining that, when a fluid is found after

* Morgagni, *Epist.* xii. art. 9; Willis; Lieustaud, *lib.* iv. p. 341; Bonetus *Magnetis*; Colletus.

death both in the head and spinal tube, it sometimes commences in one part and sometimes in the other. Since, then, the effused liquid may, in the case recited, have had a two-fold commencement, it is impossible to determine, imperfectly as the inspection was conducted, where it was really formed. Had the surgeons examined the vertebral column with proper care, I think they would have discovered in it the origin of the disease, and that the malady in the skull either proceeded from the spine, or was only a co-existent and unconnected attendant. We feel more grieved that the state and condition of this organ and its contents should have been overlooked, because the subject has been very little elucidated by careful dissection, and appears to be much in want of further investigation.

We have thus given a brief analysis of the works of Mr. Pott and Mr. Baynton, the two chief practical authorities on this subject, interspersing our remarks with the observations of other writers. This was undertaken with a view to communicate what has hitherto been advised and done in respect to one of the most obstinate and distressing calamities to which the human frame is exposed,—a calamity which poisons our present happiness, and deprives us of the hope of future enjoyment.

Some Observations on the Treatment of the Puerperal Fever.

By Mr. EDWARD JUKES, Surgeon.

TO produce observations on the treatment of the puerperal fever (*puerperal peritonitis*), after those of Ramsbotham, Gordon, Hey, and, especially, the treatise of Dr. Armstrong,—in which the nature of the disease, the indications for its cure, and the rules for the application of the remedies, are established in such a way as to place them above either doubt or disputation,—may appear to be both unnecessary and unprofitable. The anticipation of such considerations would have induced me to refrain from taking up the pen to write on this subject, had I not had frequent occasions for remarking that the existing information comprised in books, is not that of a considerable proportion of the class of medical practitioners on which the welfare of patients, in this disease, must chiefly depend; for it is to them,—that is to say, the surgeon-apothecaries,—that the management of the generality of patients is confided during the stage of the disease in which, for the most part, medical aid is alone successfully employed. A great proportion of the class of practitioners just named, consists of men either too much occupied by actual practice to be able to read much, if they are disposed to it; or who have not leisure or ability for that degree of reflection which is requisite in order

that the inferences stated in, or drawn from, books, may be applied to particular cases with appropriate energy and precision. I am aware that it may be asked here,—why I add, then, to the number of writings which, according to my acknowledgment, must be devoid of utility? My reply is, that the intercourse I have had with practitioners of my own class has permitted me to perceive the deficiencies of some of them, and the means of supplying those deficiencies, better, perhaps, than our esteemed authors, who are apt to suppose that all medical men can reason analytically and inductively as well as themselves. There are practitioners who have understood that it has been stated that blood-letting and purgatives are the only means to be depended on for the cure of the puerperal fever, and that, without them, all other measures will fail of success. They have, accordingly, employed blood-letting and purgatives, and, their patients having died, they have ceased to regard the practice in a favourable manner. They have heard of cures apparently effected by large doses of opium or other powerful stimulants, and, having witnessed fortunate results from their use in late and desperate stages of the disease, they have employed them in all stages of it; and, of course, with consequences that make them regard the puerperal fever with sentiments of hopeless anxiety on all occasions.

Such results as these ensue from the want of discrimination in marking the stages of the disease to which the several remedies are exclusively or especially appropriate, as well as of sufficient precision in the application of them to particular cases. I have, of late, treated many cases of the puerperal fever, and with nearly universal success; yet I do not pretend to have discovered a single important fact respecting the nature of the disease, or to have acted in conformity with any one precept which I have not derived from the authors above enumerated. I have merely collected their conclusions; generalized them to a certain extent for my own use, and made the principles thus formed the general guides for my conduct, to be deviated from in some degree as particular circumstances might indicate. A person who would draw up a series of aphorisms, embracing the general indications and precepts for the treatment of the puerperal fever, would, I think, confer a great benefit on many members of the profession. It is much to be regretted that this has not been effected by our latest and justly much esteemed writer on the subject. There is, I know, this objection to general principles for the treatment of disease, that they must be either too vague or too exclusive to be applied in a precise manner to particular cases; and, therefore, that, if acted on, they must occasionally be the cause of error. In opposition to this it may be stated, that some general prin-

ciples of conduct are absolutely necessary: no man can act without them; and it is much easier to discern exceptions, than it is to arrive at them by inductive reasoning; which, when it rests on acknowledged data, is the pillar of true science, the basis of which is fact, whilst truth is its capital: but, inductive reasoning, or theory, has been too often confounded with hypothesis, which sets out in search of data, instead of proceeding from established facts; and, though hypothesis has often proved the wings of science, and has even directed its course, in a flight bold but eccentric, like that of the eagle, towards the source of light, it has more frequently led to wanderings which may be compared with those of a person who, on rising from the perusal of his Plato, should embark for the fabled Atlantes.

The puerperal fever is sufficiently distinct and characterized a morbid state, and experience has sufficiently well established the principles for its treatment, to admit of the general inferences above alluded to. It is well ascertained that, whether or not it essentially depends on peritoneal inflammation, the danger is always in a direct ratio with the intensity of such inflammation; and that there is no form of inflammation, the different degrees and consequences of which succeed each other with more rapidity, or in which the period of irritation has a more short duration, especially when it tends to a fatal issue. Some pathologists, apparently overlooking the first stage of the disease, the shivering with which it is ushered in, the acute and constant pains, with excessive tenderness, in the abdomen; the burning heat and aridity of the skin; and the frequent, full, and hard pulse with which it is accompanied; and, considering alone the state of collapse, debility, cold sweats, rapid and feeble pulse, and signs of great exhaustion especially of nervous power, have viewed in the disease only a state of debility, and have not considered that this debility is merely a consequence of the previous excitement, and that the extent of the former is in a direct ratio to the intensity and duration of the latter. There are men of long experience, I am aware, who think differently of the nature of the disease, chiefly because they have found it terminating favourably, in some cases, under the use of cordials and stimulants; and perhaps they have forgotten the remark of Sydenham respecting the treatment of the small-pox, that there are cases which will not prove fatal, in spite of bad management; or they cite authorities from old writers, and the zeal of some of them for such precepts has led them, when the inflammatory nature of the disease has been demonstrated by dissection after death, to imitate the conduct of Silvius, the preceptor of Vesalius, who, when confounded by the demonstrative proofs of his pupil, rather than doubt the infallibility of Galen,

asserted that "men were otherwise made in the days of Galen than in the time of Vesalius."

The foregoing views of the nature of the disease, with a consideration of the peculiarly irritable state of the nervous system, and of the violent derangements of function dependent on it, furnish the first indications for the treatment; and, in conformity with them, experience has shown that blood-letting, employed in the first stage of the inflammation, (generally within thirty-six hours from its commencement; though Dr. Armstrong relates some cases in which it was resorted to at a later period with favourable results,) and copiously, as well as promptly, is absolutely necessary in almost every case, in order to lead it to a favourable issue. After the first stage of irritation has passed away, and real debility and collapse have come on, (and, when the disease has proceeded unalleviated, these generally occur in from twenty or thirty hours to two or three days,) blood-letting, by venesection, is then always injurious; and the application of leeches to the abdomen, is not often, though it is sometimes, advisable. In a great many cases, blood-letting, to the extent of from fifteen to five-and-twenty ounces, effected within a very few hours from the onset of the disease, has arrested it almost instantly; and but a small proportion of cases treated thus, (with the other means to be presently mentioned,) with a repetition of the measure, in some instances, after a few hours, have terminated fatally; excepting only instances where the disease has occurred epidemically, and in Lying-in Hospitals, and when it has been accompanied with habitual or recently-acquired considerable derangement and debility of the nervous system; or when it has been accidentally accompanied with other diseases.

Next in importance to blood-letting, and a measure which should always accompany it when diarrhœa does not spontaneously occur, is the use of purgatives: and experience has shown that a medicine of this kind which will act promptly and affect the whole intestinal tube, such as a scruple of calomel, with as much jalap, or salts and senna in sufficient quantities, administered in the commencement of the disease, and followed by the constant and regular exhibition of milder purgatives, such as castor-oil, or sulphate of magnesia and senna, manna, &c. so as to keep up a gentle diarrhœa for several days, have led many severe cases to a favourable termination. But hardly any case can, perhaps, occur, in which it would be justifiable to trust to purgatives alone. These remedies should accompany the use of blood-letting, and be employed with the same promptness as the latter measure. There may be cases where there has been great exhaustion from previous hemorrhage, or some long-continued or very severe disease, or some extraor-

inary derangement of the nervous system, in which general blood-letting may not be advisable; but none in which leeches, to the number of twenty or thirty, should not be applied to the abdomen, if venesection is not employed, and often when it is practised. The puerperal fever is generally accompanied with, and often has been preceded by, a costive state of the bowels; but it should not hence, or because purgatives act beneficially, be considered that the costiveness has produced the disease. Common peritonitis, from whatever cause, is ordinarily attended, in its first stage, with constipation. The irritation of the peritoneal coat of the intestines seems to suspend or lessen their peristaltic action, and to diminish the secretions from their mucous membrane; and producing irritation of the latter, by purgatives, relieves that of the peritoneum, probably, on the principle of revulsion; or just for the same reason that a patient, whose case is related in the first volume of the *Dublin Hospital Reports*, by Dr. Cheyne, had diarrhœa and ascites repeatedly alternating with each other. Sometimes the puerperal fever is accompanied with a spontaneous diarrhœa; and, when this happens in a violent degree at the commencement, it ordinarily marks an unusually severe form of the disease, which is not unfrequently fatal. In one case of this kind, in which I had an opportunity of examining the body after death, I found the mucous membrane of nearly the whole extent of the intestines had been in a state of inflammation, in addition to that affection of the whole of the peritoneum. In other cases, as far as my own observations have extended, the mucous tunic has been apparently in the most healthy state; whilst the peritoneum has been red, thickened, coated with false membranes, or gangrenous in patches, and the abdomen filled with serous and purulent fluids.

It must be obvious that when diarrhœa of the kind just mentioned takes place, irritating purgatives should not be employed; and the administration of astringents or cordial opiates, so often used, is equally injurious. Mild mucilaginous drinks and emollient enemas should be given, with ipecacuanha in small and frequently repeated doses, with or without kermes mineral, after the manner of Doulcet. Ipecacuanha is, in most cases, beneficial after the first stage of the disease has passed, and it may be combined with the gentle purgatives which may be employed. Very obstinate constipation is an unfavourable sign; and, indeed, a very great proportion of the fatal cases on record, seem to have been accompanied, in the first stage, with either this symptom or severe diarrhœa.

But, in pursuing these remarks, I might appear to be attempting to effect what it is really my intention to point out for the attention of others, who are better qualified for the

task: I shall therefore conclude this desultory paper with the history of a case of well-marked puerperal fever; which I adduce, because it exemplifies in a very forcible manner the powers of copious blood-letting as a remedy for that disease.

Mrs. Martha Clark, aged 24, of a healthy and rather robust constitution, residing at No. 40, York-street, Westminster, was delivered of her first child about nine o'clock in the evening of the 7th of September, 1820, after a labour regular in its progress, short in its duration, and favourable in its termination.

Sept. 8th.—She has passed a good night; is remarkably cheerful, and has been free from pain; the lochiæ have appeared in the ordinary way; the pulse 80; the skin moist. No motion since delivery.

9th; eight o'clock in the morning.—Violent pain in the abdomen (principally between the umbilicus and the pubis) came on late last night, occurring every ten or twelve minutes, and continuing between two and three minutes. Within the last few hours the pain has been constant; has ceased to be accompanied with a sense of bearing-down, as it was at first, and the slightest pressure increases it considerably; the weight of the hand is intolerable, and the increase of pain by pressure does not subside by a continuation of the pressure, as is commonly the case in *after-pains*. Pulse 130; skin hot and dry; tongue white; bowels confined, and the lochiæ suppressed. Eighteen ounces of blood were drawn, and one ounce of castor-oil given; fomentations ordered to the abdomen.

11 o'clock, a. m.—Pain continues with increased violence. Eighteen leeches applied to the abdomen, and the hot fomentations continued; a scruple of calomel was given, and a mixture of salts and senna directed to be taken every three hours.

4 o'clock, p. m.—The pains not diminished, and the tenderness of the abdomen so extreme that the weight of the bed-clothes is insupportable; pulse still 130. One motion obtained. The purgative mixture to be continued.

9, p. m.—The pain is still more severe, is extended equally over the whole of the abdomen, the tenderness of which is so exquisite, that, on a small piece of dry sponge falling on it, outside the bed-clothes, from the height of about two feet, the patient screamed out in an accent of extreme suffering. She lies on her back, with her arms bent, and the thighs extended apart and a little raised, so as to prevent the weight of the bed-clothes from falling on the belly. There is an expression of much anguish in the countenance, and the face is bedewed with a greasy-looking sweat. The skin generally is cold and damp. Inspiration is short, and evidently restrained by the efforts of the patient. The tongue is covered with a thick

white fur. The pulse is 140, hard, small, and thrilling. The bowels have been open once since the last report. The lochiæ are suppressed. The patient sighs, complains, in an interrupted voice, of languor and faintness, and says she is dying. The blood drawn in the morning is cupped and a little buffy.

Twenty ounces of blood were taken from the arm. Immediately after the bleeding the pulse had fallen to 90, and was softer and fuller; the breathing was much deeper, and apparently unrestrained by voluntary exertions.

A blister was applied to the interior part of each thigh; an enema of gruel, sulphate of magnesia, and oil, administered; and a draught of one grain of ipecacuanha, with two drachms of sulphate of magnesia, in infusion of roses, given every four hours.

10th, 8, a. m.—The pain subsided soon after the bleeding last evening; and the patient fell asleep about midnight, and slept for several hours. She is now almost wholly free from pain, and the tenderness of the abdomen is but very slight. The pulse is 76, and in all respects has a healthy character. The anxiety in the countenance has disappeared, and the face is more florid; the skin generally is moist and warm. The bowels have been opened several times; there is nothing remarkable in the appearance of the stools. The draughts to be continued.

11th.—Has had no return of any of the symptoms of the disease, and is convalescent. The lochiæ have re-appeared. The draughts to be continued after longer intervals, so as to keep the bowels in a relaxed state.

15th.—Is perfectly well, and has suckled the child for the last two days.

Nov. 1st, 1820.

Descriptive Account of a Bicephalous Fœtus. By Mr. I. JACKSON, Member of the Royal College of Surgeons of London.

MARY HONEYFORD, the mother of the child to be described, is unmarried, about twenty years of age, of a sanguineous temperament, florid complexion, and rather below the middle stature; her occupation is that of a weaver; and she has enjoyed nearly uninterrupted good health from infancy.

In the forenoon of the 10th of August, 1820, I was desired to visit her, and take with me the midwifery instruments. I learnt from the messenger, that she had been in labour upwards of two days, and that midwives had been with her most of the time. For the last eighteen hours, one of them had been constantly assisting her, but was unable to accomplish

her delivery: the pains were very weak, and her strength nearly exhausted.

When I arrived at the house, I was informed by the midwife that the waters had been drained off twenty or twenty-four hours; the head had been in the situation it now occupied for ten or twelve hours; and, to use her own expression, the head had descended slowly to the birth, and then it had stuck on the spare bone.

The patient's pulse was 135, and weak. The parturient paroxysms occurred every three minutes, but were inefficient. Anxiety was depicted in her countenance, and a general restlessness pervaded her; as is frequently, if not always, witnessed in tedious painful labours.

On examination *per vaginam*, I found the os frontis presenting, the integuments of which were tumefied, from the time it had remained in the pelvis, and the attempts the midwife had made to assist her. The face was towards the pubis, or rather towards the right groin, and the soft parts of the mother quite relaxed: thus, reasoning from my experience in similar presentations, I believed there could be no apparent obstacle to a speedy and favourable termination. I ordered her a little wine and water, and calmed her mind, by flattering her with a speedy release from her suffering and anxious situation.

As the parturient energies were nearly exhausted, I was persuaded it was not safe to trust any longer to nature, and that artificial aid was necessary to assist the natural efforts to promote her delivery: I therefore assisted her in the following manner. I introduced two fingers under the os pubis of the mother, where I could reach the mouth of the child, by which I could command considerable force, and assist the expulsive efforts of the uterus: thus, by extracting at each pain, in less than half an hour the face emerged from under the pubis, and the head was nearly half protruded through the os externum. Finding that something impeded the complete expulsion of the head, though I used considerable extracting force, I passed two fingers by the head of the child to ascertain the obstruction, and where it existed, when they came in contact with a firm tumor, descending into the cavity of the pelvis, closely joined in contact with the neck of the first head. On examining it as particularly as the confined state of the parts would permit, I was convinced it was the head of a child, from the hair on the scalp and the feel of a suture.

I did not recollect the record of any case similar; for I imagined it a case of twins, in which the head of the second child, by some partial contraction of the uterus, had been forced down, or somehow got entangled before the shoulder of the first. After considering for a short time what would be the

most preferable method to pursue to accomplish a safe delivery, I came to this determination, that, as the superior aperture of the pelvis was sufficiently capacious to admit the head and neck of a child in conjunction, I would attempt to extract the child whose head was already in part protruded; and, if I failed in that attempt, I would perforate the second head in the pelvis, and break it down with the blunt hook, as the most probable means of safety to the mother.

The woman continued to have regular, though very inefficient, pains, at each of which I used such extracting force as I thought was compatible with the safety of the mother and child: I easily ascertained it was living, by the pulsation of the temporal arteries. After some time I found I was gaining a little ground, and that the heads retained their relative situation; that is, as one was further protruded, the other descended lower towards the os externum. Having more space for examining the presenting part of the second head, I satisfied myself, from the sutures and posterior fontanell, that it was the occiput.

I thus continued extracting and guarding the perineum for about an hour, when the second head was protruded; immediately after which followed a sharp expulsive pain, when the body of a living male child with two heads was brought to view.

The child appeared lively: I lost no time in tying the umbilical cord, which was no thicker than ordinary, that I might remove it, to examine it more particularly and make further observations.

Having taken it into an adjoining room, I found it had all the powers of voluntary motion as perfect as a natural fetus: the eyes of each head were opened and closed occasionally; and the muscles of each face contracted, as if to squall, and one of them made a considerable noise, which was the first head protruded. On a closer inspection, I found the other head never breathed, although it was equally lively with the first. It continued gradually to weaken for about forty minutes, when it ceased to breathe. For a considerable time after respiration had ceased, I could feel the heart palpitating, with a tremulous motion, in the epigastric region.

With respect to the mother, I may briefly state, that, whilst I was making the above remarks on the child, one of the midwives, by officiously attempting to bring away the placenta, broke the umbilical cord; so that I was obliged to introduce my hand into the uterus, to detach and bring it away. Notwithstanding this circumstance, added to the tedious and painful parturition, she recovered as well as after an ordinary labour: no unpleasant symptoms supervened, except that the prostration of strength required a longer time to be re-established.

Mr. Bailey, of Blackburn, a respectable surgeon and good anatomist, assisted me in examining the child, about twenty-four hours after its birth; when we made the following observations.

External appearances.—The heads were well formed; the bones of which were as perfectly ossified, as well as those of the limbs, as is found in the majority of infants. The neck of the left head appeared somewhat longer than the other; but I attributed that to the extending force I used in the extraction; and the other would be forced in an opposite direction upon the breast, by the resistance given from the soft parts of the mother. It had clavus of the right foot. The weight of the child was eight pounds, fifteen ounces, and six drachms, avoirdupoise.

The measurement of the left and larger head, from the sinus frontalis to the tubercle on the os occipitis, $8\frac{1}{2}$ inches; round the head, 13 inches, at the same relative points. The smaller head measuring $7\frac{1}{2}$ inches, and $12\frac{1}{2}$ inches. The circumference of the chest immediately below the arms, 14 inches. The sternum was broader than is usual, and the ribs appeared to make a greater curve than is generally observed. The claviculæ were three; two in their natural situations; and the third, which was equally as large as the others, was attached to the top of the sternum, and proceeded backwards between the two necks; where there was also a third scapula, forming a protuberance, or shoulder.

On examining the back, there felt as if there were three vertebral columns: the middle one, on dissection, was found to be the cartilages of ribs, each about an inch in length, coming from the two spines, which joined at obtuse angles, and gave to the touch externally the feel of spinous processes.

Internal examination.—On opening the thorax, the first thing that attracted our attention was the situation of the heart in its pericardium: it was situated between the two lungs, and about as much inclined to the right side as in a natural case it is to the left. The lungs were large, having two tracheæ, each terminating separately; the one in the right, the other in the left, lung. Each lung had three lobes. Each head had a distinct œsophagus, which passed separately through the diaphragm. The appearance of the lungs confirmed the observation I before made, that only the left head had breathed; the right lung having the appearance of liver, the colour of which, on being inflated, was instantly changed to that peculiar pulmonary mottled hue, which characterizes this organ after respiration.

On opening the pericardium, the heart was found to be very large. The aorta, which was also very large, arose from the left ventricle; as also did the pulmonary artery to the left lung. The

aorta ascended about an inch, then made a turn backwards by the left side of the trachea at the root of the right spine, towards the right side, receiving the sinus arteriosus, from the left pulmonary artery, and giving off an artery which ramified on the short ribs connecting the vertebræ. It then passes to the right spine, on the left side of the œsophagus, and forms behind that organ a beautiful arch; and, with another artery, arising from the right ventricle, and which I will call a minor aorta, forms a large common aorta descendens.

The right ventricle gives origin to the right pulmonary artery, and also to another which may justly be called a minor aorta: the latter ascends about one inch, passing from left to right in front of the right trachea, giving off an artery which subdivides into two,—a small subclavian to the third clavicle, and the left carotid of the right head. Having passed across the trachea, it gives off an arteria innominata, which bifurcates into the right subclavian and right carotid of the right head. The continuation of the aorta then passes to the spine, on the right side of the trachea, receiving a sinus arteriosus from the right pulmonary artery, and joins the large aorta from the left ventricle, forming together, as before observed, the aorta descendens.

The aorta descendens then passes down on the left side of the right spine as low as the first lumbar vertebræ, when it gives off a large artery, which may very properly be denominated an aorta ascendens. Passing up as high as the first cervical vertebræ on the left side of the spine belonging to the left head, it then subdivides into two common trunks; the left of which immediately bifurcates with the left subclavian and carotid. The right trunk ascends about an inch under the trachea and œsophagus, and then divides into the right carotid of the left head, and a subclavian to the common clavicle. Thus the third clavicle has two subclavian arteries, but smaller than those which supply the extremities, and which inosculate in a beautiful manner.

The aorta descendens, after having given off the aorta ascendens, descends upon the spine, giving off the usual abdominal arteries, and bifurcates, as is natural, into the two common iliacs.

In the venous system, nothing particular or extraordinary was observed: the jugular and subclavian veins of each head and extremity united to form one great vena cava superior. The vena cava inferior was single, and of the usual magnitude.

The contents of the abdomen appeared natural: the liver was large, but had nothing peculiar in its appearance. On turning back the liver, we found two stomachs: the one to the left head occupied the natural situation, the other was situated under the

right lobe of the liver. Each stomach had its separate duodenum, the right one about half an inch, and the left one about an inch and half in length; when they united, forming one common duodenum. There were two gall-bladders, each having a separate duct, which terminated at the junction of the duodena. The spleen, pancreas, and kidneys, were natural; as also were the intestines; the colon and rectum were distended with meconium. The contents of the pelvis were similar to those of a single child.

On examining the vertebral columns, we found two, perfect and entire, but which gradually approximated as they approached the sacrum; which was single, but much broader than natural.

Having given a detail of the circumstances attending the labour, with the impressions they made on my mind at the time of their occurrence, the observations I shall make will be very concise.

One head appears something larger than the other; but I believe this circumstance arose from the difficulty of the labour. The one, having presented at the os externum for several hours, was tumefied; whilst the other, from excessive pressure, had the sutures considerably overlapped. The difference in the length of the necks, I have observed, arose from the extension I used to effect the delivery of the one, with the counter-pressure upon the other.

The child was well grown, as its weight intimates; the members were well formed; and the arteries and nerves proportionate to the parts they supplied.

"The annals of the history of man," says M. Fournier, "are filled with extraordinary facts of the aberrations of nature from the ordinary state in the phenomena of conception."

Deviations from the strictly natural figure, to the practitioner in midwifery, are not unfrequently met with. The most common are supernumerary toes and fingers, malformation of the parts of generation, and spina bifida. Cases similar to the present very rarely occur: few so singular are on record; and, of those related, but a small share are well authenticated, or else they were expelled before the full time of utero-gestation. Yet the history of rare and uncommon cases, the truth of the major part of which cannot be doubted, with valuable and interesting remarks, are not wanting.* A case similar to the one I have related, with a few exceptions, was recorded not long ago by that late excellent anatomist, Paul Mascagni.† It ap-

* See an article, entitled *Cas Rares*, in the *Dictionnaire des Sciences Medicales*, par M. Fournier. Also, Mr. Mason Good's *Physiological Nosology*.

† *Giorn. della Soc. Medic. Chirurg. di Parma*, vol. xv.

pears, from his description, that its external conformation was exactly the same; the difference existed in the viscera and circulating system.

It was my intention to have procured this fetus, and to have presented it to the College of Surgeons; but, public curiosity being excited from such an extraordinary circumstance, the father of the woman has contrived to make it a source of gain. It is beautifully preserved in spirits, and they are now exhibiting it as a curiosity in the neighbouring towns.

Bolton-le-Moors, Lancashire; Oct. 1820.

Case of Re-union between two Portions of the Finger, after they had been for a considerable time separated. By Mr. MILES MARLEY, Member of the Royal College of Surgeons.

A LAD, aged 11 years, whilst playing near some new buildings in the neighbourhood of Chelsea, had one half of the phalanx of the index-finger of the left hand severed off by a flag. He was brought to me about ten minutes after the accident; when I found the stump, as well as the whole of the hand, more especially the middle finger, very much bruised and lacerated. I immediately sent the friends of the boy to search for the separated part; and, during their absence, I was employed in clearing the hand from the dirt, &c. with which it was covered. The mother returned in about twenty minutes with the separated part, which was quite cold and of a livid colour. Having washed it, I brought the lacerated surfaces into contact by means of adhesive plaster, and desired that the arm might be kept completely at rest. It was examined on the fifth day, when perfect re-union had taken place. The nail came away in the course of eight days, and the inflammation that followed was so slight as not to require any attention. The second dressing was not applied till the tenth day, after which it was dressed every other day.

It is now nearly three months since the accident occurred: the lad has perfect motion and sense of touch in the finger, and the nail is almost wholly regenerated.

*Vigo-lane, Burlington-Gardens;
January 1st, 1821.*

COLLECTANEA MEDICA:

CONSISTING OF

ANECDOTES, FACTS, EXTRACTS, ILLUSTRATIONS, &c.

*Relating to the History or the Art of Medicine, and the
Auxiliary Sciences.*

*Floriferis ut apes in saltibus omnia libant,
Omnia nos illidem depascimur aurea dicta.*

(This department is vacant, in consequence of the great and unusual extent of the Journal occupied by the Original Communications which it has been necessary to insert in the present Number.)

CRITICAL ANALYSES

OF

RECENT PUBLICATIONS, IN THE DIFFERENT BRANCHES
OF MEDICINE AND SURGERY.

"I would have men know, that, though I reprehend the easie passing over of the causes of things
"by ascribing them to secret and hidden vertues and properties; (for this hath arrested and laid
"asleepe all true enquiry and indications;) yet I doe not understand but that, in the practical
"part of knowledge, much will be left to experience and probation, wherein to indication cannot
"so fully reach; and this not only in *specie*, but in *individuo*. Yet it was well said, *Vere scire
"esse per causas scire*,"—BACON.

A Practical Treatise on the Diseases of the Eye. By JOHN VETCH,
M.D. F.R.S.E. Member of the Royal Medical Society of Edinburgh,
and of the Medico-Chirurgical Society of London; lately Physician
to the Forces, and Principal Medical Officer to the Ophthalmia
Military Hospital. 8vo. pp. 267; with three coloured Plates.
Burgess and Hill. London, 1820.

WE know not a better exemplification of the advantages
that have resulted from especial study of the diseases of
a particular organ, subsequently to profound and comprehen-
sive knowledge of the principles of general pathology, than
that presented in this work; which is not more remarkable for
the extent of the original observations comprised in it, than for
the ability with which the author has every-where elucidated
the facts he relates by his reflections, and the solidity of the
general principles which he has drawn from them whenever the
extent of existing knowledge would admit of deductions of this
kind. With those merits, this treatise possesses the advantage
of being constructed in a methodic manner. The whole of

the author's descriptions, too, are traced with a degree of perspicuity and precision that is very remarkable, and cannot be excelled; and the impressions they make on the mind, in consequence of the possession of those qualities, are so strong and definite, that it is impossible for any person of ordinary intelligence to mistake or confound any thing which Dr. Vetch has stated. The same characteristics, in respect to strength and clearness, pervade the reasonings with which the descriptive part of the work is throughout accompanied. Altogether, we repeat, this treatise is a very extraordinary production. In endeavouring to show the correctness of those remarks, as well as such an abstract of the contents of the book as may be comprised in a few pages will permit, we shall, certainly, adduce many important observations which had been previously made known by others; but, in the greater proportion of these instances, it is clearly evident,—from the manner in which they are stated and illustrated by reflections and inferences peculiar to the author,—that, although they may have been original with the promulgators of them, they are, as far as regards himself, equally original with the author of this treatise. Although, therefore, by withholding so long from the profession generally, information of the results of his experience, Dr. Vetch has suffered others to anticipate him in the publication of several observations and points of doctrine, he will not lose the tribute due to him, from any one who will peruse this work with due attention; even were the evidence wanting that the greater part of the observations alluded to must have been made at periods prior to those of their publication in any literary record, and when, indeed, hardly any knowledge of the general pathology of the eye had been attained.

It is extremely gratifying, after having perused this work, to compare the knowledge we now possess of the diseases of the eye with that which was attained by English pathologists before the time of Mr. SANDERS. A little empirical information of the appearances and consequences of such diseases, and a moderate degree of manual dexterity, constituted then all the qualifications of an *oculist*, and to such persons the treatment of affections of the eye was chiefly confided; for, though POTT and CHESELDEN, and some other men of eminent talents, had studied them, their efforts were principally directed to the means of relieving them by operations: the consequences were, that, as Dr. Vetch has shown, we knew less of this subject than what was known by the ancients. There could not fail to be some apathy in practitioners in general in seeking for knowledge that would be of but little utility to them, and the deficiency of information was not likely to be supplied by the men who had confiscated to themselves the cure of such diseases.

This falling-off of the state of medical science in this as well as in other instances, commenced under the Arabians in Spain, when surgery was made a distinct department of the healing art; and when this again, in conformity with the custom of Asia and Egypt, was separated into many divisions, each of which was taken as the especial province of particular practitioners. In Germany, this subject has never been neglected by the generality of pathologists; and Italy has produced several excellent writers on it, subsequently to Professor Scarpa: but British ophthalmology has been founded in our own days, for Mr. Sanders was certainly the first (alluding to published observations, not to the acquirements of individuals who, like Dr. Vetch, retained to themselves, for a time, their collected original knowledge,) who gave it a scientific character; and Mr. WARDROP, (Dr. Vetch remarks,) in applying to it the doctrines of modern pathology, especially those established by Pinel and Bichat, has certainly contributed much to render it an interesting study for the general practitioner in medicine: he must therefore be considered one of those to whom the success of this science is much indebted. There seems now to be no fear of this branch of the healing art falling again (whilst our present social establishments continue to exist,) into the hands of mere operators; and the respectable names already mentioned, with those of several other living practitioners, have raised the practice of it to its proper degree of dignity in the profession. The most gratifying consideration, however, is, that it has of late been so successfully cultivated, that there is, perhaps, no class of diseases better understood than those of the eye at the present day.

Adverting to the particular consideration of the work before us, we commence with stating that it is divided into two parts: in the first of which the author treats especially of the diseases which affect the eye itself; in the second, he considers those which are seated originally in the conjunctiva. The former division comprises some preliminary observations on the general Character and Treatment of Ophthalmic Inflammation, and treats particularly of ophthalmitis sclerotica,—ulceration of the cornea,—opaque cornea,—ophthalmitis iritica vel sclerotica interna,—lenticular and capsular inflammation,—and amaurosis: the latter, of ophthalmia, (which term is applied by the author only to inflammation of the conjunctiva,) arising from climate and atmospheric change,—catarrhal ophthalmia,—the purulent ophthalmia of the British army,—gonorrhœal ophthalmia,—the purulent ophthalmia of infants.

The author commences his remarks on the general character and treatment of ophthalmic inflammation, with what will, to many practitioners, appear a somewhat rash assertion: "Local

inflammation," he says, "when it does not involve any of the parts immediately subservient to life, is a disease which we are able to command or subdue. Inflammation, more especially as it affects the eye, unaccompanied by constitutional disease, is always to be regarded as susceptible of being conducted to a successful termination:"—but, before such an appellation can be applied to it with propriety, it is necessary that instances should be adduced of the failure of the means proper for combatting it, when employed in the manner inculcated by the author. In the treatment of inflammation of the eye, he adds, we possess several peculiar advantages, from the singular opportunities we have of observing the phenomena, and ascertaining the precise limits of the disease, as well as from the uniform nature of the changes incident to its several stages; and hence a proportional degree of precision ought to be acquired in its management, especially in regard to the attainment of our object by the fittest method, and in adapting the extent of the means employed to the exigency of the case, as well as in regulating them according to their influence locally and on the system in general.

"In other instances of local inflammation," the author continues to remark, "the actual severity of the disease must of itself decide the extent or force of the treatment; but here, besides the degree of violence, the particular character which it assumes, according to the variety of structure which it occupies, must form an object of prominent consideration. The importance, therefore, which attaches to the difference of practice to be observed, for the cure of the inflammation of parts so very contiguous as those of which we are about to treat, and the characteristics of which I was early led to appreciate, by the strict evidence of facts, induces me to enter into a general view of ocular inflammation; believing, as I do, that the principles I am led to offer in explanation of practical results, will afford the basis of a more simple classification, and present such therapeutical indications as may prove no less conducive to the ease and decision of the practitioner, than to the security of the patient."

On proceeding to the particular discussion of those subjects, the author finds it necessary, in the first instance, to obviate the confusion which has been thrown around them by the indefinite use of the terms applied to them, that has been manifest in the writings of modern authors. Inflammation in two very distinct parts has of late been indiscriminately blended in the present acceptation of the word *ophthalmia*, which was originally employed by HIPPOCRATES as a term for catarrhal or purulent inflammation of the conjunctiva; whilst the different forms of it were distinguished by the addition of specific or qualifying terms, to which the successive writers of the Greek, Roman, and Arabian schools implicitly adhered, but which have long

ceased to convey any accurate or practical grounds of distinction. Dr. Vetch again restricts the use of the word *ophthalmia* to the sense above stated: inflammation affecting the eye itself, and cognizable as an external disease, he designates by the appellation of *sclerotic inflammation*, or *ophthalmitis sclerotica*, as the term which most distinctly describes the seat and nature of the disease. It might appear that *iritis* could not be properly comprised by this appellation, but the author, when treating of this affection, remarks, that, although inflammation may commence in the iris as a primary disorder, while the redness of the sclerotic coat appears to be secondary or symptomatic, "in either case, the formation of iritis is so blended with a modified inflammation of the sclerotic coat, that it is impossible to separate the consideration of the one from the other."

The following table will present a more particular view of the author's classification of diseases of the eye, which, he says, he has drawn up rather as an index to the observations he has to offer, than as an attempt to introduce a better arrangement of the subjects than may have been hitherto proposed.

" Ophthalmia, or Conjunctival Inflammation.

" Sp. I.—Catarrhal ophthalmia, or *ophthalmia mitior*, sporadic; endemic, and epidemic, with or without chemosis.

" II. Puriform, or *ophthalmia purulenta*, *ophthalmia gravior*, the *lippitudo*,* *ophthalmia vera*, *ophthalmia humida* of the ancients; *blephoroblenorrhœa*, and *ophthalmo-blenorrhœa* of the German ophthalmologists.

" a. Ophthalmia of infants.

" b. ————— produced by the infection of ophthalmic virus.

" c. ————— by the infection of gonorrhœal virus.

" d. ————— by the metastasis of gonorrhœal inflammation.

" e. Rheumatic, syphilitic, and arthritic.

" Ophthalmitis sclerotica.

" Sp. I.—Idiopathic, or corneal.

" II.—Iridial, or symptomatic."

The author—apparently deeply impressed with the idea that peculiarities in the *structure* of parts are essentially connected, as causes, with the peculiarities of the phenomena they develop in states of disease,—adduces, in the next place, an anatomical and physiological account of the membranes which constitute the globe of the eye; in which he especially considers the precise boundaries, distinctions, and vascular connections, of the parts in question, as well as exemplifies the influence of those circumstances on the phenomena of several diseases by some appropriate observations. The *cornea*, he remarks, although

* "The misapplication of *lippitudo* to a chronic and glandular disease is perhaps too inveterate to be now rectified; in its original sense, it applies to the acute stage of ophthalmia."

it differs from the proper sclerotica in its minute anatomy, is nevertheless a strict continuation of the latter membrane *as far as concerns its vascular connection*; which appears to be a fact of much importance, for, according to Dr. Vetch, "inflammation does not take place in the cornea, and consequently neither suppuration nor ulceration, until inflammatory action has been set up in the vessels of the proper sclerotica, even when the exciting cause is applied directly to the substance of the cornea. No symptom of re-action is visible, until that part of the sclerotic coat which is nearest the injured portion of the cornea has put on the appearance of inflammation: on the other hand, idiopathic inflammation once excited in the vessels of the sclerotic coat, invariably tends towards the cornea; and there, in consequence of the more destructible nature of the part, the common consequences of inflammation take place, such as effusion of lymph and ulceration." The very important practical indications which are founded on the above-mentioned facts, will be hereafter considered. The observations which ensue, on *iritis*, are derived from a further extension of the same views, and are still more interesting than those just cited: it is said that—

"The intimate connection formed on the internal surface of the coat with the iris and the ciliary structure, would naturally lead us to expect that the inflammation would have an early tendency to seize upon these parts: attentive observation, however, will prove that, in ordinary or idiopathic inflammation, any apparent affection of the iris is more the effect of sympathy than of the actual presence of disease, and that the supervention of *iritis* is, according to my view of the subject, to be considered as a distinct form of sclerotic inflammation, connected with some idiosyncrasy, or morbid diathesis, previously existing in the constitution; and is, for the most part, more insidious in its progress than violent in its symptoms. As, before any degree of acute inflammation can establish itself either in the cornea or in the iris, a similar action has taken place in the sclerotic coat, so the farther progress of disease, in either of these parts, continues to be indicated by the greater or less activity of the inflammation in the sclerotic, and to this appearance too much attention cannot be paid. The feelings of the patient, the stationary appearance of other symptoms, may lull the practitioner into a fatal security, which he will best avoid by making the condition of the sclerotic coat the only safe test of the arrested progress of the disease."

In further support of those views, the author remarks, that injury done to the iris by accidental or artificial wounds excites no disturbance in the part or uneasiness to the patient, and will eventually heal without any troublesome symptoms, if inflammation does not appear in the sclerotic coat also; but in that case the structure of the iris is liable to be destroyed with all the signs of active and acute disease.

Some reflections ensue on the varieties in the character of inflammation dependant on diversities in the structure of the part affected: the violence, as well as the duration of it, the author says, "is, *cæteris paribus*, according to the resistance opposed to the distention of the vessels which have become the subject of inflammatory action;" and on this principle he endeavours to account for the longer duration of active inflammation in the sclerotica than in the conjunctiva. The ordinary course of observation is sufficient to convince us of the truth of the illustration, though we doubt the validity of the arguments adduced in support of the above proposition, as they all turn on the notion that the vessels concerned in acute inflammation are in a state of increased action, or, as the author expresses it, of *reaction* on the irritative cause.

There are some other diversities in the general characters of inflammation of the sclerotica and the conjunctiva which are worthy of attention, as the knowledge of them will aid in the establishment of the diagnosis of the two diseases. In the early stage of conjunctival ophthalmia, the inflammation is most observable at a distance from the cornea, round which the membrane often preserves, for some length of time, its natural appearance. Precisely the reverse takes place in the case of sclerotic inflammation, which invariably appears at the circumference of the cornea, forming a zone more or less complete around it. Intolerance of light invariably accompanies sclerotic inflammation, and is entirely unconnected with that of the conjunctiva. Having thus pointed out the more striking diversities in the character of the two affections just enumerated, the author runs through the inferior points in their history, describing them with perspicuity, and every-where producing important original observations. Chemosis—an elevation of the conjunctiva from effusion of fluid beneath it, dependant on the existing inflammation of that membrane,—he regards as a means of preventing, in the generality of cases where it takes place, inflammation of the conjunctiva extending to the sclerotica, which would otherwise ensue, from the contiguity of the two membranes.

Having discussed the more general laws of ophthalmic inflammation, the author enters into some considerations, of a similar kind, on the principles for its treatment. They may, he says, be comprised under three general heads: "the first, consisting in remedial efforts applied to the system, with the view of diminishing local action, of which venesection is the most certain and the most powerful. The second embraces the means of lessening the immediate impulse of the blood to the diseased part, by emptying the supplying trunks, or by giving a new direction to the existing impetus. The third con-

sists in the application of agents to the immediate seat of the disease, by which the morbid state of the vessels may be altered or subdued."

Inflammation in the conjunctiva is but little affected by venesection, unless this be productive of syncope, which will prove the end or cure of the disease; whilst "the strength and fibre of the patient may be reduced, by abstinence and repeated blood-letting, to the lowest standard, without producing any material benefit, or insuring the organ against the destructive consequence of the farther progress of inflammation." But syncope, produced in the way just mentioned, has no such control over the inflammation which supports itself in the vessels of the sclerotica: in this case, however, the topical abstraction of blood, especially by cupping and leeches, has "sufficient control over the various states and individual symptoms to render any larger bleeding unnecessary." The application of leeches to the conjunctiva of the lower eye-lid, or to the septum nasi, is particularly advised. The exclusion of light from the inflamed eye, is, the author says, "often more detrimental than useful. The eye becomes more irritable, and less manageable when defended from the access of a moderate degree of light: its exclusion can only be permitted during the very early stage of inflammation. I have treated many thousand cases, and I have never suffered a shade to be worn."

"All washes, applied to the external parts of the eye, are for the most part worse than useless: the powerful agency of heat and cold ought to be employed with great precision and with due energy, or not at all. The eye-glass, or cup, used for the application of collyria to the surface of the eye, is troublesome and inefficient. The only proper mode of directing such applications, is by everting and extending the eye-lids, and injecting the fluid over the whole surface, which is best accomplished by an elastic gum syringe."

With many other experienced practitioners, the author considers that blisters near the inflamed eye too often increase the disease they were intended to remove: this is particularly the case with respect to the temples. There is but one exception to this statement as a general rule, which is, that blisters applied to the external surface of the palpebræ, in cases of purulent ophthalmia, tend considerably to diminish the purulency and chemosis.

"The *argentum nitratum* is a remedy which, in the hands of an oculist well skilled in the symptomatology of ocular inflammation, and capable of using it with the extreme delicacy necessary to insure its success, may often supply the absence of every other: the slightest application of it in substance can often remove the highest degree of morbid sensibility to light, and instantaneously restore quietude to

the organ; it can prevent incipient changes, and obviate advanced ones, and may also be used in solution, as a valuable sedative.

“With respect to conjunctival ophthalmia, there is not, perhaps, another disease for which remedies of greater specific efficacy have been found. For the purpose of altering the violent and purulent state of the membrane, it is impossible to possess a medicine of greater efficacy than the liquor plumbi sub-acetatis, infused in an undiluted state. The effect of nicotiana, as a narcotic and astringent, applied externally, is of singular use in abating both the pain and the excessive tumefaction which attends the disease, and constitutes a very valuable addition to our other resources.”

The most powerful and generally-useful internal remedies are belladonna and, more particularly, hyosciamus and stramonium; which, the author says, “the oculist can employ with a certainty more akin to the mensurable nature of physical force, than the uncertain operation of an animal stimulant.”

The most arduous part of the cure of ophthalmic inflammation, results from the disease being but seldom confined to one eye: for, “from the sympathy, contiguity, and exposure of the two organs to the same causes, we have generally the same form of disease to combat in immediate succession.” Here the author makes a remark, which is equally well applicable to many other cases,—that blood-letting so as to produce syncope, although so effectual a remedy for ophthalmia, will not, by any prophylactic power, prevent the disease which it will cure, and for which its repetition, on a new accession, will become necessary. The same may be said of mercury, when it exerts a specific efficacy in arresting inflammation.

Notwithstanding our attempt at the utmost conciseness, we find that an account of the author's general views occupies nearly half as much space as his own exposition of them; but, where almost every sentence comprises original observations of essential importance, it is hardly possible to give an abridged abstract. We could not well neglect to give such a comprehensive account of this part of the work; but we must pass over the particular disquisitions in a more rapid manner, and endeavour only to comprise in an abstract such points as will present something like a general view of the author's observations and doctrines, and recal to the mind of a person who has perused the work itself, the circumstances which are of most essential importance with respect to practice.

As the most serious and destructive effects of ophthalmia are owing to the extension of the inflammation to the sclerotica, the author thinks it convenient to commence his particular disquisitions with inflammation of the membrane just named. He first enumerates the several causes which may give rise to this disease, and which are generally pretty well understood; but

there is a diversity in the original seat of it, in the opinion of the author, as it arises from external or local sources of irritation, and as it results from "some more general and pre-existing disease of the system," which has not been before remarked. In the former case, the inflammation occupies the external surface of the sclerotica, and is termed by Dr. Vetch *scleroticocorneal inflammation*; in the latter, it has a tendency to proceed to the choroideal coat, when it is commonly known as *iritis*: this the author terms *scleroticocoroideal inflammation*. The origin and progress of this affection was noticed in a former part of this article; but we may here repeat, that the author thinks it of great importance that it should be understood that, "whether the inflammation proceeds from the iris to the sclerotic coat, or *vice versa*, the inflammation has its active character and basis in the structure of the sclerotic coat." This form of the disease has a rheumatic character, without any definite termination, and may occur as the local manifestation of a rheumatic or arthritic diathesis. It is frequently met with as a symptom of syphilis; and apparently, also, as the effect of a mercurialized state of the system, where no syphilitic taint can be suspected.

In the first species of sclerotic inflammation—the *scleroticocorneal*,—the first set of symptoms, the author says, may be considered as generic or common to both species, and may be comprehended under the following heads: "*an increased vascularity of the part, morbid sensibility to the impression of light, contraction of the pupil, pain, heat, augmented secretion of the lachrymal fluid, pyrexia.*" In his particular description of the phenomena of this affection, the author says that the encroachment of the vessels on the margin of the cornea,—as they radiate from the angles of the orbit to the centre of the eye,—and an immediate intolerance of light, are inseparable consequences. It is stated that the iris is seldom more safe from any actual attack of inflammation than when this action is proceeding with its utmost violence in the substance of the cornea; and that we never find the urgency of intolerance of light so distressing as in those cases where extensive destruction of the cornea is going on. The morbid sensibility to light keeps pace, too, with the visible inflammation of the sclerotica and the cornea, both in its advancement and decline. These are curious facts, and seem to confirm the opinion of the author, that it is on the morbid state of the cornea, rather than on that of the iris or retina, as commonly supposed, that this increased sensibility to light depends. The proposition is supported by several other, but less striking, arguments adduced by the author; and he remarks that—

"The relation which *photophobia* has to one part of the organ more than another, may at first appear of little importance: the hasty as-

sumption, however, of its being referable to the nervous or efficient structure of the organ, has led to two opposite errors in practice. An inflammatory or congestive state of the deeper-seated parts of the organ, so far from being accompanied by an augmented sensibility, more frequently occasions a contrary state; while, on the other hand, that weakness of sight, or inability to use the eyes in a strong light, often indicates an obscure inflammation in the sclerotic coat, affording two examples of inflammation attended by symptoms the reverse of each other; both require the employment of antiphlogistic remedies, and both are often aggravated by pursuing a contrary practice.*

The rest of the symptoms already enumerated as characteristics of this disease, are then individually considered, in successive order. The most remarkable circumstances respecting the *pain*, are its occurring in various parts of the head and orbit, and being subject to intermission and change of place. The matter secreted by the inflamed sclerotica does not partake of the essential qualities of pus, but preserves the tenacity and other properties of lymph. It seems to be an important point of knowledge in regard to the diagnosis of some affections of the eye, that "matter formed in the substance of the cornea never possesses fluidity." This tenacity of the matter, when it is thrown out on the external surface, is sometimes a source of the greatest distress and most severe pain with which the disease is accompanied, as we have experienced in our own person. The lymph, on becoming somewhat concreted, forms strong bands, adhering to the ulcerated surfaces, which, on being pulled by the eye-lids on every motion of these or of the eye itself, are the means of producing the most excruciating pains. This tenacity of the effused matter prevents its escaping from the little collections which form on the cornea, on these being opened into by a lancet. The protuberance formed by the collection is but slight, and the part thus affected either attains organization by the surrounding vessels ramifying into it, or is separated by ulceration.

The author's general precepts for the treatment of sclerotic inflammation, and some of the particular indications which arise from his particular views of the disease, have been already noticed. We can only add here, that he dwells strongly on the necessity of frequently-repeated local blood-letting, which is, in the generality of cases, the only mean of depletion of this kind that is requisite; though, he adds, "cases will often occur where it will be necessary to take away blood, and that largely, from the system also." Opening of the temporal artery has been strongly recommended, but, after trying it very

* "For the good effects attending the antiphlogistic treatment of this affection, see an Essay by Mr. Stevenson."

extensively, Dr. Vetch has been led to conclude that a smaller quantity of blood obtained by cupping, is of more real service than a larger quantity obtained in the former way. "The force of the general circulation may also be reduced by the exhibition of antimonials, given in such doses as will keep up a considerable degree of nausea, the direct effect of which in abating inflammation is great, while it produces the further good of forcing the consent of the patient to a due reduction of diet." Cold applications, in the way of wet compresses, and making a solution of opium or hyosciamus the means of applying the cold, are often beneficial. In some cases, the occasional use of fomentations, and the vapour of water and vinegar, produce most alleviation. But, the author adds,

"Warm poultices and long-continued fomentations are most especially conducive to the destructive consequences of ophthalmic inflammation,—the relief they may afford being treacherous in the highest possible degree; and so obvious is their tendency to effect relief, by accelerating the destruction of the cornea, that I should consider any patient entitled to recover damages, in whom the disease has terminated unfavourably, whenever it has done so under the application of a poultice."

The application of the *argentum nitratum*, formed into a finely-pointed pencil, to the vessels leading to the cornea, will be found to afford very great relief; and for this purpose it is sufficient barely to touch the surface of the conjunctiva with the greatest delicacy; after which the eye is to be immediately washed, by injecting a little tepid water with an elastic gum syringe. The following remarks, from so experienced a practitioner as Dr. Vetch, are particularly interesting; though the operation to which they relate does not appear to have been much employed by the generality of practitioners.

"When the pain is urgent, and the violence of the symptoms great, Mr. Wardrop has proposed the evacuation of the aqueous humour, by puncturing the cornea. The testimony which I have to offer on this subject goes more to establish the safety than the expediency of the operation. I have more than once had repeated recourse to the operation in the same eye, but at the same time the very necessity for its repetition has proved the effect to be less decisive than seems to warrant its adoption; and I have seldom ever performed it, where I might not with equal propriety have had recourse to it again. From the irritable state of the inflamed eye, it is often troublesome to accomplish; and in some cases, where the timidity of the patient has prevented the completion of the operation, instead of finding any bad consequences to ensue on relinquishing the attempt, the patient never failed to express as much sense of relief as if the instrument had actually penetrated the whole thickness of the cornea."

Some remarks on the use of *hyosciamus* and *belladonna*, as

topical applications; on the beneficial effects of digitalis, internally, when intolerance of light remains, along with copious lachrymation and acceleration of the pulse; the necessity of a low diet; and the utility of purgatives occasionally, terminate the author's considerations on the treatment of this form of the disease. Until it has manifested an evident tendency to exhaust its action in the structure of the cornea, we cannot, he says, be too watchful of the state of the iris; on the slightest affection of which, along with the means of subduing the external inflammation, it will be necessary to combine those which insure the safety of that part. For this purpose, the extract of hyosciamus will be found an advantageous substitute for the belladonna, the effects of the latter being precarious whenever there is much inflammation of the proper structure of the eye.

Ulceration of the cornea, resulting from inflammation, is next considered. It may take place from a similar process having been set up in the conjunctiva, as well as from idiopathic inflammation of its own structure: in the latter case, it often commences by apparent apostematation in the central of the three laminæ of which the part is constituted; and it may terminate by proceeding inwards, so as to produce *hypopion*, or in an external ulcer. The author here adduces an illustration of a proposition which he had before stated: "The first general law regarding the morbid changes which take place in the substance of the cornea, is that, until the effect of an injury done to the cornea is propagated beyond its circumference, and until inflammation is there excited, it retains its natural sensibility. If no such inflammation takes place, the ulceration is conducted by the serous vessels, without producing any visible signs of inflammation to the observer, or pain to the patient." For Dr. Vetch's account of the various appearances, progress, and consequences, of ulceration of the cornea, we are obliged to refer the reader to the work: we can only notice here his explanation of the origin of *staphyloma*, which is new and satisfactory.

When ulceration has perforated the inner membrane of the cornea, the aqueous humour flows away externally, and continues to percolate until adhesion of the sides of the opening takes place or a small portion of new membrane is formed. If a second rupture does not soon destroy this reparation of the internal membrane, it quickly—that is, in a few hours,—projects through the external membrane, in the form of a pellucid and conical vesicle, the size of which increases in a corresponding ratio with the loss of substance which the ulcer may have occasioned in the middle and outer laminæ of the cornea. If the tumor enlarges very much, so as to occupy a considerable portion of the cornea, it is ordinarily termed *staphyloma*; and

the same term, Dr. Vetch contends, should be given to its more limited formation, for in both cases the process is entirely the same. When staphyloma takes place in the centre of the cornea, and is so small as not to extend to the edge of the iris, the vesicle appears colourless from the first; but when the perforation of the cornea has happened within the limits of the iris, and immediate adhesion of that part with the inner circumference of the ulcer occurs,—constituting what is called *proci-dentia of the iris*,—the pupil is drawn to that direction, and the contact of the pigmentum nigrum gives a black hue to the point of adhesion, and communicates the same to the vesicle itself. As the vesicle increases in size, this black hue of it changes into a bluish tinge, whatever may be the colour of the iris. The resemblance which incipient staphyloma bears to the head of the house-fly, has obtained for it the name of *myocephalon*.

In the treatment of this affection of the cornea, it should be borne in mind that the ulcerative process can only be checked, when it is proceeding to destroy the inner membrane, by those measures which are capable of subduing the inflammation on which the action depends. When the immediate perforation of the inner membrane is threatened, we may with propriety resort to the operation of puncturing the cornea, at a place as remote as possible from the ulcer; “the operation being, I conceive,” says the author, “fully warranted both by reason and experience.” The indication of the ulcer healing is easily seen in the diminished activity of the inflammation, relief from pain, and the clear aspect of the part. The injection of vegetable tepid astringent infusions, or of milk and water only, may now be used. Next in importance to a diminution of the action on which the ulcer depends, is the removal of any slough formed either on its surface or in the adjoining part of the cornea. This separation may generally be effected by means of the point of a lancet or a couching-needle, round which the slough may be wound in such a manner as to enable the whole mass of it to be withdrawn at once. When it cannot be removed in this way, slight scarifications will cause it to be thrown off very rapidly. In using the instrument, it should be remembered that there probably remains nothing but the third tunic of the cornea to confine the aqueous humour. Sometimes, but always subordinate to the above indications, we may add some topical applications to the ulcer, as a solution of nitrate of silver, infusion of tobacco, or calomel applied with a camel’s-hair pencil. The application of caustic to the incipient staphyloma, whether accompanied by *proci-dentia* of the iris or not, is another of the secondary measures. The author employs the caustic here in a mode very different from that recom-

mended by SCARPA, who directs it to be applied until a slough is formed. Such an application Dr. Vetch considers would be fatal to the success of the remedy: it is quite sufficient that the caustic barely and instantaneously touches the surface. By this slight application, repeated daily, the incipient vesicle recedes, and the iris, though pointing forwards, is saved from any permanent adhesion. If the caustic touches, by accident, the edge of the ulcer, or any part but the apex of the projecting vesicle, it will often produce much mischief. The diversity in the modes of using this remedy inculcated by Scarpa and Dr. Vetch, appears to depend on the former considering the ulcer as the cause of the ophthalmia, and the latter the ophthalmia as the cause of the ulcer.

When staphyloma is of considerable size, there is much danger of its being burst by slight degrees of external violence; an accident which should, if possible, be avoided, as it is followed, generally, by excruciating pain.

The following observations will show the consequences of the more severe cases of this kind.

“Although the staphyloma when completed should extend over two-thirds of the pupil, that portion of pupil which remains behind the sound cornea suffers no kind of distortion, and is capable of a small degree of contraction and dilatation, though, for the cause I have already noticed, it does not serve the purpose of vision. The iris adheres to the tumor at each side, and that portion of it which remains beneath the sound cornea, whether it includes a portion of the pupil or not, takes the convexity of the natural cornea, but a sufficient quantity of aqueous fluid remains to prevent the consequences which would ensue from the actual contact of these parts: the fibres do not suffer that distortion which accompanies a more partial proclivitas iridis, because, in the latter case, the iris is united to the centre of the part which the ulcer occupied; whereas, in a complete staphyloma, all within the base of the tumor is absorbed, a fact which I have had several opportunities of ascertaining by dissection. The only remains of the iris I found to be a few widely-separated striæ of pigment on the internal surface of the staphyloma. With a staphyloma occupying more than two-thirds of the pupil, it is surprising how seldom the lens is displaced by falling forward into the cavity of the staphyloma. The iris adhering at each side of the tumor seems sufficient to retain this body in its place, and, although the anterior chamber is wholly obliterated, the secretion of the aqueous humour is greater than in the natural state of the part.”

The treatment of this affection has been regarded chiefly with a view to lessen the unsightliness of the tumor, for which purpose its removal, by cutting off a portion of it, has been recommended by Scarpa; whose own evidence, however, warns the reader of the severe inflammation and suppuration which are likely to ensue from the operation.

"There is, however, a much stronger objection to the operation," says Dr. Vetch, "in the fact, that an eye affected with staphyloma is not in so perfectly a hopeless state as it is but reasonable to suppose must be the case after such an operation as the one alluded to. Nature attempts the cure in every case, and in many brings it within the power of art to complete the success of her effort. While the process by which the thin membrane is converted into a white thick coriaceous substance is going on, the repeated rupturing of the parts which have not yet suffered this conversion diminishes the tumor, which, finally, contracts and subsides to the natural shape of the cornea, leaving an indelible leucoma over the place which it occupied. An artificial pupil may then be formed with the same favourable prospect of success as in any other case of obliteration of the pupil from leucoma of the cornea.

"The evacuation of this humour is often required to relieve the sudden and violent attacks of pain which its distention excites in the eye, or in distant parts of the head. Though I never measured the quantity of water which escapes, I think, in some cases, it must have amounted to two drachms. The incision heals immediately, and in a few hours the water is re-produced. I object, therefore, both to the removal of the apex of the tumor, as recommended by Scarpa, as painful and hazardous, besides being destructive of all chance of recovery; and to the puncturing of the tumor, as an endless operation, from which no permanent effect takes place. I have had recourse, with success, to caustic, and to the introduction of a seton in order to accomplish the gradual diminution of the tumor, and to bring the eye into that state where an artificial pupil may be made: at all events, to destroy the deformity of the projection, without the risk of a severe attack of inflammation and supuration of the eye-ball."

The cornea is liable to a very considerable alteration of its shape, without any evident change of its structure, by which it assumes the form of a cone. "In all my attempts to restore the natural convexity of the part," says the author, "I have failed: evacuating the aqueous humour, removing the lens, and the application of long-continued pressure, have not been attended, as far as I know, with any improvement of vision."

Sometimes the cornea suffers a loss of substance, by a process of absorption different from that which takes place when that action is combined with the more obvious signs of inflammation; the result of which is a pellucid dimple. It seems to depend on a removal of the middle lamina of the cornea. It is not productive of much inconvenience; and, when formed, often remains stationary for a very long period.

Opacity of the cornea, consequent on primary conjunctival inflammation, is next treated of. The surface of the eye sometimes presents, from the cause just designated, a rough or granulated appearance, with a varicose state of the external vessels and a secretion of puriform matter; and universal opa-

city of the cornea takes place from the irritation produced by the disease of the conjunctiva. The lining of the palpebræ, especially that of the upper, partakes of the disease, and presents a remarkably villous appearance in the first stage of it; which is succeeded in process of time by deeply-sulcated, hard, or warty, granulations. We must refer the reader to the work itself for a more particular history of this affection.

"In the whole class of diseases of the eye," says the author, "and its appendages, there is no one affection so distinctly pointed out, and its treatment so uniformly described, by all ancient authors, as this state of the linings of the palpebræ, termed by the Greek writers* *trachoma* and *sycosis*, from the granular appearance of the surface; by the Latins, *scabies* and *scabrities palpebrarum*; and by the Arabians, *sebel* †

"So completely, however, had such knowledge been overlooked in this country, that the real nature, as well as the cure, of the complaint may be considered as a recent acquisition; and we have seen the most unworthy claims set up as to the merit of introducing both the one and the other. The use of actual cautery, excision, and friction, for the purposes of curing the diseased state of the eye-lids, may be traced back to the writings of Hippocrates, and these measures seem to have been employed both separately and combined. The destruction of the granulations by friction, appears to have been the practice more generally followed in former times. The adoption of the more preferable mode of practice, by the application of escharotic agents, may, I think, with justice be conceded to Mons. De St. Ives: this author, whom I have not consulted till lately with that attention to which he is so well entitled, appears, in his own country, and at the distance of exactly one hundred years, to have performed a similar service to surgery, which, in our time, has been conferred upon it by the late Mr. Saunders, whose high qualifications and talents have now so successfully established the cultivation of ophthalmological science among us."

Dr. Vetch is decidedly adverse to the treatment of this affection by excision, preferring, as above shown, the application of escharotics. "After giving a fair trial to a great variety of this class of medicines," he says, "applied to the surface of the upper eye-lid in the form of ointment, I was led to trust to their application in substance alone, by which the treatment became so decidedly successful as to enable me to calculate with cer-

* *Trachoma asperitas intra palpebram, est hujus intentio, ut velut incisuras habeat sycosis, a ficus similitudine vocatur, ubi vero diu duravit, et callum contraxit tyosis, id est callositas nominatur, si non his cadet inversam palpebram per pumicem rademus aut per sepia testam, aut fici folia, aut etiam per instrumentum blex haroxyton ab hac opera appellatum.*

† Quum palpebra inversa est interius apparet rubra et aspera, et scabies adest, et cum super alba oculorum et super nigredinem, videtur similitudo panniculi ex venis rubris et crassis texti adest passio quæ vocatur *sebel*.—*Rhases in Regem Almanzorem.*

tainty on the cure of the disease, when not opposed by moral obstacles which no means could control." But, notwithstanding the evidence adduced by the author, as long since as the year 1806, of the superior merit of this mode of treatment, that by excision has been established in the practice of the army: "but," Dr. Vetch asserts, "it never could have received the trial which has been given to it, if left to the unbiassed decision of the profession; and, when time shall have removed all personal consideration, the mode of practice which succeeded with me will maintain its reputation, when that by excision will be left without an adherent." The following recapitulation will show the grounds for the foregoing decision, at the same time that it will point out, more particularly than we have yet done, the mode of employing the means of cure here recommended.

"First, that, of itself, the operation, however frequently repeated, is unequal to the cure of opaque cornea; while, on the other hand, the treatment I adopted in the disease does not require the aid of an operation in one case out of fifty.

"Secondly, that the operation, besides being in itself very painful, requires to be indefinitely repeated, and is often followed by inflammation; while the treatment by the properly-graduated application of caustic substances produces neither pain nor inflammation.

"Thirdly, in many cases where a new and white surface has been obtained after the repeated use of excision, the cornea often remains vascular; a circumstance which never happens when the cure of the membrane lining the eye-lid has been effected by the action of escharotics, properly applied, the cure of the cornea invariably keeping pace with that of the membrane.*

"Lastly, the claim of Sir W. Adams to an improved method of treating this disease, as announced in a circular letter of the Right Hon. the Secretary at War, is limited to the use of a knife in the place of the scissors, as employed by the late Mr. Saunders: to this novelty, if such it can be called, the objections now urged apply with tenfold force.

"The connection of the disease of the cornea with that of the lids, and the eversion of the latter for treatment or examination, was practised by Mr. Saunders, who taught it to Sir W. Adams, and was resorted to by myself, without any knowledge of the practice of Mr. Saunders or the existence of Sir W. Adams; though I did not then, nor do I at this moment, consider the eversion of the eye-lid to be generally necessary to the cure of the disease: its frequent repetition has appeared to me not only unnecessary but prejudicial; and I have met with more success by simply raising it with the thumb of the left hand, so as to admit the application of a small porte-crayon armed with the blue-stone or nitrate of silver, than I ever did by the com-

* Vide Observations relative to the Treatment of Ophthalmic Cases of the Army.

plete eversion of the palpebræ; nor will I yield the evidence of my own sense and experience on this point to any conjectural reasoning whatever. The treatment of this affection forms but one feature of a disease which, at the time that I met with it, was new to the profession in this country; and, although the practice proved successful to the utmost possible degree, it has been condemned without inquiry and without appeal, and in a quarter where professional discussion cannot be entertained.

“The whole power of high and official patronage has been employed to ensure the success of the operation for removing the granulation of the palpebræ by the knife; the failures have been concealed by every possible subterfuge. Nature, however, has proved her superiority; and it is now generally known that the operation, while it has been highly injurious in some cases, has only proved successful when aided by those applications which it was introduced to supersede; and this success has been so limited, as to prove that their use is not yet properly understood where the greatest pretensions have been advanced.”

It will sometimes be advisable to take blood from the temples by cupping, to remove increased action of the vessels of the sclerotic coat, (one of the indications in the treatment,) at the same time that the measures are employed for the cure of the affection of the palpebral linings. The escharotics, for the latter purpose, should be pointed in the form of a pencil, and fixed in a port-crayon, as already mentioned; and they are to be applied, not, as some have conceived, so as to produce a slough over the whole surface, but with great delicacy, and in so many points only as will produce a gradual change in the condition and disposition of the part. It is not necessary at all times that the escharotic should be applied to every part of the diseased surface, for, when a healthy action re-commences, it soon becomes general. As long as any purulency remains, the above applications will be much aided by the daily use of the undiluted *liquor plumbi acetatis*, applied by means of a camel's-hair pencil. When the disease resists these remedies, and the surface becomes hard and warty, the author has had recourse to finely levigated verdigris or burnt alum, and sometimes the kali purum, applied in the way just mentioned; taking care that they are washed off by means of an elastic-gum syringe before the eye lid is reverted. All the substances above enumerated, when used in the way described, are to be here regarded, the author says, as acting, not as escharotics, but as astringents. Some practitioners have attempted to aid their efficacy by astringent washes; but these are always injurious, as they act also on parts which should be preserved from their irritative agency. The solid substances can be applied with precision to the points only on which it is desirable that their influence should be exerted. The *liquor plumbi acetatis* was usually

employed by the author in the worst stage of the disease; and was changed, as the cure proceeded, for a solution of alum, applied by a camel's-hair pencil.

The excision of a portion of the conjunctiva immediately surrounding the cornea, recommended by Scarpa, was employed in the treatment of the cases in the military hospital, under the direction of Dr. Vetch, so as to give it a fair trial; but, it is said, without any good effect, excepting in cases of great relaxation of the membrane covering the eye.

The division or excision of the large varicose vessels which usually unite towards the external angle of the eye, has little or no effect, as new vessels immediately appear in the room of those removed by the operation, and the discharge of their contents does not compensate for the excitement which their excision occasions.

The prognosis in this disease is always favourable; and the more completely the cornea has become soft and opaque, the more easily it is cured; though, if the patient's age is much above twenty, he is for some time liable to a relapse.

The fundamental points of the author's pathology of *ophthalmitis iritica*, or, as it has been commonly termed, *iritis*, have been already stated in this article, and we could add but little to what we have adduced on this subject without entering into details for which it is more proper that the reader should be referred to the work. Many observations are here brought forward in confirmation of the author's proposition respecting the insensibility of the iris, even when suffering inflammation. "Iritis," he says, "when it takes place, which it so often does, without any external exciting cause, may, unless sclerotic inflammation comes on also, eventually destroy vision, by the progress of adhesive action, without giving the patient any further warning than what arises out of the gradual loss of function. The instant, however, that the sclerotic coat reddens, the progress of the primary affection is accelerated and accompanied by the usual signs of acute disease." When speaking of the rheumatic character of *iritis*, Dr. Vetch remarks that "the resemblance which the structure of the sclerotic coat bears to that of tendinous expansion, necessarily disposes it to take on the same form of diseased action to which such parts are subject. The translation of rheumatism and arthritic inflammation to this part is, therefore, consistent with the habitual tendencies of both diseases." The author here, as well as in several other parts of the work, evinces a decided concurrence with PINEL and BICHAT in their views of the influence of structure on the character of inflammation: in conformity with which it may be said, that the peculiar characters of rheumatic

inflammation depend on the inflammation having its seat in a fibrous membrane.

Dr. Vetch is not perfectly satisfied with the explanations of either Dr. FARRE or Mr. TRAVERS of the curious circumstance that mercury, although it seems to act as a cause in the production of this disease, is nevertheless one of the most efficacious remedies for it when established. He offers himself an hypothesis of the relations of those supposed facts, which is founded on the opinion that inflammation immediately results from the absorbent veins not carrying forward the blood with an activity proportionate to that by which it is brought to them by the arterial capillaries. Mercury excites the action of the absorbent veins, and thus produces a due relation in those functions, which is consequently followed by the disappearance of the inflammation. In endeavouring to account for the production of the inflammation by mercury, he says, "in proportion as the functions are excited beyond their natural standard, any sudden check given to the mercurial influence will necessarily interrupt the balance which it has established in the state of the circulation, raised at the same time beyond the natural standard. According as the operation of the interrupting cause is general or local, the effect will be a state of general fever or topical inflammation."

Dr. Vetch, however, in admitting the influence of mercury in the production of iritis, does not seem inclined to regard its operation as direct: he appears to consider it as acting only by "intermediate relations," as Mr. PRING would express it; and, viewing the subject as it is illustrated by the doctrines of causation of this physiologist, there is not the least difficulty in conceiving how the disease may be both produced and cured by means of mercury.*

On treating of the sensible appearances of *ophthalmitis sclerotica interna*, which are in general similar to those of the external sclerotic inflammation, and manifest the same zone of inflamed vessels surrounding the cornea, Dr. Vetch says, that a more leaden hue of the surface of the sclerotic coat, and the want of disposition to encroach upon the cornea, chiefly indicate the more internal course of the disease. The intolerance of light is less troublesome than in corneal inflammation, and,

* The following corollary remarks, although chiefly interesting in another point of view, relate too intimately to the etiology of iritis, to permit us to pass them over here. Dr. Vetch says, "That the use of mercury materially aids the absorption of venereal virus into the system, when it would otherwise produce only a local disease, is now admitted: when given at all, it should therefore be persevered in until it has enabled the system again to expel the virus which it has absorbed. The small quantity of mercury given by some practitioners to secure the system, as they say, is, no doubt, the fertile cause of constitutional symptoms."

as the disease extends itself more and more to the iris, this symptom, instead of increasing, sensibly diminishes. Flashes of light are often supposed to be seen, when the irritation of the iris is great; for the same reason that noises are perceived in the ear, when the organ of hearing is irritated.

In the worst cases of this disease, besides the lesions already mentioned, such a change in the structure of the sclerotic coat is liable to happen, that it can no longer preserve its regular spherical form, but suffers a partial projection in some part more than another, known by the name of *staphyloma sclerotica*. Hypopion, cataract, and amaurosis, are other occasional consequences of sclerotic iritis; the whole of which, as well as the more ordinary progress and consequences of this disease, are minutely and perspicuously described by the author, with all the originality of manner which marks the rest of the work.

The treatment of this affection should not materially vary from that proper for ophthalmitis externa, except in the use of mercury and belladonna, or hyosciamus, from the commencement; and, from the importance of the organ here affected, in the extreme vigilance and activity necessary in the employment of local blood-letting. "A small quantity of mercurial ointment with opium rubbed into the eye-lid and temple, night and morning, seems," Dr. Vetch says, "to have a specific agency in arresting the progress of the disease in the iris. The same may be said of the local use of stramonium and hyosciamus, which not only prevent the contraction of the pupil, but, after the removal of inflammation, appear very evidently to increase the mobility of the iris, by disengaging it from the adhesions it has formed with the capsule of the lens."

Dr. Vetch concurs with former experienced writers, in stating that, although the disease has occurred during a course of mercury, we may renew the exhibition of this medicine with safety and advantage. The local application of it, in the way just designated, will be sufficient when the iritis is not connected with syphilis, in which case it should be employed in a more effective manner: the rest of the treatment is not to vary from that which is proper when the disease arises from other causes. The recurrence or increase of pain, it seems important to remark, is always regarded by the author as an indisputable and unequivocal symptom of augmented inflammation.

Lenticular and capsular inflammation, and its consequence, *cataract*, are next treated on. Besides the inflammation from which cataract most frequently originates, the lens is subject to a more acute and destructive form of inflammation, which, in contra-distinction to that producing the more gradual loss of transparency-terminating in the lenticular and capsular cataract, may be termed *lentitis*. Cataract exemplifies the adhesive

process of inflammation,—this the suppurative. When suppuration is established, ulceration of the cornea comes on in process of time, and the aqueous humor is evacuated. The author seems to regard the diagnostic remarks of cataract and amaurosis proposed by Professor BEER as the most accurate. Professor Beer says,

“ First, in cataract, all objects, especially white ones, appear involved in a thin cloud or mist; second, the vision diminishes in proportion to the visible cloudiness behind the pupil; third, the cloudiness shows itself most distinctly towards the centre, seldom at the border of the pupil; fourth, as the cataract increases, a blackish ring is observable at the border of the pupil, especially in light-coloured eyes; fifth, at first cataract obstructs the vision of objects directly opposite to the eye, but, when viewed sideways and in a moderate light, they are discerned with tolerable clearness; sixth, dioptric glasses aid the vision of cataract patients, so long as the cloudiness behind the pupil is inconsiderable: the flame of a candle appears to an eye in which cataract is forming, to be surrounded by a whitish circle or vapour, which appears broader the farther the patient removes from the light: if the cataract be completely formed, the patient can no longer see the flame, and can merely say where it is. Lastly, incipient cataract does not influence the mobility of the iris; if, at last, its movements are impaired, the complaint is by that time sufficiently obvious. On the other hand, the appearances which characterize the formation of amaurosis, are the great depth at which the cloudiness appears behind the pupil on looking at the eye sideways; from the cloudiness appearing somewhat concave; from its colour being more of a greenish or reddish hue. The diminution of vision bears no proportion to any perceptible cloudiness; the pupil is more or less expanded, the iris little or not at all movable; the pupillary border angular, and the pupil not perfectly circular; the cornea loses its natural and healthy aspect. In incipient amaurosis, there is also a remarkable increase or diminution of vision, not affected, as in cataract, by the degree of light or expansion of the pupil, but depending on physical and moral causes, affecting the sensibility of the individual: violent emotions of mind often giving a temporary increase of vision; while it is evidently diminished by long fasting, restless nights, great anguish, sudden fright, or excessive venery: under the operation of such causes, incipient amaurosis often terminates in permanent blindness. To the amaurotic patient, the flame of a candle appears as if involved in a mist, but, unlike the white cloud already described, exhibits, as well as the flame itself, the colours of a rainbow. Glasses are of no use to the amaurotic patient, and he distinguishes objects at the side with as much difficulty as those directly opposite to the eye.”

All the species and complications of cataract are then considered; for an account of which it is absolutely necessary that we refer to the work. The general principles for the treatment of the two kinds of cataract, independent of the operations for

them, must be manifest on the slightest consideration on the pathology of those affections: the greatest difficulty generally consists in determining the influence of local or of constitutional exciting causes. To these remarks we may add the citation of the following paragraph:

"Practitioners, who give themselves little trouble in investigating the particular nature of cataract, too often satisfy themselves and their patients by telling them to do nothing, but wait till the cataract is ripe or fully formed,—that is, until they are deprived of all useful vision. It is obvious, however, that in all cases depending upon inflammation, however slow and however obscure, it is the duty of the practitioner to prevent the further formation of the disease, by the means directed to subdue such diseased action; and, generally speaking, the same line of treatment which has been recommended for iritis, is equally applicable to the early stage of capsular inflammation; and it will be greatly aided by the use of setons or issues and rubefacients."

Amaurosis is the next subject of the author's observations, and it furnishes him with an occasion for adducing some forcible illustrations of a pathological principle, apparently true, which he seems particularly desirous to establish, and the knowledge of which is of great importance in regard to practice. He had already remarked, on speaking of iritis, that inflammation of the nerves of the senses properly is not accompanied with increased sensibility, as is the case with the other nerves; but, on the contrary, with a diminution of their proper sensibility: hence, inflammation, or increased vascularity of the retina, instead of being attended with increased power of perception of light, is productive of amaurosis. It is common to hear this affection spoken of as a state of *debility*, or want of excitement of the optic nerve, and not of inflammation, on the analogy that irritation of nerves in general is accompanied with increased sensibility; but, Dr. Vetch remarks, it will be found that the causes of amaurosis are often those which are productive of increased determination of blood to the head, and to the eyes especially; and he, by a series of arguments, shows that amaurotic blindness may be the consequence of direct inflammation of the retina, or of the vascular structure on which it rests. But, whilst he establishes this point, he takes care also to point out that it may in some cases arise from "local debility influenced by repletion of the system, obstructed circulation," and from "actual loss of power, from the natural decay of age or protracted debility." He however considers that, excepting when it occurs at a very advanced period of life, it is almost always formed out of some pre-existing disease. After the preliminary pathological reflections above designated, the author gives a very perspicuous analysis of the symptoms, with an account of the disordered sensual phenomena, and a description of the va-

rious appearances of the eye, which accompany this affection: he then passes in review its causes in a more particular manner, and concludes the chapter with a few remarks on its treatment. This, as his pathology indicates, should in many cases consist in antiphlogistic measures; and blood-letting, productive of syncope, will often be necessary to subdue the inflammation of the retina, or of its subjacent membrane, on which it depends. Purgatives, antimonial emetics, pediluvia, seclusion from the light, cold applications to the eyes, blisters and rubefacients to the neck, and the like measures, should be employed according to the indications present.

"It will be long," says the author, "before the utmost success of a rational treatment can compensate for the cases which have been rendered irremediable by the empirical use of stimulants, and more especially of galvanism and electricity. These even at the present day are resorted to, and recommended without any reference to, or discrimination of, the causes to which the disease is owing. What can be supposed more mischievous than the application of electricity to an amaurotic eye, when it is perhaps occasioned by some organic disease, and thus stimulating a part already suffering from over-excitement. Without first removing the primary affection; by a stimulating plan of treatment, we not only take away all power of recovery from the part, but very likely bring on a state of actual suffering in addition to blindness for life."

When it proceeds from derangement of the stomach, Dr. Vetch recommends antimonials in small doses, which he has combined with the *arnica montana*. RICHTER is known to have been particularly successful in the treatment of this affection: his chief remedy was an emetic, repeated once or twice a-week; and this mode of treatment, with tartar-emetic administered to the extent of only one grain daily, on the intermediate days, has of late acquired much reputation on the continent.

We now arrive at the second part of the work, which is on inflammation of the conjunctiva. *Catarrhal ophthalmia*, arising from climate and atmospheric changes, which the author says are the most frequent exciting causes of ophthalmia, is the first subject for illustration. Here, as on former occasions, Dr. Vetch commences with some preliminary observations and reflections on the general pathological relations of the peculiar structure of the part which is to become the subject of his especial considerations; and thus, after the manner of PINEL in his Nosography, he prepares the reader for the ready comprehension of the particular details into which he subsequently enters. After pointing out, with accuracy and conciseness, adducing here and there original observations, the most remarkable circumstances in the pathology of mucons membranes, and showing their especial liability to become diseased from atmospheric

influence, the author adverts to the ophthalmia of Egypt,—a country which, of all others, seems to be the most favourable to the production of inflammation of the conjunctiva; and the causes of which he, in the first place, endeavours to determine. He shows, satisfactorily, that it is not, as it has commonly been supposed, on the sands; or the influence of the drying winds, that it depends; but on the extreme humidity and relative coldness of the atmosphere during the night. An opinion very early published by Dr. Vetch, in the fourth volume of the *Edinburgh Medical and Surgical Journal*, and in which ASSALINI and Mr. POWER coincide with him; though each of them seems to have been led to it, nearly at the same time, by their own observations.

Some remarks ensue on the catarrhal ophthalmia of Europe. The author applies the term *catarrhal* rather than *purulent* to the ordinary ophthalmia of temperate climates from atmospheric influence, although the matter formed “may, and does,” he says, “generally assume more the properties of pus than mucus;” but he chooses to reserve the term *purulent* for the more severe form of ophthalmia, which occurs from inoculation or infection. He considers, and apparently on good grounds, that ophthalmia, like fever, may first arise from atmospheric influence, and then afterwards be propagated by contagion, by the matter formed being communicated from the eyes of the diseased to those of the healthy; a proposition which he has satisfactorily and originally established in regard to the ophthalmia acquired by the British army in Egypt, in the commencement of the present century, and afterwards propagated in several parts of England and Ireland, when the soldiers affected with it were dispersed (a very interesting account of which, full of important information and suggestions for military officers and army medical practitioners, is given by the author,) on the return of the army. Dr. Vetch, indeed, goes so far as to advance as a general proposition, that, “from whatever cause inflammation of the conjunctiva may originate, when the action is of that nature or degree of violence as to produce a puriform discharge, the discharge so produced operates as an animal virus, when applied to the conjunctiva of a healthy eye.” If this proposition might be considered as well established, (and the accuracy of observation, experience, and solidity of judgment, of Dr. Vetch, must produce much aversion to doubt its truth,) what a fertile source of reflection and indications for original pathological researches would be here presented to us. There is no obvious reason why other mucous membranes, the urethra for instance, should not be subject to similar laws; and, if this be the case, many hitherto anomalies and perplexing circumstances in the history and propagation

of gonorrhœa might be readily accounted for. A little speculation will present a multitude of circumstances relating to fever and several cutaneous diseases, that seem to render it probable; but, in the present state of our knowledge, further observations and experiments should precede theory on this subject.

A very curious fact respecting this ophthalmia, analogous to that for which Mr. JESSE FOOTE first argued in regard to gonorrhœa, which Dr. Vetch has ascertained, is that the purulent matter from the eye of one man has the power of infecting the urethra of another man, and of producing gonorrhœa; whilst it is innoxious to his own urethra. This law the author has never known to be violated. This subject is more extensively discussed in the chapter on *gonorrhœal ophthalmia*, and the discussion necessarily comprises some reflections on the laws of metastasis of diseased action; because it has been made a question in respect to gonorrhœal ophthalmia, whether the ophthalmia arises from *metastasis*, (or, as it is with more propriety termed by Mr. PRING, *related extension of disease*,) or from the application of the gonorrhœal virus of the patient, or of another person, to the eye which has become affected.

If ophthalmia be the result of the application of gonorrhœal matter of the same person to the affected eye, it is obvious that the law above stipulated for is violated; and ophthalmia co-existing with gonorrhœa would appear to be too frequent an occurrence to permit the supposition that it has arisen from the contact of gonorrhœal matter of another person. Although the attempt to excite ophthalmia by the application of gonorrhœal virus is too hazardous and imprudent an experiment, because of the dangerous consequences that might result from it, the converse of this—when the great importance of the question in regard to military discipline, and the fact that soldiers have commonly wilfully attempted to produce diseases, (even the Egyptian ophthalmia,) are considered,—may (when the consent of the parties is obtained, and when it was thought to be not improbable that, if gonorrhœa were excited, it might relieve the disease of the eye,) be considered justifiable. Dr. Vetch, therefore, applied the matter formed by the eye in ophthalmia to the urethra in several instances, but no disease ever resulted from it when the inoculation was effected in the subject from which the matter was taken; whilst, in one case, (the only experiment, it appears, of this kind,) the ophthalmic matter produced very severe gonorrhœa in another individual than that from whom it was taken.

From the results of these experiments, Dr. Vetch was led to regard the converse view of the subject as a fact: he says,

"From the result of these cases, I could no longer admit the possibility of infection being conveyed from the eyes to the gonorrhœal discharge of the same person. Some time after this the improbability, or rather impossibility, of this effect, was rendered decisive by an hospital assistant, who, with more faith than prudence, conveyed the matter of a gonorrhœa to his eyes, without any affection of the conjunctiva being the consequence. From this time I was led to look for an explanation of the connection subsisting between gonorrhœa and ophthalmia, arising in the same person, in some peculiarity of the constitution; and to conclude that the disease is an extension of an inflammation which first showed itself in the urethra, and of which the different structures of the eye are liable to participate, in common with many other parts."

It happens, unfortunately, that the author has not made it evident whether the experiment of the hospital assistant was made with the matter of a gonorrhœa affecting himself or another person; but we must, with almost absolute confidence, suppose it were from gonorrhœa affecting himself, (from the nature of the context, and Dr. Vetch in another place shows that ophthalmia may be produced by the contact of gonorrhœal matter;) and, if so, it might appear that the conclusion immediately following the account of it, is not quite satisfactorily established, because the gonorrhœal ophthalmia ordinarily witnessed may arise from the matter of another individual coming in contact with the eye of the patient, and probably at the same time that the gonorrhœal infection has been received. Dr. Vetch had previously opposed this view of the subject, by the statement that gonorrhœal ophthalmia occurs too frequently to admit of the probability of its correctness.

In support of the above-mentioned views, Dr. Vetch brings forward a case where the ophthalmic affection accompanying a gonorrhœa was accompanied also with an attack of rheumatism in other parts, and the ophthalmia, as it generally is in analogous cases, was *sclerotic*, not *conjunctival*, inflammation; whilst that produced by inoculation affects only the conjunctiva.

The inflammation of the sclerotica is very frequently, in the cases under especial consideration, communicated to the conjunctiva; and so is decidedly idiopathic inflammation of the sclerotica from other causes. It is but seldom, Dr. Vetch believes, that "the gonorrhœal action is translated to the conjunctiva, without attacking the sclerotic coat also." These facts strongly favour the author's opinion of the nature of gonorrhœal ophthalmia as it most ordinarily occurs; and several others, which our limits will not permit us to notice, are brought forward by him in its support.

The author says but little respecting the treatment of gonorrhœal sclerotic inflammation; but we may suppose that he does

not consider that it should differ materially from sclerotic inflammation arising from other causes. There is, however, in this affection reason to fear a particularly rapid destruction of the cornea; and the warm poultices and fomentations recommended by most authors, are said by Dr. Vetch to be the most effectual means of assisting nature in this destruction. "Urged by the extreme necessity of the case," he adds, "I have been led to try a very free use of cold; and, from the trials I have made, I think it appears to be the best calculated to remove the disease. By the use of cold, I mean something more than the application of wet rags or compresses over the eye. The liquid applied should be cooled to a very low temperature, and renewed as soon as it ceases to be felt cold."

The subject of the last chapter of this work is the *purulent ophthalmia of infants*. The most remarkable point of diversity between this affection and the conjunctival ophthalmia of adults depends on the greater facility with which inflammation is transferred from the conjunctiva to the sclerotica in infants than in the latter subjects; and hence destruction of the cornea occurs in the former with much the greatest rapidity. The author mentions as its causes, more especially, some morbid secretion in the vagina of the mother, and an atmosphere infected with animal effluvia; and hence, that is from the latter cause, it is a disease of very frequent occurrence in lying-in hospitals, where the greatest attention is not paid to ventilation. Want of due cleanliness, and negligent exposure of the head to inordinate cold, seem also to be productive of it. Sometimes it ensues as a consequence of chronic inflammation of the glands of the eyelids. The disease, the author adds, seldom ceases in less than six weeks, and often continues from three to four months before the patient opens his eyes without assistance.

In the treatment, leeches will generally be advisable throughout the whole of the disease; and, "on the first accession of tumefaction, the best effect will often be experienced by the insertion of a small portion of ointment composed of lard, butter, or any animal fat, without wax, with a proportion of ten grains of the red nitrate of mercury to six drachms of the ointment. As the purulency advances, the liquor plumbi acetatis will be found no less serviceable than in other instances of purulent ophthalmia. It will be necessary, however, to ascertain that it has been boiled sufficiently to evaporate any free acid which might otherwise remain." When slough forms, a solution of nitrate of silver proves highly serviceable in assisting in its separation; and the recovery of the healthy state of the relaxed conjunctiva may be aided by the use of a solution of alum, or of the sulphate of copper, either dropped into the eye or injected with a syringe.

On revising the foregoing abstract, we have been somewhat perplexed in considering how we should act with it; for, although we have avoided, almost absolutely, producing any corollary observations or remarks, and have selected, almost exclusively, those parts of the work that are of a general character,—leaving a multitude of views subsidiary to them, as satellites are to their planets; quite unnoticed,—we find that it occupies an extent which is ill compatible with the numerous existing claims on the space of this Journal. But, those who agree with us respecting the character of the work will, probably, not wish that any thing we have here adduced from it had been omitted; and, in extending erasures further than we have done, we should charge ourselves with wilful injustice to the author, in consciously rendering the account we have attempted to give of his book less adequate to it than our abilities would permit.

A treatise of such a character as this cannot be expected to come frequently under our consideration. There is not another class of diseases than that which is the subject of it, that will, perhaps, admit of so much additional illustration: and we cannot venture even to hope that such an extensive series of important original observations will often be withheld from the public until they have been reflected on for twenty years,—by a man, too, to whom it is barely justice to assign the qualification of genius,—in an age like this, when every one who thinks he has got an original idea, hastens to swell it out to a great volume, which, like a bubble of soap-lather blown from the end of a tobacco-pipe, floats about for an instant; glitters with a few rays reflected from the little boys who stand round and gaze on it with admiration; then bursts, from its vacuity, when the vapour with which it was inflated has cooled; and disappears without leaving behind it even a vestige of its existence.

CRITICAL ANALYSES

OF

RECENT PUBLICATIONS, IN THE DIFFERENT BRANCHES OF
MEDICINE AND SURGERY,

In the Literature of Foreign Nations.

Παρίδες ἄρα
ἀνδρῶν, ὃ παρὰ τοὺς ἀνδρῶν, ἀγαλλόμεθα.

The Medical and Surgical Register: consisting chiefly of Cases in the New-York Hospital. By JOHN WATTS, JUN. M.D. VALENTINE MOTT, M.D. and ALEXANDER H. STEVENS, M.D. Part II. Vol. I. 8vo. pp. 240. Collins and Co. New-York; and Souter, London. 1820.

THE case first in order is that of "*An extraordinary Tumor, successfully Extirpated,*" by Dr. STEVENS.

The principal remark we have to make respecting this case, is that it presents a very favourable specimen of the degree of perfection to which operative surgery has been raised in the United States. The tumor in this case seems to have been of a carcinomatous kind: for "a scalpel pushed into it, imparted to the fingers the grating feel of bricket, or intervertebral substance;" and "it contained several cavities, from which a colourless fluid was discharged." It extended laterally from the situation of the mastoid process on the right side, beneath the tongue and the cavity of the mouth, to one inch beyond the symphysis of the lower jaw, two inches beyond the trachea on the left side, and downwards to two inches and a half below the clavicle. It hung pendulous externally to the integuments of the neck, excepting at its base, which was attached, as already indicated, to the parts bounded by the mastoid process, the floor of the mouth, the larynx and two inches beyond it on the opposite side, and the inner edge of the sterno-cleido mastoid muscle. Its original adhesion to the parts in this course had probably been increased by the inflammation produced by an awkward and unsuccessful attempt at its extirpation on a former occasion by another surgeon. It weighed, after the fluid it contained was evacuated, three pounds and a half. The dexterity and professional ability manifested in the operation, will be shown by stating that the wound left by the operation "exhibited the pulsation of the external carotid; the ninth pair of nerves; the digastric, the stylo-hyoid, and the mylo-hyoid, muscles; and in general all the external layer of muscles which elevate or depress the os hyoides and the cartilages of the larynx;" and that the operation was effected without the loss of any considerable quantity of blood, (but which was nevertheless sufficient to make Dr. Stevens advert to the "advantage of cutting and securing the trunks of vessels, before their branches are divided," on similar occasions,) and with the most favourable result.

The next case is one of "*Extensive Infiltration of Urine successfully treated*," by Dr. STEVENS. This was an ordinary case of infiltration of the scrotum and adjacent parts, from rupture of the urethra consequent on stricture. The most interesting remarkable circumstance respecting it, is the recovery of the patient, although five days had elapsed since the rupture occurred before he was brought into the hospital, and before any scarifications were made in the integuments; so that extensive mortification had already commenced when this measure was first resorted to.

Dr. Stevens remarks on this case, that it illustrates "the mischievous effects of violence in the use of the catheter, as tending to produce a rupture in the urethra;" but it does not appear, from the history of the case, that the use of the catheter had any thing to do with the production of the "mischievous effects," or that a catheter had ever been used previously to the occurrence of the bursting of the urethra, which seems to have happened in the ordinary way. The patient had a stricture, which became gradually worse; and one day, on "straining to empty his bladder, he felt that the bladder was partially emptied; for, although no water was discharged from the meatus urinarius, his scrotum and perineum began to swell."

Dr. Stevens then takes occasion to reprobate "the looseness with which writers describe the degree of force which should be employed; in the various operations of surgery, in directing *moderate* force to be employed;" and he suggests, by way of amendment, that "it were better if surgeons should say to their pupils, you may employ for the reduction of a hernia, a force equal to the weight of ten pounds; you may employ for the extension in the reduction of a dislocated arm, a force equal to the weight of fifty pounds; you may pull upon the cord with a force equal to the weight of three pounds; you may push in the catheter with a force equal to the weight of one pound." If we were disposed to wonder at any thing, we should wonder that a *practical* surgeon could suggest such precepts. They are like the speculations of a man not only unversed in clinical experience, but also unacquainted with the physiology of the human body. A disciple of Borelli and the mechanical physiologists would hardly have fallen on such a project. Were we, also, disposed to urge particular objections to the precepts which Dr. Stevens has adduced as examples of his proposed means of instruction, we should argue for the inadequacy, in the generality of instances, of the force he has proposed as proper for attaining the object in view, in at least one of the cases: thus, a degree of force equal to the weight of fifty pounds would be useless in almost every instance, in an adult, as a means of extension for the reduction of a dislocated arm; and, instead of one pound being the extent of force allowable in the introduction of the catheter, we believe that there are many surgeons, who are considered good ones too, who would think force to an extent equivalent to twenty pounds too confined a restriction in cases of very frequent occurrence.

"*Remarks on the Treatment of Secondary Syphilis*," by A. H. STEVENS, M.D. The principal object of this paper is said by the author

to be "to make known the results of the writer's experience in the New-York Hospital, in the use of a compound syrup of sarsaparilla and guaiacum; for the cure of what are termed cases of secondary syphilis, and other severe forms of disease arising from the action of mercury, in constitutions predisposed to scrofula." There is nothing in the author's observations that is either new or calculated to be particularly interesting to the English practitioner. We shall, however, extract the following passage, as it presents a summary of the results alluded to.

"The difficulty of distinguishing cases that require a further use of mercury, from those in which disease is kept up by the continued action of that medicine, after the administration of it has ceased, has led me to adopt as a rule, never to prescribe it where it has been previously given in unascertained quantities; and three-fourths of the cases I have met with have been immediately relieved, and, to the best of my knowledge, ultimately cured, by the use of the preparation alluded to and the decoction of sarsaparilla, without the necessity of a further use of mercury."

"*A Case of Contraction of the Vagina, from Sloughing caused by a tedious Labour, in which the Cicatrix was safely divided by a Bistoury to facilitate Parturition during a subsequent Labour;*" by Dr. STEVENS. The title of this paper sufficiently indicates its chief practical interest. The case comes under the class of those which the sagacious physician of the Pyrenees described to Borden as "*un tissu de menus détails, de petits faits, dans le cas d'être prévus par les connoisseurs ou du moins bien traités lorsqu'ils se présentent.*"

"*A Case of Arm Presentation, attended with untoward Circumstances, in which the Child was extracted in a peculiar manner;*" by Dr. STEVENS. The extraction was accomplished by employing force so as to make the body of the child turn in a manner similar to what it does in what is called the "spontaneous evolution," when the breech is expelled before the head in an arm presentation. This was effected by "making an incision (the child being dead) as high up on the back as I safely could," says Dr. Stevens, "with a blunt bistoury passed along the fore-finger; inserting a blunt hook into it, and pulling in a direction obliquely downwards and forwards, (the back of the child presenting to the abdomen of the mother,) so as to bring the buttocks down; and, having perfectly succeeded, I made a second incision, immediately above the hip, introduced the hook as before, and extracted the child, breech foremost."

"*Remarks on the Means of arresting the Course of the Blood through the Tibial Arteries;*" by Dr. STEVENS. The principal object of this article is to recommend the use of a form of the tourniquet invented by Dr. MOORE, of Massachusetts, with the addition of a splint suggested by the author himself, for the purpose of arresting the circulation in the tibial arteries. We have not yet received the Journal which contains the description of Dr. Moore's tourniquet, and must,

therefore, defer until a future time any remarks we have to offer in this paper.

"*Remarks on Fistula Lachrymalis;*" by Dr. STEVENS. This article contains merely an account of Mr. Dupuytren's mode of operating for the cure of the disease above named, which has already been described in a late Number of this Journal.

"*A Case of Crural Hernia;*" by Dr. STEVENS. The history of this case presents nothing new; but Dr. Stevens takes the opportunity which the narration of it offers, for making some remarks on the risk "of cutting the intestine by the incision upwards, as recommended by Gimbernat, Lawrence, and others," and of recommending the use of an instrument of his invention, for the division of the crural ligament, which Dr. Stevens does not appear to have hitherto employed. It has some analogy with the gorget, both in its figure and in the mode in which it effects the requisite incision; and, in saying this, we believe we state what is sufficient to show that it never will be employed by any good surgeon, if the inventor of it be, perhaps, excepted.

"*Cases of Gonorrhœa, cured by Cubebs;*" by Dr. STEVENS. These cases, from the length of their duration, (fifteen days being the shortest period,) under the use of the cubebs, appear to us to present evidence disadvantageous to the reputation of the remedy: the results were certainly much less favourable to it, excepting that no positive mischief seems here to have occurred from its administration, than those which have been published in this country.

The whole of the book before us is not occupied by articles of such little value as those which have hitherto been noticed. The paper next in order is by Dr. WATTS, and it, like the other writings of this physician, contains matters of interest and importance, selected and arranged by a well-cultivated taste, and reasoned on, for the most part, with sound judgment. This paper occupies above 150 pages, and presents an excellent account of the yellow-fever, as it appeared in various parts of the United States in the year 1819; with some reflections, of a general character, on the history, nature, and treatment, of that disease, and on the policy of the American government respecting the means of preventing it. As an abstract of this dissertation was given in the *Proœmium* to the present volume of this Journal, in conjunction with similar accounts of several other treatises on the same subject, we shall not here enter into the review of it; though we are disposed to remark, that Dr. Watts appears to us to have, in two instances, drawn inferences which are not sufficiently well supported by facts. The first of which is, that the yellow-fever is "an aggravated form of bilious remittent fever;" the second, that it is *never* propagated by contagion. It is shown in the compilation above referred to, that evidence has been wanting, in many cases, of the existence of either organic lesion or particular functional disorder in the liver; and, though the endemic origin of the disease in the United States seems to have been proved in the most satisfactory manner, there is still probable evidence that it has, in

some instances, been communicated by means of contagion, after having originated in the way just designated. The notion of the casual origin of animal poison in the way here indicated, is, we are aware, regarded as an unphilosophical inference by some men of eminent talents; but it is, nevertheless, a notion which is entertained by almost every modern physician who has had much experience in the observation of the fevers of Europe, to which the notion is, by them, applied; and, if it is true with respect to the typhous fever of Europe, there are good grounds for inferring, by analogy, that it is also true with respect to the yellow-fever of America and the Antilles.

"Further Remarks on the Case of Ligature of the Arteria Innominala," by VALENTINE MOTT, M.D. Although Dr. Mott makes no particular allusions, it seems that these remarks are adduced in consequence of its having been argued that the case in question was not one of aneurism. As we ourselves expressed such an opinion, (see the Number of this Journal for August, 1819,) we shall transcribe here the whole of those remarks.

"In my first communication of this case, in the Hospital Register for 1818, it is stated (page 50) that 'the subclavian artery, internally and externally to the disease, was pervious.' To this it may now be added, that, where this artery opens into the ulcer left from the wound of the operation, it appears not only pervious, but of the natural size, and the coats free from any diseased appearance. Externally towards the axilla, the artery is somewhat enlarged in diameter, but exhibits no appearance of disorganization of its coats, either externally or internally. About an inch from the ulcer, or just as the artery has passed between the scaleni muscles, there is an irregularly-shaped elliptical opening upon its upper side, large enough to receive the extremity of the fore finger. The edges of this opening are jagged and uneven, and the surface of the artery internally is of a brownish-yellow colour, to the extent of half an inch on the inside of the opening, and more than an inch towards the axilla. The internal coat of the artery has a rugous or puckered appearance; separated a little from the muscular coat, very friable, and evidently in a degenerated state. The opening of the artery communicates directly with the anterior extremity of the sac, which contains coagula; and, upon removing these, the surface of the sac is seen puckered, or thrown into a great number of little folds, giving it at first sight the appearance of containing a number of holes.

"This account is taken from the morbid parts before me; and the preparation has been seen and examined by Dr. Post, Dr. Hosack, Dr. Stevens, Dr. Watts, and others, who have authorized me to state that they are satisfied as to the nature of the case."

"Two Cases of disunited Fractures, successfully treated by Seton;" by Dr. MOTT. The results of former experience in the practice here designated, were stated in a late Number of this Journal, (Nov. 1818.) We shall transcribe the accounts of these cases in detail.

"CASE I.—Stephen Hammond, aged about 35 years, was admitted into the hospital with lameness, in consequence of a fracture of the leg

about seven months since. Upon examination, the tibia was found un-united, and to admit of very free motion between its ends. The fibula was entire, and the patient believed it never had been broken. From the account which he gave, it appeared that he had been subjected to the proper treatment for the restoration of a broken bone; but he stated that it never showed any disposition to unite, under the course which was pursued.

"As his general health was not good, he was put upon tonic medicines and invigorating diet; and I directed that a stimulating plaster of gum-ammoniac and mercury should be applied over the part, with the many-tailed bandage; and splints to reach above the knee and below the ankle, and to be very firmly secured; and I advised him to walk upon it with the assistance of his crutches, as much as the pain would any way permit; informing him that the object in wishing this exercise was to inflame and irritate the ends of the bone, and that he must not desist, even though considerable pain should accompany it. This was persevered in for several weeks; but, finding little or no pain to attend it, and no appearance of inflammation in or about the fracture, and no hope of amendment, it was discontinued. Blisters were next repeatedly tried, but to no purpose. Very powerful shocks of electricity, also, were passed in different directions through the part, but they produced no beneficial effects.

"A seton was next introduced. This I did by making a small incision upon the outside of the tibia, down to the fractured ends; then passing between the bones the stilette of a small trocar, and pushing it out on the opposite side, the seton was readily introduced with an eyed probe.

"In a few days considerable inflammation and pain supervened, which required emollient poultices and the antiphlogistic treatment to subdue. This was soon followed by a copious discharge of matter from the seton, and a collection of pus on the anterior part of the tibia, which was evacuated by a small incision. After five or six weeks, he became sensible of an increase of firmness in the leg; and from this time he was directed to diminish the size of the seton one thread every other day, until it was all removed. It continued to grow stronger every day; and, in a short time after the wounds healed, he was permitted to walk a little upon it, when splinted and tightly bandaged; and in about three months the bone was firmly united.

"CASE II.—John Smith, aged 41 years, became a patient in the hospital in 1819, in consequence of an un-united fracture of the thigh-bone, of twelve months standing. It occurred at sea, and at the same time several of his ribs were fractured. Thirty-six days after the accident he arrived at Halifax, without having had any attention paid to the adjustment of the bones. After his arrival, he states that little notice was taken of his thigh, and no attempt was made to reduce it. He recovered without difficulty from the fracture of his ribs. The several means mentioned in Case I. were tried, but without benefit. The limb was considerably shortened, from the obliquity of the fracture and ends of the bone overlapping. No advantage attending the use of the means referred to; a seton was recommended. In the introduction of

this, much more difficulty was experienced than in the case of Hammond.

"An incision was made on the inside of the thigh, a little to the outside of the artery, so as to come down upon the centre of the ends of the bones, where they overlapped. The stilette was then attempted to be passed between the bones; but this was found altogether impracticable, from their very close contact, even though the limb was changed from one position to another. Instruments of different sizes were resorted to, but they could only be made to pass a very small distance. A gimblet was tried, but very little progress could be made. Having provided for the occasion a *carpenter's bit*, about the size of a large trocar, I found with this a passage could be made with the greatest facility. Then, by making an incision down to the end of the instrument, on the outside of the thigh, a large seton was readily conveyed through between the bones, by means of a long eyed probe.

"After the expiration of three months, the thigh becoming firmer, and much less motion being felt between the ends of the fracture, he was permitted gradually to lessen the size of the seton. The firmness continued regularly to increase; but it was not until after eight months had elapsed, that the thigh had acquired sufficient firmness to enable him to support the weight of his body, by the aid of a crutch.

"It is now more than twelve months since the seton was introduced, and the bone appears to be firmly united. The shortening of the limb does not exceed three inches and a half."

"*A Case in which the Right Carotid Artery was tied, for the safe Removal of a Tumor;*" by Dr. MOTT. The practice alluded to in the title of this paper, seems to be favourably regarded by the generality of modern surgeons. The few points of an interesting character in the history of this case could not be stated, without entering into details which our limits will not admit of. We should however remark, that the operation, which was one of much difficulty, appears to have been performed with great dexterity; but, when we consider the able and successful manner in which a ligature was applied to the *arteria innominata* by Dr. Mott,—an operation which, considering its extreme difficulty, as well as its object, we regard as one of the most admirable specimens of operative surgery,—we can expect nothing from him but manifestations of great abilities in this department of the healing art.

The last article in this volume is an account of "*a Case of Carcinoma of the Penis cured by Amputation, and subsequent Removal of the Inguinal Glands, successfully;*" by Dr. MOTT. The remarks we made respecting the last case are equally well applicable to this also,

Medical and Physical Intelligence.

TRANSACTIONS OF SCIENTIFIC SOCIETIES.

ROYAL SOCIETY of London.—Nov. 23. A LETTER to the President from the Earl of Morton was read, communicating a curious fact in natural history.

The Earl of Morton had received, some years since, a male *quagga* from the Cape of Good Hope. From this and an Arabian blood chestnut mare a female hybrid was produced, which possessed indications, both as to colour and form, of her mixed breed. In the possession of another person, the same mare was afterwards bred from by an Arabian horse: the result was singular; as, although it had the general appearance of the Arabian breed, it bore, in several respects, characters which belong to the *quagga*, such as stripes and marks on the body, and in the hair of the mane.

Case of Extirpation of the Thyroid Gland, by Dr. KLEIN, of Stuttgart.—A BOY, eleven years of age, deaf and dumb, and of a very delicate constitution, had, from his infancy, a circumscribed tumor situate on the left side of the neck, about the size of a nut, and which had gradually increased in bulk. After having been put under the care of several practitioners, he was brought to Dr. Klein. The tumor commenced on the left side, at the margin of the jaw, occupied the whole of the left side of the neck, as far as from the larynx to behind the ear, and down as low as the third rib. Vessels of the size of the finger run over its surface: it was movable with difficulty, rather because of its weight than of its adherence to the parts beneath. It was a little irregular, or tuberculated, in its construction; and the pulsation of the arteries which penetrated its interior could be distinctly felt in several parts. Its removal by the knife having been determined on, the patient was laid on a table, so that his head, shoulders, and legs, might be most effectually secured by assistants. On making the necessary preliminary incisions in the integuments, the veins, which were much dilated, were divided, and threw out about half-a-pound of blood; but they were soon secured by compression with the fingers, as well as the subclavian artery. After the flaps of the integuments were dissected aside, the tumor was forcibly drawn outwards; and, using sometimes the fingers and sometimes the scalpel, or its handle, it was separated from the larynx and trachea. The operation occupied a minute and a half; but Dr. Klein was surprised to find no hemorrhage, even from the thyroid arteries, which were, of course, divided. On examining the patient, however, he was found to be insensible; and, though every proper means was used for three-quarters of an hour, it was found impossible to restore him to life. On opening the body, it was found that neither the carotid artery, the par vagum, the jugular vein, nor the trachea, had been injured; but all the vessels of the dura mater and the brain were gorged with blood:

there was a large quantity of serum in the ventricles. "I think myself authorized," says Dr. Klein, "to conclude that the patient died of apoplexy occasioned by the revolution in the circulation which the operation had caused. The tumor itself was found by the thyroid gland, which had suffered disorganization throughout its whole structure, excepting a small portion on the right side. It weighed two pounds, and its base was of about six inches diameter in every direction. It was formed of a solid, lardaceous, reddish substance, divided into several lobules."

In regard to the cause of death in this case, we would suggest to others the consideration whether or not *moral* circumstances had some influence in causing it. The agitation and suffering of the boy,—being deaf and dumb, and consequently incapable of understanding the object or necessity of the operation,—must have been extremely intense. We doubt whether such an operation on such a subject was justifiable.

Professor Duvuy, of the Veterinary School of Alfort, has made some further experiments (noticed in the *Compte rendu des Travaux de l'Ecole royale d'Alfort, dans la séance du 4 Nov. 1820*), on the inoculation of the *scab* in sheep, which tend to establish very satisfactorily the utility of the practice. At a time when this disease was prevalent in an extensive flock, and of a very confluent form, he inoculated two hundred, of which only three died; whilst the mortality in those who took the disease naturally was in the proportion of one in five. There were, besides, reasons for a supposition that the three inoculated sheep which died had taken the disease naturally previously to, or about the time of, the operation.

We are not aware whether or not inoculation for the *scab* has been practised in England, and we have not the means at this moment, when these observations are required for the press, of ascertaining this point; though we have some reason to think that, if it had been practised, it would not have escaped our knowledge; but it has often been resorted to in France, in Italy, and in Hungary, and always with the most favourable results,—the inoculated animals escaping the disease by infection, and having it, almost universally, in so mild a form as to be free from danger of fatal consequences. We may remark here, that the supposition, which had at one time gained some degree of credit, of the identity of cow-pox and the *scab*, has been proved to be incorrect; and it has also been found that cow-pox will not preserve sheep from the *scab*.

The most favourable parts for the inoculation, for many reasons, are those points of the chest which are devoid of wool. Three or four punctures should be made, to secure the success of the operation: more would produce an inconvenient local affection.

We some time since noticed in this Journal, (April 1819,) that Mr. WILKINSON, of Newcastle, had found signs of inflammation of the spinal marrow, in several instances, in horses which had died of tetanus: Professor Dupuy says, that he has always found such an

affection of this part after death from this disease. He adds to this interesting fact the following observations:—A troop of cows, consisting of fifty in number, was attacked with an epizooty which was prevalent at the time, (in 1814;) when, on the 3d of June, there occurred a violent storm of thunder and lightning. On the following day, fifteen of the animals, which had been previously diseased, died on the next day, almost suddenly. Prof. Dupuy dissected them. He only found considerable disease of the spinal marrow, which was much softened, with effusion of serum into the theca.

Sulphate of Platinum a Test for Gelatine.—Mr. E. DAVY recommends the use of the sulphate of platinum in detecting small quantities of gelatine. From comparative experiments made with it, and astringent infusions, he found, that when the quantity of gelatine was so small as not to be effected by strong infusions of oak-bark, nut-galls, or catechu, still there was an immediate precipitate on adding the sulphate of platinum. Where the proportion of gelatine was so reduced as not even to affect sulphate of platinum at first, the precipitate was immediately produced on boiling the fluid.

The different astringent infusions, as of oak-bark, nut-galls, catechu, &c. do not act uniformly on the various kinds of gelatine: thus, an infusion of catechu would produce no precipitate in solutions of paper-hangers' size; but the sulphate of platinum acts equally on all kinds of size, and throws down precipitates which appear to be always similar, not being affected even by the presence of free acid in the solution.—*Journal of Science, &c.*

On Iodine, and its Existence in Sponge.—We inserted, in a late Number, a paper by Dr. COINDET, on the use of iodine as a remedy for bronchocele; and gave, at the same time, an account of Mr. FIFE's discovery of its existence in sponge: we now find that Mr. STRAUB, of Hofwyl, as early as December 1819, appears to have shown its existence in this substance, and proposed the use of it instead of the *spongia usta* in medicine. In order to obtain the iodine from sponge, the latter, after being burnt, was washed with water, and the solution decomposed by sulphuric acid; and in this way so much was obtained from half an ounce of sponge as to confirm the ideas previously entertained, that its medicinal properties were owing to this substance.

Mr. Straub recommends trials of preparations of iodine in medicine, and thinks that, where salts formed from it cannot be obtained, an alcoholic extract of burnt sponge is much to be preferred to the burnt sponge itself.

Mr. Straub also asserts the existence of iodine in turf. He was led to examine this substance in consequence of the peculiar odour he observed in the neighbourhood of those buildings where turf is burnt. Repeated experiments confirmed this conjecture; and, by acting on two pounds of turf, abundant evidence of the existence of iodine in it may be obtained. It was found also in the cinders of the *helminthocorton*, though in very small quantities.

METEOROLOGICAL JOURNAL.

By Messrs. WILLIAM HARRIS and Co: 50, Holborn, London.

From December 20 to January 19, inclusive.

Day of Month.	Moon.	Rain gauge	THERM.		BAROM.		DeLuc's Hygro.		WIND.		ATMOSPHERIC VARIATION.					
Dec																
20		•03	44	50	49	30	38	30	20	66	67	WSW	SW	Cloudy	Rain	Cloud.
21			49	50	43	30	11	30	17	65	63	W	WNW	Cloudy		Fine
22			38	47	42	30	15	30	09	61	62	WNW	WNW	Cloudy		Rain
23		•04	40	45	33	30	00	29	90	62	61	NE	ENE	Cloudy	Sho'ry	Cloud.
24		•02	33	36	33	29	88	29	87	59	57	ENE	ENE	Cloudy		
25			33	36	30	29	84	29	84	56	56	ENE	ENE	Cloudy		
26			31	33	31	29	78	29	90	57	58	E	E	Cloudy		
27	☾		32	33	31	29	92	29	93	58	60	NE	ENE	Cloudy		
28			31	31	27	29	94	30	00	59	59	NNE	NE	Fine		
29			28	29	26	30	00	29	93	56	57	NE	NE	Fine		
30			28	29	25	29	97	29	97	57	56	E	NE	Cloudy	Fine	
31			27	30	28	29	95	29	93	55	57	NE	ENE	Fine		
Jan.																
1			31	32	27	29	88	29	86	57	56	ENE	ENE	Cloudy		
2			28	30	27	29	74	29	60	58	57	SE	E	Cloudy		
3			28	31	27	29	46	29	35	57	57	E	NE	Cloudy		
4	●		27	31	30	29	41	29	40	58	58	NE	ENE	Cloudy		
5		•05	31	32	33	29	35	29	23	59	62	NE	E	Cloudy	Light Sn	Snow
6		•26	36	40	35	29	20	29	27	63	65	E	NNE	Rain		
7		•11	37	39	38	29	27	29	23	67	67	E	E	Rain	Cloud.	
8		•15	40	43	35	29	21	29	19	66	67	E	E	Fine	Cloud.	Rain
9		•03	39	43	40	29	12	29	10	63	65	SSW	SW	Fog		
10		•13	41	46	40	29	16	29	11	65	67	SW	E	Cloudy		Rain
11	☾	•28	45	49	40	29	16	29	20	67	65	E	ENE	Rain		
12		•19	43	53	39	29	30	29	40	65	65	SW	SE	Rain		Cloud.
13		•09	45	50	39	29	57	29	23	60	63	WSW	SE	Fine	Cloud.	Rain
14		•31	43	45	38	29	15	30	08	65	67	E	ENE	Rain		Fine
15		•13	41	45	39	30	00	30	07	67	63	E	E	Rain		Cloud.
16		•09	43	47	41	30	11	30	17	60	60	SE	ESE	Fine	Cloud.	Rain
17		•11	47	50	45	30	20	30	21	64	62	E	SW	Rain	Cloud.	
18	○		51	54	48	30	18	30	23	65	63	SW	WSW	Cloudy		
19		•08	50	52		30	31	30	27	63	61	SW	SW	Rain	Cloud	

The quantity of rain fallen in the month of December,
is 1 inch and 61-100ths.

MONTHLY CATALOGUE OF BOOKS.

General Elements of Pathology. By Whitlock Nicholl, M.D. 8vo.

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[HIGLEY AND SON, FLEET-STREET.]

TO CORRESPONDENTS.

Several Communications were received about the commencement of the last month, that, from their relation to others which had appeared in the Journal, or their peculiar nature, should have been inserted with as little delay as possible. Our apology for their not appearing in this Number is, that (in consequence of the Proemium occurring in the past month) it was printed off unusually early. These remarks apply especially to the Papers of Dr. GRATTAN and Mr. WALLIS.

Notwithstanding the great extent to which the Journal has, of late, been occupied by Original Communications, we view with much pleasure the large packet of them which is still before us; for, though it has become necessary that we should select from them with much restriction, a very great proportion of them is of so valuable a kind, that we cannot but be gratified on the reception of them. The writers will, we are confident, readily excuse the necessary delay in the publication of them; for, the necessity of this delay, under the circumstances above alluded to, must be as pleasing to the Readers of the Journal as it is to the Editor.

We offer our best thanks to the Oxford Correspondent who has of late shown so much assiduity in our favour; but we cannot insert the account given in his last letter without giving at the same time some authority for the facts related in it. None of our Readers, we are assured, would think statements of assumed facts, speaking with a general allusion, worthy of consideration, unless they are in some way or other authenticated.

TO SUBSCRIBERS.

The increased value of the back Numbers of this Journal, since the publication of the GENERAL INDEX to the FORTY VOLUMES, having caused so considerable a demand, as to oblige the Proprietors to reprint several of the early Numbers, they have determined upon a TEMPORARY REDUCTION in the PRICE of all the NUMBERS comprised in the FORTY VOLUMES, (being No. 1 to 2384) which will, therefore, be sold, during the next six months, at 1s. 6d. each: after which time, they can only be had at the usual price of 2s. 6d.

** * THE GENERAL INDEX to the London Medical and Physical Journal, from Volume 1 to 40 inclusive, comprising an Analytical Table of their Contents, arranged in Alphabetical Order, with reference to the whole of the cited authorities under their Nominal Characters, &c. was lately published by JOHN SOUTER, 73, St. Paul's Church-yard.*

Persons finding any difficulty in procuring the progressive NUMBERS of the JOURNAL, the PROEMIA, or the GENERAL INDEX, will please to address a line to the Publisher, as above, which will ensure a removal of every difficulty.

THE LONDON
Medical and Physical Journal.

3 OF VOL. XLV.]

MARCH, 1821.

[NO. 265.]

NOTICE.

Another of the series of PRÆMIA to the several volumes of this Journal; (which commenced with that to the forty-third volume,) comprising a History of the Progress of Medicine and its auxiliary Sciences for the half-year immediately previous to the period of their production, respectively, was published on the last day of January. One of the especial intentions of those Præmia, is to present a comprehensive view of the state and progress of Medicine throughout Europe generally, and in the United States of America; an object that cannot be effected in the regular monthly Numbers of the Journal, because of the small extent of space which can be there appropriated to this purpose.

Original Communications, Select Observations, etc.

A Letter to the Editor of the London Medical and Physical Journal, in Reply to a Letter from Mr. Dickinson to Dr. Grattan. By RICHARD GRATTAN, M.D. Fellow and Censor of the King's and Queen's College of Physicians in Ireland; Physician to the Fever Hospital and House of Recovery, Dublin; &c.

"Let us be judged, both by the public and among ourselves, rather by our works than by our own conceits."—DICKINSON'S *Letter to Dr. GRATTAN.*

SIR,

HOWEVER reluctant I may be to engage in controversy, I cannot avoid troubling you on the present occasion, in answer to a Letter addressed to me in the last Number of your Journal. I confess that I am not displeased to find that my publication on the "State of Medical Practice in Ireland," should have attracted the attention of the profession in England; and I am particularly gratified to perceive that you have considered the subject of sufficient importance to devote so large a portion of your Journal to the insertion of the observations of Mr. Dickinson in reply to my "Remarks."

In assuming to myself the merit of purity of intention, and of a wish to support the respectability of my profession, uninfluenced by illiberal or selfish considerations, I willingly ascribe to Mr. Dickinson motives equally honourable and conscientious. I have no doubt but that his object is to arrive at the truth, and to assist others in forming a right opinion on a matter of such moment as the improvement of medical practice, in the proper regulation of the different departments of which every member of society is interested. I shall therefore, without further apology, proceed to examine his statements, and

endeavour to set him right as to some points, on which I consider him to be altogether in error.

However attentively he may have perused my essay, he has, in several instances, (I am sure unintentionally,) totally misrepresented my meaning; and, like Don Quixotte in his furious assault on the barber and his basin, suffered his imagination so far to prevail over his judgment, as to create an opposition for the sole purpose of appearing to overcome it. It cannot be expected that I should now occupy my time in correcting Mr. D.'s misconceptions. Perhaps, a more attentive perusal of my "Remarks" may enable him to discover them: at least, I believe the majority of my readers are not likely to fall into similar mistakes. A great portion of Mr. Dickinson's letter I shall, therefore, pass over in silence; although, in so doing, I must be understood not as assenting to his positions, but rather as considering them too unimportant to merit a particular refutation.

The object which I had in view in publishing my "Remarks," was to show the necessity of preserving the different branches of the medical profession as distinct from each other as possible. Convinced of the expediency of this measure, and of the advantages which the public must derive from its general adoption in cities and in populous districts, I argued against the admission into the College of Physicians of practising surgeons and apothecaries; and, in the second part of my "Remarks," suggested a plan which appeared to me to obviate every objection that could be advanced against the practicability of the measure which I supported.

Mr. Dickinson, on the contrary, thinks that the public interests are best promoted by encouraging the class of "general practitioners," or those individuals who practise the three branches indiscriminately. On this point we are completely at issue. I have already explained my reasons for contending that the physician should alone be considered competent to treat those diseases which by universal consent are termed medical; and the names and leading symptoms of the greater number of which, Mr. D. will find by referring to "Cullen's First Lines of the Practice of Physic." When I use the term physician, I do not mean, as Mr. D. would seem to insinuate, "men dubbed doctors without a thorough knowledge of the healing art, merely in consequence of a collegiate residence and academic accomplishments," but physicians practically conversant with their profession, and who have super-added to their "academic accomplishments," which Mr. D. affects to despise, an acquaintance with disease to the full as extensive as can be ascribed to any surgeon or apothecary by the most zealous advocate for either.

That "some diseases are treated principally by regimen and internal remedies, while others, on the contrary, are managed for the most part by mechanical means,"* Mr. D., I believe, will not be disposed to deny; and I can hardly persuade myself he seriously thinks that, differing as they do in their treatment; the same system of study is equally calculated for physicians and surgeons. To the physician belong all those diseases which affect the entire system, and to the surgeon those which relate to fractures and dislocations, to wounds and to operations. Surely, no one who pretends to possess the slightest portion of common sense would affect to believe, or endeavour to persuade others, that, because surgical diseases are often accompanied by constitutional symptoms, and medical diseases by local complaints, the entire should be confounded, and this distinction of diseases so obviously different, termed "an arbitrary and unnatural division;" that, because "it is impossible to define their limits," and "a bond of connection is so intimately established between all the parts of the living system," these circumstances should be represented as sufficient "to defy every attempt to consider diseases separately and by piecemeal, general or local, internal or external, exclusively." As well might Mr. D. assert that morning and evening are the same, and that any distinction between them is "arbitrary and unnatural," because he cannot determine the precise moment at which twilight begins and ends.

Common sense and the experience of ages, however, are so opposed to the opinions of Mr. D. that it is almost unnecessary to dwell on the subject of separating medicine from surgery; and therefore, instead of entering into physiological discussions, or repeating what I have already mentioned in my "Remarks," I would simply ask whether, in the case of a patient affected with a calculus in the bladder, if an operation were considered inadmissible, the physician, conversant with chemistry,—capable of determining experimentally the proper solvent of the calculus,—of ordering the medicines adapted to prevent its increase, and the means calculated to allay irritation,—would not be more competent to treat the disease than a surgeon full of all the knowledge of the dissecting-room? And, on the other hand, if the constitutional symptoms and other circumstances rendered an operation advisable, would not the surgeon then supersede the physician with advantage to the patient? I would ask, whether the physician who devotes his entire attention to the treatment of any particular disease,—be it fever, dropsy, gout, or consumption,—must not, from his extensive practice and constant habit of considering the symp-

toms of such disease, as they occur under different circumstances and in different individuals, attain a more perfect knowledge of the nature of the disease, in its different shades and varieties, than if he were only to meet it occasionally in the ordinary routine of common practice? I would ask, must not the surgeon, who operates for the stone exclusively,—who has devoted his entire attention to ascertain the comparative advantages and disadvantages of the various methods of operating, and the circumstances under which one kind of operation is to be preferred to another,—be supposed to operate with more success than if, professing the entire art of surgery, he were to draw teeth, bleed, cup, operate for artificial pupil, amputate, and trepan, in succession? To this there can be but one answer; and yet Mr. D. asserts, “that some of our most eminent surgeons, considering it impossible to form a line of separation, either on the principle of science or on the ground of public good, practise not only surgery in all its branches, but medicine also: and,” he adds, modestly enough, “both of the departments with the greatest honour to themselves and advantage to society.”

That Mr. D. and some other “eminent surgeons” should advance such opinions, is not at all wonderful: the question is, are the opinions which they entertain warranted by reason and experience? That they are not, I am persuaded; nor am I singular in thus thinking; and, although I am not in general disposed to rest my arguments on the mere authority of any individual, yet, were I to decide on such ground, that of Mr. Abernethy, I contend, is an authority more than sufficient. Mr. Abernethy, a surgeon of as much talent, and at least as eminent, as any of those on whose judgment Mr. D. relies, in the Hunterian Oration delivered by him in 1819, before the London College of Surgeons, which I quoted in my “Remarks,” states it as his opinion, “that the division of medicine into two great departments, which custom has established, seems also to have received the fullest sanction of experience; and, were we not to acquiesce in it, *we should subvert the institutions of society, and throw the whole profession into confusion.*” So much, also, is to be known and done in either department, that, if we invade each other’s province, we must neglect properly to cultivate and improve our own.”

Admitting that Mr. Abernethy formerly entertained different sentiments, this very circumstance is, I think, a strong argument in support of the correctness of those which he now adopts, as it places in a most respectable light his candour and sincerity, and affords the most decisive testimony of his anxiety to discover truth, and of his readiness to acknowledge and to adopt it.

All the remarks of Mr. D. relative to my opinions as to

minute and morbid anatomy, are totally inapplicable; for I nowhere asserted that to the physician dissection was unnecessary. I distinctly stated the importance of anatomy, so far as it is applicable to medical purposes; but intimated that its minutiae, such as the exact course of every nerve and artery, and the origin and insertion of every muscle, were in a great measure superfluous, and that such knowledge should not be obtained at the expense of more useful information. On the subject of anatomy, either minute or morbid, Mr. Dickinson and I, I fear, are not likely to agree; which I regret the more as I presume it is "a favourite pursuit," if an opinion may be formed from the—I will not say "vivid style," but ardent manner in which he speaks of its importance. In his anxiety to correct my errors on this head, after informing me "that every thing we understand concerning the living principle is entirely derived from our knowledge of the phenomena resulting from function," he immediately observes, "that all our knowledge of the organs, and the manner in which their functions are performed, is solely derived from patient research into the mechanism of animal structure;" a position from which I most distinctly and decidedly dissent,—which I pronounce to be erroneous in theory, and dangerous in practice,—which leads me to think that Mr. D., notwithstanding his anatomical arguments, cannot be acquainted even with the first principles of physiology, and convinces me that the practitioner who values himself on his imagined anatomical superiority "will form only an ill-educated, half-taught, incompetent, and therefore a dangerous, physician."

In support of the pretensions of surgeons and apothecaries to be admitted as licentiates of our College, Mr. D. mentions that, "if you refuse the solicitation of a candidate to substantiate his claim to the honour he seeks by a demonstration of his fitness, let the measure be adopted when or where it may, the motive which directs the refusal is founded on sinister views, inseparable from the selfishness and illiberality which you so distinctly disclaim." "Perhaps the soundness of policy to which you look up, may have a character distinct from the general good; and the regulations you are so determined to maintain, may be unworthy to exist."

Mr. D. writes on a subject which he does not seem to understand. Does he know why the College of Physicians was incorporated; and is he aware of the duties which its members have to perform? In the preamble to our charter it is stated, "that King Charles the Second, duly considering the daily abuses of the most laudable and necessary art of physic in the kingdom of Ireland, by the practice of mountebanks, empirics, and other ignorant and illiterate persons, to the impairing

and hazard of the lives of his good subjects, did, for the remedy of these and other mischiefs, and for the encouragement of the learned and experienced practitioners in physic, and for the benefit and safety of his good subjects, by letters patent, found and establish a college or corporation of physicians in the city of Dublin:" that, "because their power and jurisdiction did not extend further than seven miles from Dublin, all the rest of the kingdom was exposed to the same inconveniency it was liable unto before the said grant; whereby the number of unskilful and illiterate practisers of physic hath much increased, and the frauds and deceits of empirics, apothecaries, and druggists, doth abound, to the dishonour of our government, and to the great prejudice and destruction of our good subjects. For the remedy, therefore, of those and the like evils, and for the prevention of the like mischiefs for the time to come, and to supply the defects of the former charter," their present charter was granted to the College by William and Mary.

Does he know why the College of Surgeons in Ireland was founded; and why the Company of Apothecaries, also, was established? Is it not evident that the legislature appointed each corporation, to prevent, as far as possible, abuses in their own department? thus recognizing the propriety of rendering each department distinct; and, of course, imposing on the College of Physicians, from its more extensive powers, the duty of carrying its intention into effect, and of preserving them separate. Is Mr. D. aware that in Ireland the law requires an apothecary to be apprenticed for seven years, before he can be examined as a master; that, in the College of Surgeons, an apprenticeship of five years is necessary; and that, in our University, the degree of M. B. is not conferred until seven years after the period of entrance? I cannot imagine that he is ignorant of these facts; nor can I persuade myself that he considers such precautions for the public good as "inseparable from selfishness and illiberality."

Mr. Dickinson evidently is not a close reasoner. His arguments in general either prove nothing, or, what is worse, they prove too much for his purposes. I shall endeavour to reduce his reasoning to a syllogistic form, and ascertain to what sort of conclusion it will lead. He says,

"If you refuse the solicitation of a candidate to substantiate his claim by a demonstration of his fitness, the motive which directs the refusal is inseparable from selfishness and illiberality.

"But, a candidate may be fit who has not been previously apprenticed, or regularly educated.

"Therefore, to refuse the solicitation of a candidate who has

not been apprenticed or regularly educated, is inseparable from selfishness and illiberality."

If such be really the opinion of Mr. D., he indeed belongs to that class of persons who, to use the words of Mr. Abernethy, "would subvert the institutions of society." According to his view, every one, "by a demonstration of his fitness," would have a right to obtrude himself where it would be a violation of propriety to admit him, and an insult to common sense to suppose that he ought to be admitted. A tailor or a shoemaker might be a more pious character, a better biblical scholar, and more deeply read in works on divinity, than the Archbishop of Canterbury or of Dublin; and he might therefore wish, by "demonstration of his fitness," to substantiate his claim to promotion in the church: yet I can well conceive that the motive which would refuse to allow him to do so, is neither "selfish nor illiberal;" and this, plainly, because his previous habits and ordinary avocations are altogether incompatible with the functions of the high office to which he would aspire. Mr. D. surely must know that an attorney is not permitted to practise as a lawyer; and yet I have never heard it urged, as a reproach to the bar, that such a regulation was selfish or illiberal. The College of Surgeons in Dublin refuse to acknowledge, and strike from their list, any of their members who practise as apothecaries: they refuse to consult with such surgeons as are not members of their own body; and would object to recognize either Mr. Abernethy or Mr. Astley Cooper, or even Mr. Dickinson himself, were he to settle in Dublin. They opposed, and successfully resisted, a Bill which was lately attempted to be passed, for the purpose of rendering members of the London College capable of holding situations in Ireland: and is Mr. D. therefore prepared to charge his own profession with "selfishness and illiberality?" Does he mean, because they are so tenacious of their own privileges, to represent their policy as having "a character distinct from the general good?"

In London, the College of Physicians require that a candidate for their license, who has been formerly either a surgeon or apothecary, shall, before his admission to an examination, procure himself to be disfranchised by the body to which he previously belonged. Does Mr. Dickinson mean to charge the London College of Physicians with illiberality? or, if the usage of the London College be correct, why should the Dublin College be less careful of their reputation, or less attentive to the public interests; and why should that policy which is proper in London be disapproved of in Dublin, and the regulations necessary to uphold it designated as "unworthy to exist?"

Mr. D. does not always clearly state his own arguments, and therefore I am not surprised that he should either misconceive

my sentiments or express them obscurely. To a reader of his letter who had not previously perused my "Remarks," it would appear that I resisted the admission, on any terms, of surgeons or apothecaries into the College of Physicians;—that, because an individual happened to have been at any former period a surgeon or apothecary, I wished to confine him for life to his original "cast," and for ever deprive him of the power of changing it for the profession of the physician. On the contrary, I distinctly recommended that the example of the London College should be followed, which, "while it violates no principle, and exacts nothing harsh or oppressive, is found sufficient to answer the proposed end. It does not go so far as to confine such persons for ever to the business which they first professed, or tend to render them incapable of participating in the honours and advantages which the physician enjoys. A resolution of this kind would be illiberal and unjust: but the practice of the London College is not liable to this charge; it imposes no unfair restraint; it only places them on a level with other physicians, by obliging them to renounce their former profession. In this there evidently is no hardship. If the art of the apothecary be less profitable or less respectable than the profession of medicine, there can be no hardship in resigning it for one more profitable and more respectable. If, on the contrary, his business possess superior advantages, and if the apothecary do not wish to give it up in order to become a physician, his continuing to practise pharmacy is altogether a matter of his own choice."*

But, Mr. Dickinson may reply, the London College have two classes of licentiates,—those who practise in London, and those who reside in the country parts of England. Admitting that medicine and surgery ought to be separated in "cities," why refuse to license as physicians those surgeons and apothecaries who practise in the country? You yourself, in the second part of your "Remarks," allow that, under certain circumstances, it may be necessary for the same individual to combine in his own person the different branches of the profession. To this I answer in the words of the "Address" from the Licentiates to the College of Physicians, quoted in my "Remarks." "They [the licentiates] would further suggest, that, although in the country parts of this kingdom it may be necessary to combine the different branches of medicine, yet this, being essentially a defect, ought not, on any account, to receive the sanction of the College, as such sanction would be in effect a sacrifice of principle, and an avowal that the propriety of distinguishing the professions was at least questionable."

* *Remarks, &c.* p. 60.

Besides, as there is no law to prevent an apothecary or surgeon from practising medicine, a license from the College of Physicians can be only useful by enabling the public to distinguish between the comparative pretensions of different individuals, so as to dispose them to give a preference to the regularly-educated practitioner. But in country districts, which only support a single practitioner, such competition cannot exist. The practitioner, no matter what his medical qualifications may be, will practise medicine, whether licensed or not; his own necessities, and the exigencies of the country in which he resides, requiring him to do so. For what purpose, then, should he be licensed by the College? or how would the community be benefited by such a measure? If, however, a regular practitioner were to reside in the same neighbourhood, why should the surgeon and apothecary, each of whom possesses privileges peculiar to himself, be placed on a level with the physician, by an act of his own College;—a college “established for the management of the learned and experienced practitioners in physic?” Mr. D. says, “None, I think, can refuse their general assent to the physician’s skill;” but, if the College of Physicians are further persuaded that physicians are more competent to treat diseases than surgeons or apothecaries, how could they, without a breach of their duty to the public, license the latter, perhaps to the total exclusion of the physician?

I now pass to the subject of confining apothecaries to their shops, and restricting them entirely to the preparation and composition of medicines. Mr. Dickinson remarks, “If the Irish apothecaries and others, whom you specify to be thus ignorant and incapable, deserve to rank with their fraternity in England in their elementary education and professional attainments, which I should take to be the case, from the statement you make of their growing influence with the public, your invectives against them are surely illiberal, and the ground of your aspersions wholly untenable.”

Whatever difference of opinion may exist,—however sceptical some may be as to the utility of rendering medicine distinct from surgery,—I cannot conceive how it is possible for any one to contend that an apothecary should be permitted, much less sanctioned and encouraged, to neglect his shop, and to leave the preparation of his medicines and the compounding of the recipes of physicians and surgeons, to illiterate shopmen or careless and ignorant apprentices. In my “Remarks,” I stated at length my reasons for resisting, “in every possible way,” the admission of practising apothecaries as licentiates of the College. I contend that, were the College to give their license to a practising apothecary, however extensive his in-

formation or superior his attainments in medicine, they would deserve to forfeit their charter: and I avow that I should myself be one of the first to apply for such disfranchisement, on the ground of their having betrayed the interests of the public and of the profession.

On the "elementary education and professional attainments" of the "fraternity of England," I shall not offer an opinion. In my publication I described matters as they exist in Ireland; and I stated that, "according to the present system, the apothecary, by whom the medicines prescribed by the physician are presumed to be prepared, is scarcely ever in his shop. The moment he becomes a master, he in fact ceases to be an apothecary. From that moment he considers himself a medical practitioner, and regards the business of the apothecary as quite a secondary pursuit. He procures a mere school-boy as an apprentice, and to him is entrusted the serious, the important, office of compounding medicines. The most active poisons are placed within his reach,—the tinctures of opium and digitalis, extracts of hemlock and henbane, the arsenical solution, and the caustic alkalies, &c. are all at his disposal.

"These medicines may, and have often been confounded with others of much less activity, and have thus been administered in doses sufficient to destroy life. We know that such accidents do occur,—the daily prints constantly inform us of them; and yet, so powerful is the influence, they scarcely produce any effect on us. Accustomed to hear of such occurrences, we consider them as mere matters of course; and, finding that we have not ourselves suffered, we never anticipate the possibility of danger. Confidence does not impart security, though habit may totally suppress every feeling of apprehension. On the side of Vesuvius, over which torrents of lava have perhaps a hundred times flowed, the proprietor cultivates his vineyard, undisturbed by the vestiges of former ruin and devastation which every-where surround him: but is he on that account the more secure? Hazardous as his situation may be, it is not more so than that of the patient whose medicines are prepared by an ignorant or giddy apprentice, entrusted with the possession of active remedies. The public are not aware of the danger to which they are exposed from this cause. Physicians write their recipes in a dead language; in the hurry of prescribing they frequently use signs and abbreviations, and do not at all times write even these distinctly. The difference between the mark for a drachm and an ounce is trifling, and requires a practised eye to distinguish it at once. Sometimes in the names of very different medicines only a trifling difference exists.

"The legislature requires that an apprenticeship of seven

years shall be devoted to the craft and mystery of an apothecary; but now a raw school-boy, in his master's absence, mixes and compounds at his discretion. A recipe is handed to him, which he is probably incapable of reading, and which he is told must be prepared with the greatest expedition. Is it to be supposed that the apprentice will hesitate, confess his ignorance, and wait until his master's return? No such avowal, I will venture to say, is ever made. The medicine must be prepared at all hazards; and accordingly the ingredients in a remedy, on the efficacy of which a patient's life may depend, become a matter of conjecture, instead of being accurately and faithfully compounded.

"When medicines are substituted for those which the physician intended, and effects different from what he had anticipated are produced, his situation is rendered most arduous. He is completely misled: he naturally supposes that he has taken a wrong view of the disease; he is deterred from persevering in the remedies which he before considered useful, and he, perhaps, has recourse to others not at all suited to the real nature of the complaint. These are points which deserve the most serious consideration. These facts alone are sufficient to convince every reflecting mind, that the master apothecary ought on no account to neglect his shop, and confide it to the care of his apprentice."*

These also are reasons sufficient to cause us indignantly to spurn the imputation of being actuated by "other motives than the justice of the cause;" and they are reasons which, I think, should have induced a liberal and candid writer to pause before he advanced such an insinuation. Does Mr. D. really believe that my statements, or my "aspersions," as he terms them, are the result merely of "peevish discontent," and that they have no other foundation than the disordered fancy of a prejudiced and dissatisfied mind? Or does he think that I am singular in my opinions; and is he sincere in doubting that such abuses can exist in the department of pharmacy? If he is still incredulous, I can adduce an authority in corroboration of my assertions, which may perhaps have greater weight with him. The celebrated Dr. Lucas, who formerly represented the city of Dublin in Parliament, was originally an apothecary. While he practised as such, he published his "*Pharmacomastix, or the Office, Use, and Abuse, of Apothecaries explained.*" In this work, which was printed in the year 1741, and addressed to a member of Parliament, with a view to obtain an Act for the better regulation of the profession of the apothecary, he says, "It is not to be reasonably imagined that I should know-

* *Remarks*, pages 50, 52.

ingly do any thing to the prejudice of a profession, which I have made my choice, by which I have hitherto lived, and still propose to follow." "I will, however, confess that I have some *selfish private* (though not altogether mercenary) *views*, in soliciting a reformation of pharmacy, and a strict examination of apothecaries and their shops; because I would from myself, as well as others, remove all temptations to the abuse or corruption of my profession."

"Nothing assuredly can contribute more to the improvement and benefit of the healing art, than the professors of the different branches thereof applying themselves entirely to the study and practice of their respective callings. And this was certainly the wise design of our predecessors, in dividing it into those three several parts; and, were it yet further subdivided, it is probable it might be brought to greater certainty, and rendered more generally beneficial to mankind, than it can be in an aggregate state; since the meanest branch can sufficiently employ the whole attention and understanding of a man. We find that many of the ancients were of this opinion, and that it was approved of by the late celebrated Dr. Harris, of London. What considerable discoveries and improvements have hitherto been made in physic in general by these divisions, are very obvious to all that are conversant with the history of medicine. Physicians, by appointing proper agents for the more operose and mechanic part of their profession, threw off all incumbrance, and obtained more time and leisure for study. Chirurgery was cultivated under the same management; and both have arrived at the extraordinary pitch of eminence we now see them in, under these wise regulations. Pharmacy, too, while it kept within the proper bounds, shone and flourished; for it is observable that no set of men made a better figure in their way, than the apothecaries that retained their integrity and kept within the just limits of their occupation; which the works of many of them amply testify."

Having enumerated the names and works of several writers on pharmaceutical subjects, he observes, "While pharmacy was practised by such able hands as those, physic, which has so great a dependence upon it, must have flourished. But such became the insatiable avarice of most of the apothecaries, that they could not long content themselves with the ample profit that arose upon the sale of their proper commodities and manufactures; but, envious of their elder brethren of the faculty of physic, they now endeavoured to imitate them, and sought after new methods of increasing their sordid gains. This they effected by treacherously and surreptitiously invading the provinces of physicians and chirurgeons, and alluring the populace under the specious pretence of giving advice in physic

and chirurgery *gratis*; which fatal delusion readily insnared the ignorant vulgar, who could not be sensible of their gross ignorance and manifest incapacity for such an undertaking, nor apprehend that these very bountiful gentry took care to tax their medicines with their invaluable advice; so that their prescriptions (of which, it must be confessed, they are always most liberal,) are now charged infinitely more than their real value, or what those of physicians and chirurgeons (though of more intrinsic worth, being certainly better adapted to particular exigencies, to the constitution, and the indications of cure,) might be made up and sold for. And thus, by iniquitously enhancing the prices of remedies, and giving them in unnecessary abundance, they clandestinely acquired unmerited fees, equal to those justly due to the most regularly educated and most experienced physicians or chirurgeons.

“That this is the present case, every intelligent, candid apothecary must confess. And these fallacies might be further evinced beyond dispute, were the generality to be made sensible of the just rates of medicines, and the most monstrous, extravagant bills they are frequently charged. To see two drachms of sal prunel, not worth a penny, disguised with some insignificant colour and an unintelligible pompous title, and sold for sixteen pence, must surely move your contempt and indignation!

“I have known a gentleman’s bill, who sickened on Monday and died the Wednesday following, amount to above five pounds, though his careful apothecary had but a street’s breadth to cross between his shop and the patient’s lodgings. It is more than probable that, if the unhappy sufferer took to the amount of his bill in medicines, it was the cause of his death, and would, though he had the most athletic constitution; but, to acquit his apothecary of this *murder*, every one of the profession must confess that no man could well require or consume medicines to that value in the time. Then, what physician could expect so much for his attendance in the ordinary manner for so long?

“Sure there is nothing more inconsistent with common reason, than the taking of apothecaries from their proper office and station! I never saw a patient that was not desirous the apothecary should himself prepare whatsoever medicines should be prescribed for him; and if he is employed in *quacking*, and his time devoted to that alone, how is it possible he can see his customers justly served?

“It is commonly said, ‘that physicians and apothecaries kill more than they cure.’ However ludicrous or satirical this phrase may sound, it is a melancholy consideration that the ill-natured sarcasm often proves just, and is likely to continue so,

in fifty patients exposed to the infection, and seem (speaking from experience) often induced by a preposterous mode of treatment.

The impression so generally fixed on the mind of young practitioners, and many venereal patients, that mercury alone is the grand specific, remains to be discussed.

To form a table of the calamities which the abuse of mercury in the venereal disease has occasioned since the empirical introduction of it into practice, would require a large volume. To those who have not sufficient evidence of this abuse, an examination of the collection of authors, published by Boerhaave, on *Lues Venerea*, termed *Aphrosodiacus*, will, it is presumed, remove all doubts on that head. Yet it may be alleged, that the abuse of mercury, or any other remedy, should not militate against the appropriate administration of it; which axiom cannot be disputed. But, when we behold the debilitated victims to syphilis, or, more properly, to the abuse of the supposed specific, with which our hospitals and our streets abound, and compare their sufferings with the easy and safe cure for the same disease which the American and African natives find in the efforts of nature to relieve herself, in regimen, and in their own vernacular remedies, shall we not be inclined to judge that, in this disease, medicine, as practised according to the rules of art, does more harm than good, and that the remedy is often worse than the disease?

This digression may perhaps be deemed misplaced, and the subject of it would more properly demand a more copious investigation: it is here brought forward principally in behalf of young naval and military surgeons, who are earnestly recommended to be cautious in the administration of mercurials to seamen and soldiers in venereal complaints, and to reflect,

First, That the best authors allow, and daily experience proves, that the simple topical affections of gonorrhoea, inflammation and ulceration of the penis, scrotum, and inguinal glands, are curable without any administration of mercury; which often aggravates both the inflammatory and the ulcerative stages of these complaints, though sometimes useful in the latter as an escharotic and detergent, in the form of corrosive sublimate, &c. externally.

Secondly, that in scorbutic seamen and soldiers, so much exposed, from regimen and manner of life, to scurvy, itch, herpes, ulcers, and arthritic affections, these complaints, whether simple or variously complicated, are often mistaken, both by the patient and surgeon, for syphilis, and maltreated, in consequence, by long and debilitating courses of mercurials; mankind being unfortunately too prone to attribute to occult

causes maladies arising from errors in regimen. In one of the cases of suppressed lues which occurred this year, the patient, during a long passage, had been put on a course of mercurials on account of a chancre, which this treatment had aggravated into a gangrenous, sloughy sore, destroying a considerable part of the glans penis. The patient was extremely irritable, restless, and emaciated. A change of diet; the free use of bark, and sarsaparilla in powder and decoction; the occasional administration of opiates internally; whilst the ulcer was deterged with the expressed juice of worm-grass, or *oleonopodium anthelminticum*, and dressed with a pledget of mel *Aegyptiacum* and ointment of storax; brought the sore to the state of a clean ulcer, tending to cicatrization. In another more acute case of gangrene of the penis, the best effects were experienced from a poultice of the pulp of roasted limes, applied over a pledget of ointment of storax, &c.

Paris; Rue Neuve Saint-Augustin, No. 41.

Jan. 15, 1821.

History of a Case of Hernia Cerebri, consequent on Fracture of the Skull. By C. TURNER THACKRAH, Member of the Royal College of Surgeons in London; Licentiate of the Society of Apothecaries, &c.

IN the evening of the 24th of February, 1818, George Jackson, a healthy lad of seven years, had his skull fractured by a blow from a horse's heel.

A portion of cranium, about the size of an egg, situated near the junction of the frontal and temporal bones on the left side, was found to be deeply depressed, and part of it driven edgewise into the cerebrum. The brain was consequently much lacerated, and detached masses of it were scattered on the hair. No considerable hemorrhage had ensued. His pulse was 58, and free from intermission. No vomiting had occurred; the breathing was not stertorous; nor were the pupils wholly devoid of motion. The lad was disposed to continual dosing; but, when roused, his answers were coherent, and his mind apparently rational. He complained, however, of pain in the head.

The fractured portions of bone were successively raised with the forceps and elevator, and, being detached from the dura mater with a scalpel, were readily extracted. Considerable bleeding took place during the operation, but soon subsided. Pulse 74; pain in the head much relieved; the pupils contracted naturally.

The next morning, after passing on the whole a favourable night, he was composed and easy. Pulse 140. I found the wound filled with a mass, apparently composed of brain, mem-

brane, and blood. This, as well as the surrounding integuments; was tender on pressure. The exposed part pulsated strongly. No vomiting had occurred; no intolerance of light; little thirst; and no disposition to delirium. He was bled, and freely purged.

Vesp.—The appearance of the wound was not altered, and his general state the same. He had occasionally pain in the head; which, however, he found relieved by bleeding. He vomited immediately after the venesection, and rejected also some doses of his medicine.

Feb. 26.—No change in the general symptoms, nor urgency in any. As, however, there was some pain in the head, the application of leeches was ordered. The lad was generally disposed to dose. As the exposed cerebrum had assumed a fungous appearance, lint and simple dressings were applied.

27.—In much the same state as yesterday. Some compression was made on the protuberant brain, by lint and strips of plaster.

March 1.—The pressure had not been particularly painful. On the removal of the dressings, the lint usually tore away a portion of the fungoid mass. This occasioned no severe pain; nor did the tumor ever manifest very acute sensibility. It had not increased in bulk. The constitution, however, began to suffer some perturbation. The eyes, of late, had become more susceptible of stimulus; the tongue was white; and the face frequently flushed. Pulse 110—125. He took an occasional purgative.

5.—For the last few days he remained in much the same state; but, indisposition having prevented my seeing him, I found, to my surprise, that the cerebral substance was considerably higher.

7.—The fungus was still on the increase; had attained the size of a large orange; and on its surface were many fissures, from which fetid matter was freely effused. The scalp surrounding the tumor always evinced fully as much tenderness as the substance itself. The lad now complained more of head-ach, and was more restless. His eyes were generally humid, and slightly inflamed, but the pupil had a healthy contraction. Pulse 130. He had vomited once or twice, but this was traced to error in diet. He felt considerable relief from a refrigerant lotion, with which the head was constantly moistened.

10.—From the fissures and the fetid discharge, I had hoped that a natural separation might have taken place; but, no such change occurring, and the tumor rather increasing in size, I was desirous of removing it with the scalpel. This, however, was disapproved of by my friend Mr. Battye, (who had the kindness to see the patient along with me,) under the appre-

hension that a profuse and fatal hemorrhage might ensue. He recommended a trial of the sulphate of zinc as an escharotic. Cloths moistened with a solution of this salt were, therefore, freely applied.

11.—The boy remained in the same state; nor had he suffered much pain from the lotion. A portion of the fungus being torn off by the dressings, the tumor manifested a disposition to hemorrhage.

13.—No alteration in the appearance of the fungus; but the soreness and inflammation of the surrounding integuments had extended over the left cheek.

16.—As the escharotic seemed to produce no diminution of the protuberance, mild dressings and a saturnine lotion were substituted. The discharge was copious and fetid. The lad was now allowed to sit up occasionally. He was generally lively, ate moderately, and had no great degree of feverishness.

18.—In the morning I was hastily informed that the boy was in a fit. On my visiting him, however, it appeared that the attack had been one of delirium, on his rising in bed to take his breakfast.

20.—The fungus was still in some degree on the increase, but became proportionally narrower at its base.

22.—As it had assumed a form sufficiently pendulous to admit a ligature, this mean was adopted. A narrow piece of waxed tape was insinuated round its root, and a moderate compression made. The boy once or twice cried out from violent abdominal pain. In a short time, however, he felt no particular uneasiness either in his head or belly. The pulse for a while was somewhat quickened. During the afternoon he suffered some attacks of head-ach, but perfectly retained his senses, and even slept well at night.

25.—As no marked change had taken place either in the constitutional or local symptoms, a second ligature was applied tighter than the first. No particular pain or uneasiness ensued.

28.—The ligature was drawn closer. An abscess was found on the outer edge of the fungus, which discharged about 3ss. of laudable pus.

April 1.—The ligature was again tightened. Severe, though temporary, pain in the head, was the immediate effect. The lad had not borne any but a recumbent posture for the last ten days.

3.—The constriction was again increased. The fungoid mass was now evidently shrinking, laid pendulous on the eye, and was partially covered with a dark crust.

4.—The ligature was again tightened. A very slight hemorrhage succeeded this, as well as the former constrictions. An

appearance of cerebral fungus presented itself beneath the ligature. The patient's general health was good.

6.—The neck of the tumor had become narrow. The ligature was drawn closer.

8.—The decayed and shrivelled fungus* was wholly removed; but a fresh one appeared to be rising at its base. To this I applied several compresses of lint, bound down with adhesive strips. The purulent discharge had greatly diminished; nor was there any appearance of abscess.

9.—No considerable pain produced by the compression. The pulsation of the exposed cerebrum was strong.

11.—The fungus began to decrease.

18.—It was considerably lessened, and the granulations on the integuments fast approximated. From this period, the lad was dressed every second or third day, the compression being gradually diminished. The wound at first closed rapidly, but a small ulcer, about the size of a pea, in the middle, was tedious in healing. The boy was quite healthy, and wholly free from pain.

July 3.—After passing a very restless night, he had a convulsive attack, which continued for some minutes. On my seeing him in the forenoon, I found him seriously ill, though complaining of little pain. The pulse was 100, intermitting, and irregular. The pupils contracted, not naturally, on stimulus; and the mind was disposed to delirium. Leeches, blisters, and free purgation, were soon, however, productive of decided relief; and in two days every trace of disease was removed.

Since this period the boy has remained in perfect health.

Of this disease, few cases of a favourable result have been recorded; nor has the nature of the tumor, and the means best calculated for its removal, been hitherto ascertained.

In the preceding details, some particulars seem worthy of regard.

1. The fungus was highly vascular, and appeared completely organized.

2. It was connected with abscesses, probably situated near its base.

3. Compression at first seemed to repress its growth.

4. Nature was not able to cast off the full-formed tumor.

5. The application of sulphat of zinc also proved inefficient.

6. Strangulation by a ligature, *gradually tightened*, finally removed the hernia, without inducing any alarming constitutional irritation.

* This substance appeared to consist of cerebrum preternaturally vascular. Except in its dark grey hue, it resembled the figure which accompanied Mr. Stanley's paper in the eighth volume of the *Medico-Chir. Trans.*

7. Pressure prevented the growth of the incipient tumor which arose on the destruction of the former.

Hernia cerebri generally induces a fatal event too rapidly to allow the means used in the preceding case. When the constitution becomes seriously affected, excision affords the only prospect of cure. But, should no alarming symptom occur, the surgeon, I conceive, will not err by waiting till the tumor assumes a form suitable for the application of a ligature. The mode in which this remedy should be used, I conclude, from the case related, to be that of gradually increased compression. Had the constriction been severe at first, dangerous affection of the brain would probably have ensued: and I have been recently informed that, in a case occurring in the neighbourhood, such symptoms succeeded on the application of a firm ligature as indicated the propriety of its speedy removal.

Leeds; December 1820.

A Descriptive Account of a rare Case of Mal-formation in a Child.

By JOSEPH HOWELL, Member of the Royal College of Surgeons in London.

AT the termination of the eighth month of her pregnancy, a young woman was delivered of her first child, the subject of the following singular case of *lusus nature*.

Through an opening, formed by a deficiency of the upper part of the parietes of the abdomen, six inches in circumference, protruded the liver, gall-bladder, spleen, pancreas, the stomach, and the whole of the small intestines. The edges of the opening through which these viscera protruded, was smooth and well-formed, being very similar in appearance to the prepuce embracing the glans penis. The upper and greater part of this mass of viscera was surrounded by a muscular membrane, separable into three distinct thin layers, but destitute of common integuments, coming from the edge of the opening at its upper part. This covering hung loose, not adhering to the parts over which it was reflected, and did not extend low enough to cover the protruded intestines, the coats of which were protected by little detached slips of muscle, covering those convolutions only which were external. The two umbilical arteries arose from the left common iliac, and, emerging from the cavity of the abdomen, entered the membrane hanging over the viscera at its left edge; proceeding between the layers of muscle, a little upwards, they came in contact with the umbilical navel which descended from the anterior part of the broad ligament of the liver; passing out from the said covering, the vessels formed the chord of the usual appearance and length. The thorax and head were examined, and found perfect; and

the child, which was a male, was well-proportioned, and of the common size. It lived twenty-four hours, and was fed once; but passed nothing from its bowels. The handling of the protruded viscera did not seem to give pain, yet at other times the child cried lustily. It died quietly, without seeming to experience any suffering.

Wimbleton; Jan. 19, 1821.

A Memoir on a new Process for the Discovery of the greater Part of Mineral Poisons, when mingled with coloured Liquids. By J. B. ORFILA, M.D. &c.

I HAVE particularly insisted, in my work on Toxicology, on the difficulty often experienced in the discovery of poisonous substances of the mineral kind which have been mingled with coloured liquids, such as red wine, decoction of coffee, &c. Indeed, almost all the re-agents proper to render manifest a mineral poison dissolved in water, act in such a manner that we cannot recognize it when it is united with deep-coloured liquids. I may add, indeed, that, under certain circumstances, the precipitates produced by those tests are of such a colour, that, far from furnishing any sure indications respecting the nature of the poisonous substance, they lead us to believe that it does not exist in the deleterious mixture we examine. This assertion has been so well placed beyond doubt, by the numerous experiments detailed in my Treatise on Poisons, that it would be superfluous to adduce here any further examples for its support. Any one will be easily convinced, on reading what I have written on this subject: 1°, that a physician charged to make a report on a case of poisoning, would be blameable if he did not know the means for discovering poisonous substances mingled with coloured liquids; since, in the greater number of cases, those substances have been, either designedly or accidentally, mixed with the coloured drinks ordinarily used in domestic economy; 2°, that the processes hitherto known to determine beyond doubt the existence of mineral poisons in such mixtures, differ considerably for each of those poisons; 3°, that the chemical operations by which they are constituted, are almost always very complicated, and are often but very ill adapted for the fulfilment of the proposed object. Indeed, is it easy to demonstrate the presence of metallic arsenic, on calcining with potash and charcoal the dry product of the evaporation of a mixture of a grain of white oxide of arsenic and one or two pints of wine or decoction of coffee? Would similar measures furnish more readily the proof of the existence of a small quantity of corrosive sublimate, of verdigris, of muriate of tin, &c.? I believe not; and

this object appears to me to be of so much importance, that I am induced to publish the results of a great number of experiments which I think calculated to simplify the solution of the problem in question. The following are the reasonings by which I was led to the discovery of the process which I am about to describe.

Mineral poisons mingled with deep-coloured liquids, act with the tests proper for their discovery in a different way from that in which they would act if they were dissolved in water: this circumstance depends entirely on the presence of the matter which colours the liquid mixed with the poison; on destroying, then, this colouring matter, the poison should act with the tests as if that matter were not present, provided that the agent employed to deprive the mixture of colour does not decompose the poisonous substance. Now, *chlorine dissolved in water* (oxymuriatic acid) possesses the power of depriving of their colour red wine, decoction of coffee and of tobacco, &c.* and it decomposes but a small number of the poisons of the mineral kind; and, consequently, might be advantageously employed. Experience has not failed to convince me that such was the case, and that the greater part of mineral poisons might be detected by those means.

White oxide of arsenic.—A mixture was made of a solution of white oxide of arsenic in water and red wine; a sufficient quantity of liquid and concentrated chlorine (strong oxymuriatic acid) was poured into it to cause it to assume a yellow colour; a reddish-yellow precipitate was formed, composed of chlorine and the glutinous matter contained in the wine: this precipitate was suffered to settle, and the liquor was filtered. A *white* precipitate was produced in the filtered liquor by lime-water, a *green* by the sulphate of ammoniated copper, and a *yellow* by hydro-sulphuric acid (sulphurated hydrogen gas dissolved in water): the presence of white oxide of arsenic was then demonstrated. It has sometimes happened that no precipitate has been obtained on mingling the tests just mentioned with the filtered liquor, because the quantity of oxide of arsenic contained in the mixture was too small, or because the chlorine, instead of being concentrated, was diffused in a large proportion of water. From these two circumstances it happened that the solution of oxide of arsenic was too weak to act with the tests in a sensible manner: in this case, it became

* In stating that chlorine deprives red wine, coffee, &c. of their colour, it is not meant that they render those liquids colourless, but only that it destroys their red or brown colour: indeed, the mixture obtained is of a yellow colour. This is of but little importance; for this shade of colour does not prevent the principal tests acting on mineral poisons as they would do if those poisons were merely dissolved in water.

necessary to evaporate the liquid, to concentrate it, and, when it has been reduced to a third of its original volume, it has constantly furnished the white, green, and yellow precipitates above mentioned. These considerations show that it is important that *concentrated and recently-prepared chlorine* should be employed in inquiries of this kind. Analogous experiments were made with white oxide of arsenic dissolved in water, and mixed with various quantities of decoction of coffee: the action of the chlorine and the tests was the same.

Arsenic acid.—When a mixture of red wine and arsenic acid is made, and it is discoloured by concentrated chlorine, a yellow precipitate is formed: if the liquor be filtered, it is found to redden the tincture of turnsole, and to give rise to a *white* precipitate with lime-water; a *light-blue* precipitate with the acetate of copper, especially if a few drops of chlorine are poured into the liquor; and a *rose-coloured* precipitate with the hydro-chlorate, but slightly acid, of cobalt: which prove that arsenic acid may be discovered by those tests as if it were merely dissolved in water. The same circumstances were observed when coffee was substituted for wine.

Acid arseniate of potass.—The mixture composed of this substance and red wine or decoction of coffee, may be readily discoloured by chlorine. The liquor when filtered, after the deposition of the precipitate, give rise to a *white* precipitate with lime-water, a *light-blue* with acetate of copper,* and a *rose-colour* with the hydro-chlorate, but slightly acid, of cobalt. If it should happen, by chance, that the last test does not produce any precipitate, the excess of its acid should be saturated with ammonia, (*liquor ammoniæ*, Pharm. Lond.) and the liquor will then soon be found to become opaque. It follows from these experiments, that chlorine will serve for the detection of the existence of arsenic acid and the soluble arseniates, when mingled with red wine, coffee, &c.

Corrosive sublimate.—A mixture of corrosive sublimate and wine was discoloured by *concentrated and recently prepared chlorine*; it was filtered after the precipitate of the chlorine and the vegeto-animal matter had settled; the filtered liquor

* Sometimes the precipitate, instead of being of a *light-blue* colour, is dark-blue, or a blue verging towards green: this happens when the quantity of chlorine employed for the discolouration of the wine or the coffee has not been sufficient. In this case, the colour of the precipitate may be changed to a light-blue by the addition of a few drops of chlorine. This circumstance has been observed in the greater part of the mixtures of poisons and coloured liquors which are considered in this memoir; so that I believe I may state, that in general it is sufficient to add a few drops of chlorine to the several precipitates obtained by means of the tests, to communicate to them the shade of colour which is proper to them, and that which they present when they result from the action of the test on the poison merely dissolved in water.

produced a *yellow* precipitate with potash, a *white* with ammonia, and a *black* with the hydro-sulphurets, as if the *sublimates* had been dissolved in water. A mixture was made of decoction of coffee and an aqueous solution of corrosive sublimate; the combination was treated with chlorine and the tests above mentioned, and precipitates of the same colour were obtained. In both cases, the liquor discoloured by chlorine has acted on a plate of copper deprived of its polished surface, as a solution of corrosive sublimate in water would do. It is, then, possible to discover the presence of this substance in wine and coffee, by the means which I have indicated. Nevertheless, I think it is preferable to have recourse to the following process: The mixture of sublimate and wine or coffee is put into a flask; two or three drachms of sulphuric ether are poured on it; the flask is to be corked, and gently shaken for ten or twelve minutes, so, however, that the ether may be several times in contact with every part of the liquor. The ether takes from the wine and the coffee the greater part of the sublimate, and the liquor separates into two *strata* when we cease to shake it: the upper stratum is formed by the ether holding in solution the corrosive sublimate. The whole is poured into a tunnel, the extremity of the pipe of which is closed by the fore-finger. After a few instants, when the separation of the liquor into the two strata above mentioned is seen to be effected in the body of the tunnel, the inferior coloured liquor is suffered to flow out, by partly removing the finger from the end of the pipe; when the lower stratum of the liquor is just flown out, the opening of the pipe of the tunnel is again closed by the finger, to prevent the escape of the ethereal stratum, which is now to be poured into a cup or any other vessel presenting an extensive surface: the ether evaporates, and the sublimate is left in a solid state. This is then dissolved in a small quantity of distilled water, and a concentrated aqueous solution is thus obtained, in which the yellow, white, and black precipitates, which it furnishes with potash, ammonia, and the hydro-sulphurets, are easily recognized. The same process might be employed with success, where the sublimate has been dissolved in alcohol, or so large a quantity of water that it is in such a state impossible to detect its presence by the tests. Indeed, experience has proved to me that we may easily discover it when it exists in the proportion of only one grain to four thousand grains of distilled water. I think it useful to remark, that if the two liquors are shaken too violently, and for a very long time, and if the ether employed is not sufficient in quantity, the experiment will fail: indeed, the ether in this case would be entirely dissolved by the water or by the coloured liquor mixed with the poison, and

we should not obtain the two strata of different specific gravity, on which the success of the whole operation rests.

It was known long since that corrosive sublimate was much more soluble in ether than in alcohol and in water. Mr. Chausier announced, in 1811, from some experiments made by Mr. Henry, that ether took corrosive sublimate from water; but I do not know that any person has yet spoken of the experiments I have just related, relative to the use of ether for the separation of corrosive sublimate from coloured liquors which have not decomposed it.

Acetate and sulphate of copper: artificial verdegria dissolved in water.—Mixtures of red wine and various quantities of the salts just mentioned were made: chlorine dissolved in water and concentrated (strong oxymuriatic acid,) was added to the mixture, until it became of a yellow or greenish-yellow colour. A precipitate formed, composed of chlorine and vegeto-animal matter. The liquor was filtered: a *brown-marron* coloured precipitate was produced by the addition of prussiate of potash (hydrocyanate of potash and iron), a *green* by arsenite of potash, and a *black* by the hydro-sulphurets, or by the hydro-sulphuric acid. Now, these characters appertain to the salts of copper dissolved in water. Ammonia and potash acted on the mixture of wine and sulphate of copper discoloured by chlorine, as they do on the aqueous solution of sulphate of copper, though it was not thus, with a mixture of wine and acetate of copper, discoloured. This is of but very little importance, because the *brown-marron*, *green*, and *black* precipitates, obtained by the prussiate of potash, the arsenite of potash, and the hydro-sulphates, permit us to affirm that the solution contains a salt of copper. Sometimes the mixture of wine and cupreous salt, discoloured by chlorine, contains so small a quantity of the poison, that the tests do not render it manifest: the solution must here be concentrated by evaporation, as I stated when speaking of the oxide of arsenic. Coffee rapidly decomposes the salts of copper, and produces a precipitate so abundant, that it is impossible to suppose the possibility of such a mixture in the liquid state.

Acid tartrate of potash and antimony, (antimonium tartarizatum, *Ph. Lond.*)—Tartar emetic, being decomposed and precipitated by chlorine, should be ranged amongst the small number of poisons which do not remain in solution, when, after having been mingled with wine or coffee, they are treated with chlorine. The process under consideration is not, then, applicable to this particular case.

Compositions of lead.—If it be incontestible that the soluble salts of lead cannot be mingled with red wine or decoction of coffee, because they are decomposed by those liquids, it is

equally true that, under certain circumstances, wine which has stood on litharge, or tobacco which has been packed in leaden boxes, combine with a proportion of lead sufficiently considerable to produce deleterious effects on our system: it is of importance, then, that the means for the detection of the saturnine preparation should be indicated.—*Red wine.* Acid wines which have been for some time in contact with litharge in small particles, may still preserve their red colour; they acquire an astringent and slightly sweet taste. When they are mixed with concentrated chlorine, they become discoloured; and, if they are filtered after the precipitate of chlorine and vegeto-animal matter which forms is suffered to settle, it is found that the liquor, when filtered, of a yellow colour, hardly furnishes, with the proper tests of lead, any sensible precipitate; the want of it depending on the small quantity of salt of lead contained in the wines. But, when the liquor is evaporated until it is reduced to one-third of its original volume, we *constantly* observe that the sulphate of potash gives rise to a *white* precipitate, the hydro-sulphates to a *black*, and chromate of potash a *bright yellow*; from which it follows, that the process under consideration should be employed in preference to any other to demonstrate the presence of lead in wines imbued with litharge. The measure proposed by authors to accomplish this end, and which I had myself indicated, is certainly very conclusive, since it consists in separating the metallic lead by calcination, but it is a much longer and much more difficult process.—*Tobacco.* If tobacco has been packed in leaden boxes, and it contains oxide or subcarbonate of lead, it should be boiled for ten or twelve minutes with a mixture composed of equal parts of distilled water and distilled vinegar, to change the saturnine compound into a soluble acetate; it is to be filtered: the filtered liquor will be of so brown a colour, that it will not be possible to demonstrate the presence of the metal by the aid of the tests above mentioned; but this will be easily effected if the liquor is discoloured by means of chlorine, and treated as I have described when speaking of wines imbued with litharge.

Acid nitrate of bismuth.—The acid nitrate of bismuth may be united with such a quantity of wine, that the mixture may preserve the transparency and red colour of the wine: when a sufficient quantity of concentrated chlorine is poured into it, it becomes discoloured, and at the same time a precipitate of a yellowish-white colour is formed;* a few drops of hydro-chloric acid (muriatic acid) are sufficient to make the precipitate dis-

* Chlorine precipitates the acid nitrate of bismuth in a white powder.

appear, and then the liquor is of a yellow colour. Water and potash produce a *white* precipitate from it, though the precipitate formed by the alkali becomes yellow when it is dry; and the hydro-sulphates produce a *black* precipitate: the solution acts, then, with the tests as if the wine were not present. When mixed with coffee, the acid nitrate of bismuth is affected as it is when united with wine, when chlorine is added: however, the proportion of the coffee to the salt should not be very considerable, for there would then be formed a very abundant grumous precipitate.

Sulphate of zinc.—Red wine was mingled with a solution of sulphate of zinc in pure water; a quantity of concentrated solution of chlorine, sufficient to discolour it, was added: the precipitate of chlorine and vegeto-animal matter which then formed was permitted to settle, and the liquor was filtered: it was of a yellow colour, and furnished a *white* precipitate on the addition of potash, a *white and slightly-yellow* with the hydro-sulphurets. The tests, then, have acted on it as if the sulphate of zinc were dissolved only in water. A mixture was made of a decoction of coffee and a solution of pure sulphate of zinc: it was discoloured by means of chlorine; the liquor was filtered, and furnished a pale *yellowish-white* precipitate with the hydro-sulphurets, a *white* with the hydro-chlorate of barytes (muriate of barytes), and a *brown* with potash: but this last precipitate became *white* when a few drops of solution of chlorine (oxygenated muriatic acid) were poured on it.

Hydro-chlorate of gold, (muriate of gold.)—A mixture of red wine and hydro-chlorate of gold was discoloured by means of chlorine: the filtered liquor furnished a *deep-purple* precipitate with the proto-hydrochlorate of tin, a *deep-yellow* with ammonia, and a *blackish-brown* with the proto-sulphate of iron, (common sulphate of iron, or purified *green vitriol*.) The hydro-chlorate of gold, then, mingled with wine and discoloured by chlorine, acts with the tests as if it were simply dissolved in water. A mixture of coffee and the salt under consideration was discoloured by means of chlorine: the liquor, when filtered, furnished a *blackish-brown* precipitate with the proto-sulphate of iron, a *deep-purple* with the proto-hydro-chlorate of tin, and a *deep-yellow* with ammonia: this last became of a *pale-yellow* when an excess of ammonia and a few drops of chlorine were added.

Nitrate of silver.—The nitrate of silver, being suddenly decomposed by chlorine, and transformed into an insoluble chloride of silver (oxy-muriate of silver), cannot be detected in the liquor when it has been mingled with wine or coffee and the mixture discoloured by means of chlorine.

Deuto-hydrochlorate of tin.—A mixture was made of this salt and red wine, and a quantity of chlorine added sufficient to discolour it: the precipitate formed was permitted to settle; the filtered liquor was yellow, and furnished a *white* precipitate with potash, and a *yellow* with the hydro-sulphurets, as if the deuto-hydrochlorate of tin had been simply dissolved in water. A mixture of this salt and coffee acted in the same way with chlorine and the tests.

Proto-hydrochlorate of tin.—When a mixture is made of proto-hydrochlorate of tin with wine, and concentrated solution of chlorine is added to discolour the liquor, it is found that this discolouration is very difficult to be effected: it does not take place until six times as much chlorine has been employed as is necessary for the destruction of the colour of wine mingled with the other mineral poisons; whence it appears that the proto-hydrochlorate of tin must be in but a small proportion to the liquor, and that the tests proper to detect it do not act on the mixture. But it is quite otherwise when the liquor is reduced to the fifteenth part of its volume by evaporation, and filtered: potash then gives rise to an abundant *white* precipitate, the hydro-sulphurets to a *yellow*, the infusion of cochineal to a *scarlet*, and the hydrochlorate of gold does not render it opaque or troubled. Now, these characters appertain to the proto-hydrochlorate of tin; whence we must conclude, that the chlorine has transformed the proto-hydrochlorate into a deuto-hydrochlorate, as if wine had not entered into the combination. This process, then, will enable the physician to affirm that the liquor contains a salt of tin (which is of the most importance), without permitting him to decide on the nature of the oxide which enters into the composition of this salt.

Hydro-chlorate of barytes, (muriate of barytes.)—When a concentrated solution of chlorine is poured into a mixture of red wine and hydrochlorate of barytes, or of this salt and decoction of coffee, the liquor is discoloured. If it is filtered after the precipitate of chlorine and the vegeto-animal matter has settled, we find that the filtered liquor, having also been heated so as to cause the excess of chlorine to be disengaged, furnishes, with nitrate of silver, a *white curdy* precipitate, composed of chloride of silver, insoluble in water and in nitric acid; and produces, with sulphate of soda, a *white* precipitate, composed of sulphate of barytes, insoluble in water and in nitric acid. The tests, then, act with this mixture as if the hydro-chlorate of barytes were alone present.

Composition blue, (a mixture of concentrated sulphuric acid and indigo.)—It is known that composition blue gives out sulphurous gas, recognizable by its odour, when it is boiled with

metallic mercury; a proof that it contains sulphuric acid: but, if we wish to determine the presence of this acid, either by the tincture of turnsole or by the salts of barytes, we observe phenomena but little qualified to enlighten us in regard to the real nature of the liquor. It is not so when this is mixed with a concentrated solution of chlorine: discolouration of it instantly takes place, provided that a sufficient quantity of chlorine be employed, and the liquor resulting from that combination, of a yellow colour, gives a deep-red colour to the turnsole paper, and furnishes, with nitrate of barytes, a precipitate composed of sulphate of this oxide, which is of a white colour when it is collected into a heap, and which does not dissolve in either water or nitric acid. The tests act, then, as if indigo made no part of the mixture.

Alum mingled with wine.—It is well ascertained that wine-merchants sometimes add alum to red wine, to communicate to it a rough taste and a deeper colour: now, this mixture may produce in the system ill effects of a more or less serious kind. Several means have been proposed for the discovery of the fraud in question, but many of them might be shown to be ineffectual: none appears to me to be proper for the discovery of the presence of the alum but the following: The wine is to be discoloured by means of a concentrated solution of chlorine; the mixture is to be evaporated until it is reduced to nearly the fourth of its original volume; the liquor is to be filtered, and we then find it to possess the following properties when it contains alum:—1, it has a sweetish astringent taste; 2, it furnishes a white precipitate (sulphate of barytes) with nitrate of barytes, insoluble in water and in nitric acid; 3, caustic potash gives rise to a yellowish-white precipitate of alumine, soluble in an excess of potash; 4, the subcarbonate of soda produces a yellowish-white precipitate (subcarbonate of alumine), decomposable by fire into carbonic acid gas, and alumine easily recognizable by its characters.—(*Nouveau Journal de Médecine*, Juillet 1820.)

Some Observations on Injuries of the Brain. By C. C. WALLIS, Member of the Royal College of Surgeons, and Licentiate of the Society of Apothecaries, of London.

FACTS which have a tendency to elucidate pathological obscurities, are at all times worthy of our best considerations; and it is therefore to be regretted that rare occurrences in surgical practice are not more minutely attended to, or, rather, that their knowledge is not more generally diffused. This circumstance may arise from a want of zeal on the part of the

practitioner,—from too fastidious conceptions of his own capacity,—from dread of public criticism,—or from his time being too exclusively required by other more immediate and less avoidable professional duties. Every member of the profession, however, were he actuated by so desirable a spirit, and that spirit not opposed by the intervention of insuperable barriers, would unquestionably have frequent opportunities of contributing largely to the advancement of science; and it would then be imperative on him to give all possible publicity to every extraordinary or uncommon case that might fall under his cognizance.

Accidents and diseases occurring to the brain are not the most unfrequent affections that claim the assistance of surgery, and are at all times subjects demanding the practitioner's mature and deliberate investigation. Much obscurity for a long time prevailed respecting the effects of injuries of this organ; and the uncertainty of the diagnosis of compression, concussion, &c. was a reason for many of the French surgeons (among them Bichât and Dessault) for renouncing the trephine. Definite discriminations and symptoms characteristic of each state have, however, been lately made with much success by our justly-celebrated countryman, Mr. Abernethy.*

Injuries of the head are, generally speaking, dangerous from the undue pressure produced on the brain by depression of bone; and extravasation of blood, or the formation of matter, within the cranium; each of which causes will finally produce the same symptoms,—viz. insensibility, weak pulse, dilated and immovable pupil, stertorous respiration, &c. and can be relieved only by the timely application of the trephine. Before, however, such an application is made, it is of the utmost moment to know which of the several causes alluded to is exerting the baneful influence we are called upon to remove.

Depression of bone, then, is characterized from either of the other two states, by the symptoms supervening immediately after the infliction of the injury; by there having been no interval of sense, nor time sufficient for the completion of supuration.

It will be necessary, also, to distinguish depression of bone from a state of an equally alarming nature—concussion. This kind of injury is denoted by a sudden stunning or insensibility; the patient is cold, the pulse slow and intermitting, and respiration scarcely perceptible. In proportion as the stupefaction goes off, increase of pulse and respiration take place, the pupil is contracted, the patient is sick, and vomits. If roused, he

* Surgical Works, vol. ii.

will answer peevishly, and then fall asleep again. As the disease advances, febrile action is manifested, and phrenitis rapidly ensues.

I have been led to the foregoing remarks by a recent occurrence in this neighbourhood, which will undergo judicial investigation at the ensuing assizes. It was a case wherein the operation of trephining was performed about a fortnight after the receipt of external violence; and it was stated in evidence at the coroner's inquest, that the patient followed his accustomed bodily labour for *five days* after the infliction of the injury; but that, on opening the head after death, the skull was found to be extensively fractured, and that several spiculæ of bone had actually been driven into, and produced disorganization of, the substance of the brain itself, although *fourteen days* had elapsed before the usual symptoms manifested themselves.

These facts present forcible contradictions to the doctrines hitherto promulged on this interesting and important topic; and society will be benefitted, and myself much obliged, to any of your readers who will explain this apparently irreconcilable contrariety.*

Langport; January 8th, 1821.

* We insert this paper because it contains an useful lesson that may be repeated from time to time, without disadvantage, to many practitioners. The records of Surgery, and especially *ex professo* treatises on the subjects of Medical Jurisprudence, abound with cases analogous to that above related by Mr. WALLIS; whose query, we suspect, cannot be replied to in a satisfactory manner. Between the fatal consequences from as slight a blow on the head as that received by the girl whose case is related by Hippocrates, and the retention, for years, of a bullet in the midst of the brain without apparent harm, we commonly find many varieties of results from similar modes and immediate consequences of external violence; but, of the causes of those varieties pathologists are as ignorant as they are of the reason why one man is salivated by a grain of calomel, whilst others will take a drachm of the same mineral without such an effect being produced. Dr. MALE's Elements of Medical Jurisprudence will show the views that are to be taken of such cases as that of Mr. Wallis, in relation to jurisprudence.—
EDIT.

COLLECTANEA MEDICA:

CONSISTING OF

ANECDOTES, FACTS, EXTRACTS, ILLUSTRATIONS, &c.

*Relating to the History or the Art of Medicine, and the
Auxiliary Sciences.*

*Floriferis ut apes in saltibus omnia libant,
Omnia nos itidem depascimur aurea dicta.*

*Extracts of the Report from the Select Committee of the House of
Commons on the Doctrine of the Contagion of the Plague, in 1819.*

Veneris, 19^o die Martij, 1819.

SIR JOHN JACKSON, Baronet, in the Chair.

Dr. AUGUSTUS BOZZI GRANVILLE, called in; and Examined.

ARE you acquainted with the plague, and have you formed any opinion as to its being contagious: favour the Committee with your opinion respecting it?—I have seen the plague, and I have no doubt that it can be conveyed by an individual infected by it, to another in perfect health.

Where have you seen the plague?—In various parts of Turkey, Greece, Asia, Syria, Egypt, &c. and in Constantinople, where I resided two years.

What are the symptoms and characteristics of plague?—The symptoms are permanent; and, during the time the plague raged in Turkey in 1812, they were permanent throughout the country. It is a sudden dizziness, great pain in the head, great prostration of strength, affections of the nervous system particularly; (there are no symptoms of inflammation whatever, not such as attend inflammatory diseases during their first attack;) sickness of the stomach occasionally, and the invariable appearance of glandular swellings, if it goes beyond sixty hours.

Is it not also attended with carbuncles?—With carbuncles and other pestilential eruptions, particularly livid spots on the body, partial mortification of the body.

What do you consider the cause of the plague in Turkey?—It is a question no practical man can answer; it is entirely unveiling the mystery in which all diseases are enveloped: I can only answer that it does exist, and is conveyed in the way I have stated.

To what do you ascribe our not having it in Great Britain?—To the regulations of the quarantine laws. It appears to me, as far as I can judge of the nature of a disease without knowing its origin, that, being endemic at certain parts of the globe, it might explain why it is not peculiar to this country unless imported.

Does it not often appear in places in the Mediterranean, where the quarantine-laws are severely rigid?—Never, except in lazarettos, or where a violation of those laws has taken place.

Then you conceive it must be imported, and that it cannot originate in those places?—It does not originate; certainly not.

What precautions are taken to prevent infection, by the Frank inhabitants of Smyrna and Constantinople, and other places visited by the plague?—If they can afford it, shutting themselves up in the houses before communication with persons infected; if they are obliged to go abroad, as some are, such as physicians who have their livelihood to get, some wear oil-skin dresses, oil-skin gloves, and other medical precautions to prevent breathing the infected air; others anoint themselves with oil, and avoid contact as much as possible, under a strong persuasion that contact produces disease. In Egypt and Syria they shut themselves up as soon as there is a rumour of the plague, and never quit till the dews fall, that is, till St. John's day; then they come out, and proceed to church in order to sing *Te Deum*.

During the prevalence of great disease in any of these towns, did you ever know the plague destructive in the families of the Franks who avoid contact with diseased persons?—No one instance, where contact or some other conveyance, by goods or other articles, could not be ascertained. If the Committee will allow, I will state a circumstance as to the house in which I lived myself. The house was one of the chief houses of the hospadar of Wallachia, Prince Suzzo; that was the house where I lived for eleven months, during which time, at one period, in 1803, the plague prevailed; the precaution of shutting up the family immediately on the appearance of the plague, was never omitted; and no one case was on record in the family, of any plague having occurred within the walls of it.

Could such a circumstance possibly have occurred, provided the plague had been an epidemic disorder: could the shutting-up of one house in an infected city save the family?—Certainly not. But the plague is not epidemic. I can bring cases in support of the assertion with regard to the plague not being an epidemic distemper, and that it does not depend on atmosphere or ventilation.

Do you not conceive that, in the case of contagion, the state of the air may have the strongest effect in stopping or promoting the effects of the disorder, as you have instanced in the case of the procession on St. John's day?—It may render the person exposed to the contact more or less liable to feel its effect, but will not operate in checking the disease.

To what do you attribute the periodical appearance of the plague in the spring and autumn?—Because the seasons have an influence on the character of the disorder, the same as in this country; in winter you are more likely to catch a cold or catarrh.

Can you guess how plague was originally produced?—I should premise by stating that the impossibility of ascertaining the origin of a disease does not do away with its existence. Every medical man has attempted to form an opinion; and, I should state in answer, it is most probably an epidemic disease at some particular parts of Egypt. The first mention of it is as coming from that country.

What authors do you refer to?—First to Thucydides; though I am myself of opinion, that the plague of Athens, mentioned by him, was

not the plague of the present day. The other authors are Muratori, Guastaldi, Foderé, Nacquart, and very recently Jourdan and Valli.

Are cases of plague pretty frequent in the division of Constantinople called Pera?—Not so frequent, of course, as in Constantinople, because every Frank takes precaution against the disease.

Is the suburb of Pera differently built from Constantinople?—It is a little more elevated, and is a long narrow street. As to the houses, many of them are of stone, whereas in Constantinople they are chiefly wood; and the streets are wider at Pera than they are at Constantinople, generally speaking.

Is Pera upon the whole, from its situation, a more airy place than Constantinople?—More than some parts.

Do you think Pera is a less likely situation for the production of any disorder peculiar to the climate, than Constantinople?—I should say that there is no difference, except with regard to the crowded state of the houses; the topographical difference would not make much. The street leading from Constantinople to Pera is one of the dirtiest streets in Constantinople.

Have you been at Aleppo?—I have been at Aleppo.

Do you know that caravans proceed very frequently, for the conveyance of goods from Aleppo eastward, through the continent of Asia?—Certainly.

Have you ever heard that the plague was conveyed by those caravans eastward, so as to establish itself?—Not except some of the few thinking Christians; the mass of the people never think of the disease at all. I have heard it among a few persons I have conversed with, and who thought the plague could be carried.

Has it actually been carried eastward?—I have no knowledge myself.

Do you believe it?—I do believe it:

Have you heard so?—I have heard so, but I do not know it.

In cloth carried eastward?—In the caravans leaving Aleppo. I have no knowledge myself of the fact.

Have you ever heard of any city to which the caravans proceeded, becoming the seat of the plague?—Damascus, in 1804; it was carried by the army of some pashwa, who had been on the coast to assist in the reduction of Jean d'Acre. Bagdad is often, and has been lately, infected with the plague.

Have you heard of any other instance?—None from Aleppo.

And none of the plague carried eastward?—Not to my own knowledge.

You have not heard of the plague establishing itself, and destroying the inhabitants, in any of the interior cities of Syria, except in Damascus in 1804?—I have had no means of investigating that.

Have you ever been in Smyrna?—I have been in Smyrna.

At the time of the plague?—Not when it was very prevalent; but when a few cases were reported to the different consuls, so as to induce them to give to the vessels a bill of health, which is called a Suspicious Bill, *touched*; that is, infected.

Have you ever heard of the plague being communicated from Smyrna

to the interior cities of Asia Minor?—I have not heard any particulars.

Have you ever heard of the plague being at Brusa in Bythnia, eastward of Constantinople?—I will not take on myself to say.

Have you heard of the plague being communicated westward of Constantinople, over land, to Adrianople and other cities?—I have.

To Adrianople?—Yes.

Do you know instances of the plague being destructive at Adrianople?—I believe the plague raging in 1812 was nearly as fatal as it proved at Constantinople.

Have you heard of the plague being communicated from vessels from Smyrna to many parts of the Levant?—Continually.

What is your opinion as to the infectious nature of the plague to be carried in bales of merchandize?—If the word infectious is to stand in my examination, I beg leave to qualify it. The plague is not infectious, as the yellow fever is; but it is contagious, which explains the way in which it is carried; for infection cannot be carried.

Explain the difference between infection and contagion?—Contagion is a mere mode of action resulting from the habit of certain diseases to affect individuals; it is not a principle, such as the electric fluid and such kind, as many persons give an idea of in their writings, flying about the air. Contagion expresses this: during such a disease as the plague, there are certain animal emanations which partake of the morbid state of the body from which they issue; when these are applied by direct contact, or by any mediate contact, namely, objects on which these emanations rested, to an healthy body, it will contract the disease. Infection is this: infection is a peculiar state of the atmosphere, which has been rendered unfit for the healthy exercise of life, by the crowding together of a number of persons ill of the same fever in a given place, and during a given time; thus an epidemic may become infectious.

You mean epidemic influence. What is your opinion of the contagious nature of the plague to be carried in merchandize from one country to another?—The answer is included in the one precedently given: I stated that the plague, and two or three other contagious diseases, seem to give out, as the body does, certain emanations, which must partake of the same disease as the body. If these are applied to goods liable to receive and nurse it, even then the principle of the disease, (if you call it the principle, but I am averse to such a name,) may be conveyed by such articles containing the emanations being carried about.

Do you think that articles so contaminated by the matter of the plague would retain the matter for a long time, so as to communicate the disorder upon being touched?—There are examples, and those very authentic, proving that this matter of the plague can, if applied to an healthy body, cause the disease to break out even at a very long period after; and I should mention, several months. There is one instance in point among the most recent, and it rests on the highest authority. During the plague at Corfu in 1815, one of the villages, which had been infected several months, had for some time, I believe

for forty-three days, exhibited no sign of the plague, owing to the measures of segregation adopted by Sir Thomas Maitland; the village was reported to be released, and fumigation, preparatory to its receiving *pratique*, ordered. The officer who had the surveillance of the village, during the three or four months had resided in the church, from there being no house that was not thought infected; in which church the people and the priest had been crowded just before the laws of segregation were ordered by Sir Thomas Maitland: some of these died subsequently, for the church was ordered to be shut the instant the plague begun. It was therefore necessary to purify the church before the people could go in again, as well as the village altogether. Leave being granted, the priest went in, and touched the cloth of the great altar, so as to shake it to purify it, when he was seized with the plague, beginning with the head-ach, so as to cause him to fall on the steps of the altar almost immediately; and in three hours, before he could be carried to the lazaretto, he expired, with buboes under the arm and livid spots over the body.

I thought you said, in a former part of your evidence, that the buboes only appeared when the disorder continued certain hours?—When it continues sixty hours, they are sure of coming, but they may be earlier. I merely said that the glandular swellings must appear if the disorder goes beyond sixty hours.

Do you think, from your general knowledge of physiology, that the priest then took for the first time the infection of the plague, and sickened on the spot, so as to die of the plague in three hours from the time of touching the cloth of the altar?—From analogy to the rapid action of other poisonous substances with which I am acquainted, I should think there was not the least doubt as to the priest having been infected for the first time.

And do you think he took the disorder by contact through the skin of his hand with which he touched the cloth, or by any effluvia from the cloth which he might have inhaled at the time, when he went so near as to touch it with his hand?—Certainly through the skin; because, if the effluvia had arisen from the cloth, there is no reason why the officers who had resided there two months shut up in the church, should not have felt the effect.

Did he fall down upon the steps of the altar, immediately after touching the cloth?—He was seized with a dizziness of the head; touched with the plague: there came on dizziness, and he fell in a fainting-fit; he was seized with the plague. It is the usual expression. The case is stated thus: he was seized with the plague, and died in three hours afterwards.

Do you believe the case to be exactly as you have reported it?—From my own knowledge of several facts, such as I can have no hesitation in believing.

Have you seen such instances yourself?—I have not seen them myself; but I have had them from such good authorities, where I met with them, that I cannot doubt their truth.

And such an event is agreeable to your general knowledge of physiology?—Certainly; I do not see any law in physiology that can

prevent the belief that virulent poisons can be carried into circulation; and go through the lymphatic system in less time.

Then, according to your opinion, the plague must be brought: the contagious matter of the plague must be brought either by persons or in bales of goods on-board ship from the Levant to England; and persons touching the infected portion of merchandize packed in these bales, must exhibit such phenomena in the lazarettos in England as you have described to have been reported to you to have happened in the village in Corfu?—Without the smallest doubt, that is my firm belief. Cases in point have happened at the lazaretto at Leghorn, since 1814; at Marseilles within fifteen years, twice; and recently, according to the dispatches of Mr. Hoppner, the British consul at Venice, in October 1818.

But you have not heard of such instances in the lazaretto in England?—No.

Have you ever heard of the plague being caught by any of those persons appointed to see the quarantine-laws put in execution in the lazarettos in England, and who in discharge of their duty must be liable to such communication with goods and persons as would expose them, one would think, to the contagion?—I have not heard of any cases of the kind happening in England, to my personal knowledge; but I have never made inquiry in England.

Do you think it extraordinary that, in the lazarettos in England, none of the officers appointed to carry the quarantine-laws into execution should not have caught the plague?—In order to answer whether it is extraordinary or not, I ought to know whether a vessel arriving at a lazaretto has performed any quarantine or made any stay in the Mediterranean ports; whether they have come direct; whether, before the bales have been touched, there has been purification and other means of precaution observed.

You have stated, that the contagious matter of the plague, being entangled in goods fit to preserve it, may remain in a state to communicate the disorder for many months?—I do think so.

Do you not think it likely, that by some accident in a long series of years, from the contagious matter of the plague, and from the nature you think it to be, it might have been introduced into England from the ports of the Levant?—I should humbly conceive, that its non-appearance in England does not do away with the contagious nature of the disease: it must be supposed that the goods had the plague; I should say, then, that the expurgators could not avoid having the plague.

Then, as they never had the plague in our lazarettos, you naturally conclude that those goods which arrived were not, on their arrival, infected with the plague?—Certainly not, if they had been touched by several individuals on their arrival before purification, and they had not excited symptoms.

Are you aware that, during the prevalence of the plague in the Levant, goods are in general not allowed to be shipped for England, under the quarantine-laws, till after the disease has ceased?—I am perfectly aware of it.

Would not the length of time after such goods were shipped, and the length of the road to England being so great, be sufficient to account for the contagion being considerably weakened?—If the length of time is very great between the time of shipping and unloading, and if certain circumstances have taken place, either on the removal of the cargo during the voyage, or in altering it, or the vessel's meeting with bad weather and being washed over and over again, it is not improbable to suppose that part of the plague-matter, if any existed in the cargo or attached to any part of the vessel, may have been weakened in its virulence: but I beg to give that as a supposition, and not as my belief, because we know that all poisons may be qualified by many circumstances, so that the strongest may not have effect. A barrel of gunpowder may not take fire with a red-hot poker, under certain circumstances; that is, if by moisture you render it incapable of combustion.

Are not different persons, in different climates, more or less susceptible in various degrees of contagion?—No doubt: the prevalence of certain circumstances, both with respect to individuals and climates or seasons, would very much forward or diminish the chance of the person receiving the effect of contact.

Would not these circumstances, and the length of time which elapses in a voyage between the Levant and England, account for the very rare degree of contagion which has taken place in England, without supposing the impossibility of it?—I should think it scarcely probable, that if what I have called a contagious matter is in the bales of goods, unless the period of time is very great, that it would fail to excite the disease.

Would that account for its not having taken place during the last 154 years?—The only way I can account for its not having taken place is, that it was never shipped from the Levant; for, though I admit there is a probability that circumstances will diminish the virulence, I do not admit that, if the disease is shipped on-board, any circumstances will prevent its spreading.

Is not the fact of its never having occurred for 154 years sufficient to inspire the confidence that it cannot exist in a British atmosphere?—Neither 154 years, nor six or seven centuries, can give such hope, when we know that such a disease existed before.

Was the plague of 1665 the plague of the Levant?—No doubt, according to Dr. Mead.

CRITICAL ANALYSES

OF

RECENT PUBLICATIONS, IN THE DIFFERENT BRANCHES OF MEDICINE AND SURGERY.

"I would have men know, that, though I reprehend the easie passing over of the causes of things, "by ascribing them to secret and hidden vertues and properties; (for this hath arrested and laid asleepe all true enquiry and indications;) yet I doe not understand but that, in the practical part of knowledge, much will be left to experience and probation, whereunto indication cannot so fully reach: and this not only in *specie*, but in *individuo*. Yet it was well said, *Vere scire esse per causas scire*."—BACON.

Practical Observations on the Colchicum Autumnale, as a general Remedy of great Power in the Treatment of Inflammatory Diseases, both acute and chronic; and therefore as a Substitute for Bleeding, in Disorders which are connected with increased Action of the Heart and Arteries. By CHARLES THOMAS HADEN, Esq. Surgeon to the Chelsea and Brompton Dispensary; late Surgeon to the Derbyshire General Hospital; Member of the Medico-Chirurgical and other Societies. 8vo. pp. 84. Burgess and Hill, London. 1820.

THE powerful agency of colchicum in gout and rheumatism, has for some years been so generally admitted by medical practitioners in this part of the world, that, however common it may be to write what has been better written before, few persons, we believe, would think of gravely announcing its utility in these complaints, as a matter of novelty. Mr. HADEN, accordingly, lays claim to originality on different grounds: first, as having introduced a new form of exhibition; and, secondly, of having discovered in colchicum the power of producing certain effects on the sanguiferous system. "In both these particulars, therefore, the writer claims for this pamphlet the praise of containing something that is new and useful."

Occasional mention is made of colchicum by the ancient writers both of Greece and Rome, by whom it was ranked among the poisons; but, we believe, the earliest particular account of its properties was given by PAULUS ÆGINETA, who regarded it as a remedy of great efficacy in gout, the symptoms of which, he asserts, were generally removed by it within two days. He goes on to say, "I also knew a man who prescribed hermodactylus, not according to the usual forms and in substance, but he boiled the plant with anise and parsley, and gave the decoction to be drank." DEMETRIUS PAPAGOMENUS, who

wrote in the thirteenth century, likewise particularly mentions it; a translation of one of whose formulæ we subjoin:

R. Aloes 3j.
 Hermodactyli . . 3ss.
 Cinnamomi . . . 3jss.
 Scammonizæ . . . gr. x.

Ex iis fiant pilulæ, dentur pro viribus materiæ copia.

Mr. KERR (to whose "Medical Sketches" we refer our readers for some curious information on the early history of this drug,) goes on to remark: "In this prescription we have no account of the exact preparation of each of the ingredients, nor of the medium to be used in forming them into pills. Most probably, the dry root of the hermodactylus was powdered, and some simple addition made to the aloes, that the whole might be made into a ductile mass."

In the reign of Queen Elizabeth, a translation of WERTZUNG'S "Praxis Medicinæ Universalis" was published, in which a particular description is given of this plant, and very favourable mention made of its virtues. More recently we find it enjoying its reputation, and entering into the "Pulvis Arthriticus" of Sir THEODORE MAYERNE, in which it was combined, among other *et cætera*, with the powder of unburied skulls, (*cranii humani insepulti*;) an ingredient which, it would appear, was not much relished by his Majesty King James the First; for it is added in a note, "N.B. In casu D. N. Regis qui *ανδροποφαις* odit, cranium humanum poterit in ossium bubulorum rasuram permutari." What alteration the efficacy of the nostrum suffered from this change, we are not informed.

From the foregoing remarks and quotations, it appears that colchicum has been used in medicine from a very early period, and *generally in the solid form*; so that, if there be any novelty in our present practice, it consists in our having discontinued its exhibition in that state: and Mr. Haden has, therefore, the merit of recalling our attention to the manner in which this drug was given in former times.

So much for the form of exhibition. A more important question, however, is to ascertain whether Mr. Haden be or be not right in regarding colchicum "as a remedy of great efficacy in controlling the action of the heart and arteries, *and therefore* as a substitute for the lancet in the treatment of inflammatory diseases, and of those acute and chronic complaints which are designated diseases of excitement." We have heard it objected, that a reviewer cannot fairly argue against a remedy he has not tried; and we acknowledge that we have not seen this drug given in the *form of powder* sufficiently often to

judge of its effects. But, against this imagined necessity of pitching experience against experience, and case against case, we must protest, and beg to remind those gentlemen who use this shallow argument, that when any one is happy enough to discover a new remedy, or new virtues in one already known, that the *onus probandi* lies entirely with such discoverer, and it becomes the duty of the reviewer to examine carefully the evidence by which the pretensions to novelty are supported, and to point out wherein it is deficient.

It is true that we possess certain remedies which do command, to a great extent, the actions of the sanguiferous system,—such are digitalis, antimony, perhaps elaterium, and prussic acid: it seems, too, that when Mr. ALCOCK publishes his promised work on Mucous Membranes, we are to be made acquainted that the same power is likewise possessed by ipecacuanha. It appears, however, that the power of a remedy in controlling inflammation, is not in the ratio of its command over the heart and arteries; for no other medicine possesses so marked an influence on these organs as digitalis, yet its power of lessening inflammatory action is by no means equal to that of antimony, although antimony cannot be made to govern the pulse with nearly so much certainty as foxglove: and, consequently, we regard Mr. Haden's reasoning as inaccurate, when he argues that colchicum, possessing great power in controlling the action of the heart and arteries, is "therefore" capable of subduing inflammatory action. None of these remedies, we suspect, however, is capable, at least in this country, of being substituted with safety for blood-letting; and a medicine capable of really fulfilling this indication, is still a great desideratum in physic. We must confess, however, that we scarcely believe any such medicine exists; and still less do we expect to see any individual drug controlling the various forms of inflammatory action, modified as it is in all its phenomena by the structure it attacks and by other adventitious circumstances.

The author, however, seems to think differently; and his own opinions, as well as those of his father, will best appear from the following quotations:

"I intend, in this essay, to offer to the profession the colchicum autumnale as a most powerful means of subduing increased or irregular action, or what we call inflammation in the constitution; and to show its powers of lessening the necessity for employing more hurtful remedies, such as bleeding, in acute cases of disease."

"If it be given every four hours, until it produce an abundant purgative effect, the pulse will become nearly natural, from being either quick and hard, or slow and full. This frequently happens

even before purging has taken place; and the effect is so certain, that I never bleed, unless inflammation exists to an alarming degree in a vital part, and then never more than once.

"Fevers and inflammations so removed never require the use of tonic medicines during convalescence: the patients, indeed, generally appear to be as well as though they had not been at all the subject of disease; and, although it sometimes happens that a recurrence of symptoms take place, it is in a much milder degree, and the new disorder is always immediately removed in a few hours, by a very little of the same treatment."

In order to illustrate these doctrines, several cases are related, both of acute and chronic diseases, treated with this medicine; some of which are good, but scarcely any unexceptionable, as there is very seldom such a detail of symptoms as to enable the reader to judge for himself of the complaint; and, in most instances, the drug was combined with other remedies of acknowledged power. Besides, there are many of these cases so indefinite that, as they lead to no conclusion for or against the argument, we are at a loss to discover the object of their insertion. At pages 42 and 43, the three following cases appear in succession.

"Mrs. E. aged 30, was suffering from a common catarrh. She took six five-grain doses, and a small quantity of common opening medicine, in the morning, for a few days: after which, she took a bitter opening draught daily, on account of an old stomach affection; but was otherwise well.

"October 11th.—Mrs. L. aged 55, a corpulent person, who had suffered for some time from a slight degree of general feverishness, attended by heat at the stomach, and an itching eruption on the skin similar to urticaria. A *slight mercurial*, with *ippecacuanha*, was ordered for bed-time; and five grains of colchicum, with a drachm of sulphat of potash, in warm water, for the morning. A restricted diet was also recommended.

"26th.—She has continued to take the medicine, and is now nearly well.

"October 12th.—Mr. P. An old case of deranged digestive organs, with a white furred tongue, some heat of the skin, and considerable itching when warm in bed. Seven grains, with sal polychrest, were ordered to be taken every morning.

"26th.—He was better, but far from recovered: he had, however, not taken his medicine very regularly."

* * * * *

"Mr. B.'s servant, aged 16. This was a similar case to the last. The patient was going into the country, and eight powders were given to him, with directions to take one three times in the day. No report has been received of his progress.

"Mr. G.'s servant. Four powders were here given, for a *whitlow*; but, as the abscess was freely opened, the writer does not lay much stress on them as being the efficient means of relief."

"Mr. P. a baker, aged 30, applied for a *swelled face*, produced by the irritation of a portion of decayed tooth acting on the gum, in consequence of the habit being predisposed to fever. The piece was removed; but his tongue was much furred, and in the evening he had a severe attack of fever. Calomel and purging medicine were given; but, as he was very little better on the succeeding day, the colchicum was ordered. On the day but one after, he was met wheeling his barrow along the street; and had discontinued his medicine after the fourth dose. He was quite well on the sixth day."

Now, we can have no objection to the author prescribing colchicum in whitlow and tooth-ach; and, provided opening the abscess and extracting the carious tooth be premised, as in the examples just quoted, we have little doubt of the treatment being found efficacious.

Several cases are given of what is called "common fever," treated with colchicum; but the symptoms are not detailed. Mr. Haden, however, does not trust entirely to colchicum in idiopathic fever, but gives an instance, which we shall quote, of this disease *cured* by that remedy.

"For a reason which is mentioned afterwards, I have not hitherto trusted to the single use of colchicum in what we call idiopathic fevers; but, in the following case, it cured the complaint, and acted the part of an opiate, after calomel and common purging medicine only gradually and imperfectly relieved the symptoms. The colchicum, however, was pushed rather too far.

"*Case of simple fever treated by colchicum.*—Mr. Coates, aged 35, and weak, applied, with symptoms of common fever, on Sept. 28, 1819. Had had it for some days. The excitement was not fully developed, and the chief symptom was head-ache, which was rather severe. Ordered calomel and antimony, followed by black dose. Tongue was covered with a dull, dirty, brownish-grey, but smooth, slimy coat.

"29.—Was somewhat easier; had been purged. Rep. pil. et haust.

"30.—Head not much better; tongue beginning to clean at the edges; had been purged. Colchicum et sal polychrest. ter die.

"October 1.—Much better; was tranquillized in a very remarkable manner. Tongue much cleaner at the edges; great sweating produced; had been purged. Continue.

"2d.—Still better. Continue.

"3d.—Not quite so well, the medicine beginning to disagree: it had created sickness and general uncomfortable feelings. Sweating and purging continued. Stool dark, and in pieces; tongue with a small quantity of the dirty appearance in the centre, but the clean

edges becoming covered with a thin white scurf, certainly from irritation on account of the over-dose of colchicum.

"This patient, from this time, required small doses of opening medicine for some days; when he was able to resume his common occupations."

Now, in perusing this case, the reader cannot but be struck with the very unsatisfactory detail of symptoms; for, though it is stated to have been fever, there is no mention, from first to last, of the state of the pulse; and of the skin we are only informed, that sweating followed the exhibition of the colchicum. With regard to the treatment, the medicine said to have cured the disease was not begun till, by repeated doses of calomel, with antimony and purging draughts, the tongue had begun to clean; a state of matters certainly very promising for any favourite remedy to play its part; and, had bread-pills been exhibited, the patient's recovery would probably have gone on without interruption. But, on the contrary, colchicum was ordered, which in three days had produced such general derangement as to require its discontinuance; after which, the patient got well. Similar to this are some cases published by Mr. RICE, in the *London Medical Repository* for January, as a kind of supplement to Mr. Haden's pamphlet: in all of these, except one, calomel and other powerful medicines were exhibited; and in some bleeding, both general and local, had recourse to. Nothing can show more strongly how unfit medical men, who have taken any particular remedy under their especial protection, are to judge of its real effects. We beg not to be misunderstood: we have ourselves employed, and seen others employ, colchicum largely, and, in certain complaints, with unequivocal advantage; and it is only when the author seriously proposes it as a substitute for bleeding, and other powerful means, in acute inflammation, that we must refuse our concurrence in his opinions, because they are not supported by sufficient evidence. We fear, too, lest the reputation of this excellent medicine should suffer from having virtues ascribed to it which will not, we believe, stand the test of experience.

There is another question of some importance in the therapeutic history of this drug: we mean, whether its purgative operation be or be not connected with its curative effect. Mr. Haden argues in the affirmative; and on this head, also, we differ from him. We have been in the habit of prescribing colchicum during the last three years, and have lately witnessed its exhibition on an extensive scale at one of the public institutions in this town. The result of our observations has been, that, when uncombined with laxatives, it does not induce

purging nearly so often as seems to be generally supposed; and, as we never could perceive that the occurrence of this circumstance was attended with greater relief than when no such effect was produced, we are now in the habit of immediately checking it. We must be understood as speaking only of the vinous tincture, not of the powder; but we presume that the fact of purging being connected or unconnected with the peculiar effects of the remedy, must apply equally to either form. In general, we are in the habit of beginning with twenty drops three times a-day, and increasing the dose five drops each day till the patient takes two hundred or upwards in twenty-four hours; and, while we daily witness good effects under this plan in gout, rheumatism, and affections of the mucous membrane of the air-passages, we seldom see any considerable purging induced by it. When, however, this does occur, we have found a few grains of magnesia, night and morning, of great use in commanding it; and the same advantage, we are inclined from some recent trials to believe, may be obtained from small doses of ipecacuanha.

While speaking of the action of this remedy, we may here remark, that the two writers who have volunteered as its advocates differ somewhat in their opinions; for, while Mr. Haden regards it as particularly useful in cases of *excitement*, Dr. Williams of Ipswich, on the other hand, regards it as beneficial chiefly in complaints of the *asthenic kind*.

It is but justice to Mr. Haden to say, that he does not seem quite satisfied with his own work. "It appears," he says, "that this is a very imperfect account of the medicinal effects of the *colchicum autumnale*. It fails in discriminating accurately the forms of acute disease in which the remedy acts most efficaciously: it does not point out those in which it is useless; it refers almost entirely to the use of the remedy when given only in one state and in one combination; and, therefore, the advantages which may doubtless be derived in many forms of complaint by giving it in conjunction with other kinds of medicine, are scarcely hinted at." Were authors in general to point out the errors of their works with as much fidelity as is done here, it would often save the reviewer an ungracious task.

General Elements of Pathology. By WHITLOCK NICHOLL, M.D. M.R.I.A. F.L.S.; of the Royal College of Physicians, London; Member of the Medical and Chirurgical Society; Corresponding Member of the Association of King's and Queen's College of Physicians, Dublin. 8vo. pp. 233. Callow, London, 1820.

Εγώ δὲ ἀρχὴν μὲν τῶν νοσημάτων ἵνα τῷ σώματι, ἀλλὰ πάντα ὁμοίως ἀρχὴν, καὶ πάντα τελευτῇ κύβητα γὰρ γραφέντος, ἀρχὴ οὐχ εὐρεθῇ καὶ τῶν νοσημάτων ἴσιν παντὸς ὁμοίως τὸ σώματος.—HIPPOCRATES de Locis, &c. cap. i.

THIS work has neither "Preface," "Introduction," nor "Preliminary Observations;" but, in compensation for the loss of these fashionable preludes, the author adduces, as an epigraph, an extract from the writings of BAGLIVI, in which he exposes his intentions, and designates pretty well, we think, the merits and utility of the book.

"*Hoc opusculum ut in publicum ederem, non fecit profecto inanis, ac popularis auræ captandæ cupiditas, sed eo adductus sum, ut multis meorum æqualium hinc inde errantibus viam monstrarem, et aliquantulum munitrem.*"

We like so well the abrupt manner in which the work commences, and we have been of late so heartily nauseated by the jejune and trite *exordia* which most of our modern critics, especially those of a neighbouring continental nation, append to their "Reviews," and lavish in a way that might alone lead a man of ordinary shrewdness to anticipate their inanity, and suspect that they must be constructed at no great degree of expence, that we are glad to have so good an apology as the example given us by our present author, for omitting a preamble about the utility of systematic works on science;—the relative value of those constructed solely of precise inferences from acknowledged facts, and those framed by adopting, in the first instance, partly on analogy, some general principles under which the requisite facts may be arranged with tolerable grace and facility;—some congratulations with our contemporaries on the establishment of the inductive mode of philosophizing now in vogue, and on the discredit of speculative reasoning; with a few complacent hints that we are the finest specimens of human nature, and that our forefathers, especially the "ancients," knew not how to use their intellectual faculties;—we are glad that we have so good an apology for omitting all this, and for entering at once on an exposition of the work before us.

The book commences with an "*outline of the human economy*," in which the author gives only a simple demonstration of sensible phenomena, in the order in which they occur, as the functions are generally performed in the healthy state of the system, without entering on any considerations respecting their causes, or the means by which they are effected, beyond

such data as are evident to the senses, or obviously exist in the mechanism of the organs by which those phenomena are developed. He begins by saying, that "Living man consists of an organized body, to which are attached life and intellect. The organized body may be described, as consisting of a system of supply and waste; of a nervous system; of various assemblages of contractile fibres, which are called muscles; and of a fundamental structure, which consists of bones, of cartilages, and of the varieties of membrane. The system of supply and waste, consists of the vascular system, and of its two appendages,—namely, the alimentary canal, and the pulmonary air-cavities, (air-cells.) The vascular system consists, of the heart; of the arteries; of exhalants; of secreting vessels, with their several ducts, reservoirs, and outlets; of sinuses and veins; and of absorbents."

Having described the distribution of the vascular system, the author then treats of the course of the blood, (which fluid is not, we cannot discern wherefore, mentioned amongst the constituents of "living man,") and the changes produced in it in the lungs, and by means of the fulfilment of the functions of the systems of *supply* and *waste*. The phenomena of digestion, and the functions of the absorbents, are next described; when the author remarks that, "although the office of all absorbing vessels is so far the same that they all take up, or receive, various kinds of matter throughout the body generally, yet may they be considered as performing three sets of offices. In the first place, they take possession of fresh supply, which is for the first time entering the system; as when fresh matter is absorbed from the internal surface of the alimentary canal, or from the pulmonary air-passages, or from other passages having external outlets, or from the surface of the body. In the second place, they take possession of fluids which form a part of the waste; as when exhaled, or secreted, fluids pass into absorbents. In the third place, absorbents take possession of matter which has previously been deposited from the vascular system, and which has formed a portion of the structure of the body; as when the more solid parts of the body are removed by absorbing vessels."

As the author, in this part of the work, treats of the phenomena of the body in the state of health alone, it is obvious that he asserts that the solid structures of the body are in that state undergoing mutation: a proposition which is not at all satisfactorily established, as the observations and reasonings of WINTRINGHAM, LISTER, BOHN, SCHELLAMER, GIBSON, and PRING, sufficiently indicate.

As this work is constituted, almost exclusively, of a series of aphoristic sentences that can be of utility only by furnishing

the means by which a reflective man may attain to some general views of the functions of the human economy, it might, with reason, have been expected that the materials, which are in fact but the "*Elements*" of a system of pathology, had been only such as are of undisputed validity, or that the author had more strictly distinguished the positive facts he adduces, from inferences which are merely probabilities. There are, however, but few specimens of the fault here designated, though this section of the work contains another, where it is said that "some of the extreme branches of this artery (the aorta) terminate in exhalants." It has never been demonstrated that the arteries any where terminate in exhalants: on the contrary, it is rendered highly probable, by the experiments and observations of LEUWENHOEK, COWPER, MALPIGHI, SPALLANZANI, RUYSCH, DE GRAAF, VIEUSSENS, and many other able anatomists, that the extremities of the arteries are every where continuous with the veins, and that the exhalants are, exclusively, given off from the sides of the arteries throughout the greater part or the whole of their course: a view of the subject that is not at all opposed by the results of the experiments of CRUICKSHANKS, HALLER, NUCK, and several other anatomists, who have filled the lymphatics of a part by an injection of the arteries of it.

This section goes on to treat, in a very general way, (the author referring to a work which he published a short time since, entitled "*A Sketch of the Economy of Man*," for details respecting the physiology of the body,) of the phenomena of respiration, the action of the heart, the properties of the arteries, and the influence of the nervous system; and this short section concludes with the first of a series of remarks that are illustrative of the motto to the work.

"Thus it appears that the several parts of the body are intimately connected with, and dependent upon, each other. For, the functions of the vascular system would cease, if those of the nervous and muscular systems were suspended; the functions of the nervous system would cease, if the offices of the vascular system were suspended; and the muscular system would be inert, were it not for the influence which it derives from the vascular and from the nervous systems."

Having thus treated of the phenomena of the human economy in the state of health, the author proceeds to consider, in a similar, though more particular, manner, those which occur in the deviations from that state; and he discusses them in an order, in regard to their origin, correspondent with that which he has adopted in passing in review the subjects of the former section. The first relates to the "*quantity of the blood.*"

It being part of our duty to give a specimen of the author's manner of treating the subjects of his disquisitions, we shall

transcribe the whole of the present section : it is one of the most concise ones in the book, and it will enable the reader to form an adequate idea of the general character of the work ; for each of the subjects of it, to be presently enumerated, is treated on in a precisely similar manner.

“ If there be an increase of the mass of blood, unaccompanied by an increase of the action of the heart, either as to force or as to frequency, the blood, although its quantity is increased, will have its momentum diminished.

“ In such a case, the tonicity of arteries being less strongly opposed, the capacity of those vessels,—of the smaller arteries especially, (in which, as I have already stated, the degree of tonicity is greater, in proportion to the size of the vessel, than it is in the larger arteries,)—will be lessened, so that an increased quantity of blood, and an increased proportion of the general mass of that fluid, will be contained in the larger arterial trunks, in the veins, and in the cavities of the heart. The whole round of the circulation will be obstructed. A diminished quantity of blood will pass into secreting vessels; but exhalation may be increased, owing to the lessened momentum of the blood. The action of the heart will consist of slow, feeble, contractions, or of ineffectual flutterings. The pulmonic process will be imperfectly performed, and respiration will be laborious and hurried. The quality of the blood will be rendered unnatural.

“ The circulation of blood of an unnatural quality through the cerebral blood-vessels, will induce disorder of the cerebral structure, and of its functions. From the obstructed state of the whole round of the circulation, congestion of blood may take place in the cerebral veins and sinuses, whence may ensue inordinate compression of the cerebral substance, and, consequently, disorder of the whole of the nervous system. Or, should the cerebral exhalants pour out an increased quantity of fluid, in consequence of the obstructed state of the cerebral veins, inordinate compression of the brain may also be produced, unless the exhaled fluid be removed in an equally increased proportion. The sensibility of nerves will be lessened, in consequence of the smaller quantity of blood which circulates through the blood-vessels in their vicinity, as well as from the altered state of that blood, and also in consequence of the disordered state of the cerebral structure with which their cerebral extremities are connected. The production of nervous power will be lessened, and the expenditure of it will be disturbed. The generation of heat, and the capacity of the body for heat, may be lessened. Muscular action will be feebly, or imperfectly, or irregularly, performed. Sensation will be sparingly, or imperfectly, produced. The depressing passions will prevail. The faculties will be feebly, or imperfectly, exerted. All these altered conditions of the several parts and functions of the economy, will produce disorder of the alimentary canal; consequently, digestion and chylification will be imperfectly performed, and the supply will be diminished and depraved. The performance of the pulmonic process will become still more deranged, in consequence of the disordered

state of the nervous system and of the intellect, owing to the direct effects which such disordered states produce on the respiratory muscles. In short, cause and effect will act reciprocally throughout the whole economy, producing complete disorder of every part, and of every function.

“ If the mass of blood be increased, and if the action of the heart be also increased, so as to propel the greater mass with freedom, the momentum of the blood will also be increased.

“ In such a case, the tonicity of the smaller arteries being more powerfully opposed, these vessels will yield more readily to the current of the blood; they will therefore receive a greater quantity of that fluid, and, consequently, an increased quantity must pass by their terminations. The quantity of secreted, and of exhaled, fluids will be increased. The blood will flow with greater force, and in greater quantity, throughout the whole round of the circulation. As an increased quantity of the blood will pass, in a given time, through the pulmonic circuit, respiration must be more quickly performed; otherwise, that fluid will not duly undergo the pulmonic process. The sensibility of the nervous system may be increased, and the functions of that system may be more freely performed. Nervous power may be more freely produced. Heat may be generated in increased quantity. Muscular action may be performed with increased energy, and to a greater extent. The depressing passions will subside, and the livelier passions will predominate. These altered conditions of the several parts and functions of the economy, will affect the state of the alimentary canal, will alter the processes of digestion and of chylication, and will influence the action of that canal.

“ It may happen that the increased quantity of blood received by the cerebral blood-vessels, may induce what I have termed *erethism** of the cerebral substance; or it may induce inflammation of that substance. Or it may happen, either from simple plethora of the cerebral blood-vessels, or from erethism of the cerebral structures, (whether the state termed inflammation be or be not present,) that an increased flow of fluid from the cerebral exhalants may take place, producing inordinate compression of the cerebral substance. Or simple plethora of the cerebral blood-vessels may cause the brain to be inordinately compressed. Such a state of the cerebral structures, in what way so ever induced, will cause disorder of the whole nervous system.

“ If the quantity of blood in the vascular system generally be lessened, the heart (having a smaller mass to propel, and probably receiving a slighter stimulus than before, and probably, also, having the tone of its fibres diminished, owing to the lessened quantity of blood that flows through the coronary vessels,) will act with less force or with less frequency, or its actions may consist of feeble contractions quickly repeated. The blood, then, will either flow in a languid manner, or it will be rapidly hurried through the round of the circu-

* See *Remarks on Affections of the Cranial Brain in Infants.* Transactions of the Association of the King's and Queen's College of Physicians in Ireland, vol. iii.

lation. The momentum of the blood will be lessened; consequently, the smaller arteries will more successfully oppose the entrance of that fluid into them. This increased resistance of the smaller arteries may still keep up, in the larger arteries, a quantity of blood nearly equal to that formerly contained in them. The quantity of secreted fluids will be diminished; but, if the blood flow in a feeble manner, the usual quantity, or even an increased quantity, of fluid may pass from the open mouths of exhalants.

"If the blood, although its quantity be much lessened, flow with increased rapidity, the velocity with which it is distributed may, in some respects, compensate for its diminished quantity: still, as the actual quantity of it is lessened, the quality of the fluids separated from it in secreting vessels will not be the same as before, even supposing that those fluids are secreted in the usual quantities.

"As a smaller quantity of blood will, in the case now under consideration, flow through the blood-vessels of the cranial and spinal brains, the functions of those structures may be, more or less, suspended or destroyed. The nervous power will be sparingly produced, and it may be irregularly distributed. The sensibility of the nervous system, generally, will be lessened. The temperature of the surface of the body will be lessened. Sensation may be feebly and scantily produced. The livelier passions may be wanting, and the faculties may be feebly and imperfectly exerted.

"It may happen, in consequence of the languid motion of the blood, that an increased flow of fluid may take place from the cerebral exhalants, in which case inordinate compression of the cerebral substance may arise.

"All these altered states of the nervous system, of the muscular system, and of the intellect, will re-act upon the system of supply and waste, and upon the parts appended to it, increasing the disorder of these several parts.

"The quantity of the general mass of the blood is dependent upon the quantity of supply which that fluid receives from absorbing vessels, and upon the extent of the waste which it suffers from the processes of secretion and of exhalation."

Amongst the objects (besides that already mentioned) which we have had in view in making the foregoing transcription, one is that of acquainting the reader that an abstract analysis of this work is not to be expected in this article,—it must be evident that it will not admit of such a production; and another is that of showing the grounds on which we establish the principal critical remarks we have to make on these "*Elements*" as a medium of instruction in pathology. We think they are qualified to fulfil, to a very considerable extent, the intentions expressed by the author in the epigraph which we have quoted. It is especially to those who approach somewhat towards "*æquales*" of the author that the work will be of much utility; and it will prove useful to them principally, by furnishing precise, and, with but a few probable exceptions, correct, ideas

of the phenomena of the human economy, in its various states of disease, derived, generally, from views equally minute, perspicuous, and comprehensive, with those manifested in the passage above transcribed. We do not mean to state that it is not qualified to benefit the young student: on the contrary, we earnestly recommend a perusal of it immediately after he has attained a moderate degree of anatomical information; but a student might retain the whole of the book in his memory, and yet, if he were not otherwise instructed, be at a loss to know the nature of almost any one of the diseases he would meet with in walking round the wards of an hospital, in consequence of its not presenting any thing like pictures of the distinct groups of symptoms which ordinarily accompany individual forms of disease. There is, in the work, a section devoted to "general Inferences;" but these are not calculated to supply the deficiency here alluded to. As nothing respecting symptomatology is advanced, no notice, of course, is taken of the peculiarities in the phenomena and effects of disease dependent on peculiarities of the structure—as the mucons, fibrous, and serous, membranes,—in which it is seated. We are aware that the author has not professed an intention of adapting his work for the instruction of students, properly; and our object in the foregoing remarks has only been to express somewhat fully our opinions respecting the extent of the utility of these "Elements." Pursuing our intention, then, we may further observe, that this work is qualified to be very useful to the young student, by supplying him with precise ideas of the phenomena of the human economy, devoid of any general abstract notions of disease, which can only prove embarrassing in the first instance; but, when he has acquired such ideas, he should leave the study of it for clinical observation and treatises comprising histories of individual diseases, and more abstract and general notions in pathology, and revert to it when these have been duly attended to, as a source of the most important of the essential facts concerned in the origin and progress of diseases, that is characterized by the qualities which we have already conceded to it. The study of it will also tend to generate habits of more minute as well as more extensive circumspection, in his clinical observation, than he might, probably, fall into without such a guide. Although it possesses such qualities, we, however, suspect that the book will never become a popular one, (and that it should be such, does not appear to have been the intentions of the author:) a great proportion of the medical men, to whom such a treatise may prove useful, like books that require less mental exertion on their part, in order that inferences may be obtained from them applicable to the purposes of practice. There will not, however, be wanting

those who will think that Dr. Nicholl has rendered an important service to medical science, by forming a summary of the "Elements of Pathology" acknowledged at the present day, that, although it wants the lustre and highly important uses of general principles founded on abstract theory, has the advantage of being almost wholly devoid of hypotheses. We say *almost* wholly devoid of hypotheses, because, besides the notions already alluded to respecting the functions of the absorbents, it comprises some opinions on the nature of inflammation that have the character just indicated. These will be noticed hereafter.

The work being, as it appears to us, what the author intended it to be, it would be impertinent in us to advert in a particular manner to the superior value of treatises which comprise more general or abstract notions of the relations of morbid phenomena; but, there is one very important related series of such phenomena, the laws regulating which are really "*Elements*" of Pathology, that is not at all indicated in the work of Dr. Nicholl, and which, indeed, could not, perhaps, have been considered without a deviation from the plan he has followed in his dissertations. We allude to the phenomena termed *sympathetic*. A great deal of vague and silly declamation has been uttered against the use of the word sympathy in pathology, by persons ignorant of its etymology, and of the sense in which it has been employed by medical writers. No intelligent practitioner doubts of the occurrence of related morbid phenomena, the connection of which is not explicable by any principles founded on the anatomy of the body, or by a generic mode of function, and which occur too regularly to be attributed to chance; and the term sympathy is applied to the relation in question, not with the view of *explaining** it, but merely to express that relation; as the word *attraction* was employed by NEWTON to designate a *result*, without pretending to state whether its cause existed in the subjects of it, or in some other bodies extraneous to them. We shall notice this subject more particularly in an ensuing part of this Journal. At present we direct our attention to the part of the work before us where our last transcript terminates, for the purpose of passing in review the rest of it, though we know not what more we can do than designate the particular subjects of the several sections of which it is constituted, and

* This word is here used in its ordinary sense, though, in fact, *explanation* is but the assimilation of phenomena of which we know but little, to others with which we think we are better acquainted: thus, after having ascertained that the centripetal force of the moon and *weight* were conformable, in their effects, with the same laws, Newton affirmed their common cause to be *gravitation*. We fancy we have some ideas of gravitation; and so, by this *explanation*, we think we are enabled to understand in some degree the cause of the centripetal force of the moon.

point out the opinions contained in them that are, more or less precisely, peculiar to the author: and we wish to have it understood, that when we cite the title of a section without making any remarks on it, that section contains no such opinions as those just designated; the characteristics of them being the same with those of that on the "quantity of the blood."

The next section treats of the "*action of the heart*:" then ensue others on "*secretion*," "*exhalation*," the action of the "*veins*," and of the "*absorbents*." From that on the latter subject we extract the following remarks, as the doubts they involve of the direct influence of certain medicines on the action of the absorbents, may be regarded as notions proper to the author. The best alternatives, in the way of inferences, that we are acquainted with, on this point, are stated by Mr. PRING, in his essay on the absorbent system, (pp. 33—45.)

"It is easy to comprehend," says Dr. Nicholl, "how medicines, which are admitted into the alimentary canal, may get into the mass of circulating blood; and how they may, consequently, affect the condition of that fluid, and the state of the vessels in which that fluid is contained. But we cannot well conceive how such medicines can act upon absorbents, unless we suppose that they are poured forth from exhalants, and that they then act upon the mouths of the absorbing vessels, or that they act upon absorbents through the medium of the nervous system. Digitalis and squill, when admitted into the alimentary canal, cause an increased secretion of urine, as do other medicines belonging to the class diuretics, which also produce the removal of dropsical accumulations. But neither digitalis nor squill, nor other diuretic medicines, procure the decrease of dropsical accumulations, unless they cause an increased production of urinary fluid, or produce an evacuation of watery fluid by some other outlet. It seems probable, therefore, when the exhibition of digitalis, of squill, or of any other diuretic medicine, is followed by a decrease of any dropsical accumulation, that such remedy must produce this effect by causing a suspension, or a diminution, of the process of exhalation, either by its direct effect upon exhalants, or, as is more probable, by causing an increased separation of fluid by the kidneys; in consequence of which, exhalation is diminished or suspended. For it is plain that, if exhalation be diminished or suspended, while the absorption of the effused fluid continues to the usual extent, a diminution of the quantity of accumulated fluid must ensue. In short, we seem to be warranted in considering that, when diuretic medicines procure the removal of dropsy, they produce this effect by their direct influence on the process of secretion, and by their consequent influence on exhalation, rather than by their influence on absorbents."

The author does not altogether deny that stimulants may directly influence the absorbents, and thus increase absorption; for he says (in his aphorisms respecting absorption) that it seems probable that those vessels possess a contractile structure;

and that the application of stimulants may cause increased contraction of that structure.

The ensuing sections relate to the "*alimentary canal*," the "*pulmonic process*," the "*cranial brain*." In the last-mentioned section, the author, after BORDEU and some other writers, employs the term *erethism* in the same sense as *excitement* has ordinarily been used by the generality of pathologists. Erethism of the brain, he considers, may exist without any alteration of the state of the vascular system of the part; but it is disposed to cause an increased quantity of blood to flow through those arteries, and this combination of phenomena, he believes, constitutes inflammation. This is a point of doctrine that wants confirmation: it is by no means proved that an increased quantity of blood flows (in a given time) through the arteries of a part in a state of inflammation; and there are not wanting forcible arguments for a converse inference; for some of which we may refer to our review of Dr. Hastings' Treatise on Bronchitis.

The author then treats on the "*spinal brain*," and the "*sensibility of nerves*."

"There are," he says, "two kinds of habits, or temperaments, in each of which there is an unusual degree of sensibility of the nervous system generally. In the one, there is a great disposition to the production of heat; in the other, there is a great disposition to a diminution of temperature. In the one, any sudden alarm, or agitation of mind, or bodily pain, induces great increase of temperature, which is attended with dryness of the skin and with quickened action of the heart: in the other, such mental affections are attended with great coldness of the body, diminished action of the heart, and profuse perspiration. In the one, the skin is scarcely ever moist, and the pulse is rarely slow: in the other, the skin is generally damp, and the pulse is rarely frequent. In the one, there is great muscular strength and activity; while the other is delicate and feeble. In the one, there is generally an active, acute mind, whose energies are called forth by passions or by pain: in the other, there is rather a feeble, dispirited state of mind, which by powerful passions, or by pain, is rendered inert."

The "*nervous power*," "*temperature*," and "*muscular action*," are next discussed, (the last subject in a very extensive manner;) and then the dissertations of this class conclude with one on "*intellect*:" after which follows a section devoted to "*general inferences*." The author here, in the first place, adduces a series of remarks, the chief object of which is to show the mutual influence of the various phenomena of the economy on each other,—or, in other words, to demonstrate the truth of his motto: *It appears to me, (says HIPPOCRATES,) that no one of the phenomena of the human body can be regarded as the prin-*

cept, or source of origin, of the rest; but that all are equally principles and all final results. We cannot mark out, in a lined circle, any point at which it begins. Dr. Nicholl then adduces some reflections, of a general character, on the nature of disease; the tendency of which is to show the impossibility of constructing any nosological system that is not qualified, to a certain extent, to lead to error, by confining our views to merely partial assemblages of the phenomena of morbid states, and, using the metaphor of GALEN, by leading us to mistake shadows for the objects to which our attention should be directed. He then, alluding especially, it would appear, to the arrangements of SENNERT and his followers, and PINEL, observes that—

“There are two ways in which the bodily structure of man may be described. We may speak of it as being composed of distinct sets of structures, as of those which are comprised under the terms vascular system (and its appendages), nervous system, muscular system, and basal structure; which latter term comprehends the bones, the ligaments, and the varieties of membrane: or we may treat of it, after the manner of geographers, as consisting of the head, of the neck, of the thorax, of the abdomen, and of the upper and lower extremities; subdividing each of these parts, and enumerating the several contents of each. An equal degree of mischief may result from each of these modes of considering our structure; for, whichever mode we adopt, we acquire a habit of considering each portion which we enumerate, as a distinct and insulated part. The consequence is, that when any deviation from the healthy state occurs in any one of the subdivisions which we have made, our attention is fixed upon the diseased condition of this particular portion of the body, while every other portion is supposed to preserve its former healthy state.

“Thus, those who have adopted the first mode of considering our bodily structure, speak of diseases of the nervous system, diseases of the vascular system, and diseases of the muscular system. In so doing, they are incorrect; in as much as a disordered state of either of these systems implies also a diseased condition of each of the other systems.

“Those, on the other hand, who adopt the latter mode of regarding our bodily fabric, speak of diseases of the head, of the chest, of the abdomen, of the skin, &c. They subdivide these into diseases of the cranium, of the cranial brain, of the lungs, of the heart, of the liver, of the stomach, of the bowels, of the spleen, of the pancreas, and so on. These persons act incorrectly, also, because, in as much as, in each part which they enumerate, there are portions of the general nervous system and of the general vascular system, it is evident that each part cannot be considered as a distinct, insulated republic, but as a constituent portion of the general commonwealth, whose health is dependent upon a certain condition of every portion of the body.

“As the several animated beings of our globe have been reduced into classes, orders, genera, and species, in which they have had their

several places assigned to them, from some leading characteristic in their form, their habitation, or their economy, so has an attempt been made to form a similar arrangement of diseased states.

"If we examine the different arrangements which have been framed by nosologists, we shall find that each class, and indeed each sub-division of each class, is, for the most part, founded upon some leading symptom, or upon the preponderance of the deviation from the healthy state occurring in one particular part, or in one particular function. Since, then, such symptom, or such preponderating deviation, may, as we have seen, be the result of an altered condition of any part or function of the economy, it follows that no one of such classes, nor any one of the sub-divisions of either class, can comprehend the result of any peculiar primary deviation from the healthy state; for the same primary deviation from the healthy state may induce conditions of the economy which are comprehended under each of the classes, and probably under each of the sub-divisions of each class. In other words, diseased states which are arranged under a variety of distinct heads, may alike be the consequence of the same primary deviation from the healthy state: and, on the other hand, each diseased state contained in each class may alike arise from an altered condition of any one part, or of any one function, in the economy."

The nosology of Cullen is then more particularly examined, in pursuance of the foregoing views; and, finally, the ills that arise in practice from a devotion to such classifications, are pourtrayed with equal force and precision.

Three Appendices are attached to the work: the first of which consists of an analysis of the phenomena of fever, constituted after the manner of the sections in the body of the work, and, consequently, is not adapted for an abstract exposition; nor is it a fit subject for any other judicial remarks than such as we have made on the author's dissertations in general. We may refer to the *Proemium* to the present volume of this Journal, for details on this subject. The second Appendix comprises a disquisition on inflammation, of a similar character to the foregoing one; only that this comprises the hypothetical opinion respecting the state of the circulation in an inflamed part, noticed on a former occasion. The last is constituted of a series of "Aphorisms respecting Absorption." Of the merits of this part in particular, we could only express the same opinions with those we have adduced respecting the work in general.

CRITICAL ANALYSES
OF
RECENT PUBLICATIONS, IN THE DIFFERENT BRANCHES OF
MEDICINE AND SURGERY,
In the Literature of Foreign Nations.

Παρίδες ἀρα
Ἀνδράσιν, ἡ παλαιὴ ἀνδρεία, ἀγαλλόμεθα.

Principes généraux de Physiologie-Pathologique, coordonnés d'après la Doctrine de M. Broussais. Par L. J. BÉGIN, Chirurgien Aide-Major à l'Hôpital Militaire d'Instruction de Metz. 8vo. pp. 390. Mequignon-Marvis, à Paris, 1821.

ABOUT a year since, we noticed a book professing to be the substance of the Lectures of Dr. Broussais, and of which we promised to present a particular account to the readers of this Journal; but, on further consideration, we determined not to do the Professor at the Val-de-Grace so much injustice as to promulgate such vague, imperfect, and desultory statements, under the title assumed for them. Dr. Broussais has himself published only a Treatise on Chronic Inflammation, a work entitled an "Examination of the Medical Doctrine generally adopted," and some Memoirs in the *Journal Universel des Sciences Medicales*, and other Journals; of the whole of which an abstract has been given in the Exposition of his Doctrine inserted in some late Numbers of this Journal, and the Historical Sketches of the Progress of Medicine: so that, on perusing the work of Mr. Bégin, and finding him say, in the Introduction, if there is any thing good in it, it must be attributed to Dr. Broussais, we are obliged to infer that the Professor has developed many views in his Lectures that are not to be found in his printed publications. Yet, even admitting this supposition, we cannot but admire the modesty of Mr. Bégin, in claiming for himself so little merit as he has done on this occasion. The work is composed in so methodic and perspicuous a manner, and presents such a well-arranged exposition of the principles in physiology that are of most essential importance in regard to pathology, that, as a mere compilation, it might obtain no small degree of credit to a writer who had even acquired so much estimation as had already been attained by Mr. Bégin. This work is not, indeed, a mere exposition of the doctrines of Broussais; for the author has advanced objectionable criticism to his opinions, and has in some instances deviated from his principles in the theories he endeavours to establish; and he has himself shown, in a very favourable point of view, the benefits that will arise to pathology from the further application of the knowledge of the human economy in the state of health than had hitherto been effected.

Amongst the reflections contained in the "Introduction," (which is

full of judicious considerations,) the author adduces some good remarks on the utility, and indeed absolute necessity, of theory, which he concludes with saying, that, "notwithstanding their continual declamations against theories, the empirics always suffer themselves to be guided by the ideas they form of the nature of the disease; and they constantly appropriate, in spite of themselves, their remedial measures to that supposed nature." An English author (whose writings we have so often quoted of late, when some of the most interesting and difficult questions relating to medicine have come before us,) had previously remarked, in his "View of the Relations of the Nervous System," that general conclusions are made "by none more frequently than by those who profess to hold theory in contempt, but who, in fact, practise more upon theory than any others, and in general upon that of the very worst kind,—namely, that founded upon the loosest analogies; upon that made with no care, with no trouble; upon that which they would be offended to hear called theory: unaware of the processes of their own minds, they fancy that inferences are visible things; they are perpetually meeting with solitary facts, and as often generalizing them, until they meet with opposite facts; and it is highly probable that they would generalize them too; or else they are at their *ne plus ultra*, and conclude it is all a dark business."

The first chapter in the work before us relates to "the Vital Properties." The author here exposes the errors of conduct of those who have regarded the results of the properties of organization as elementary existences independent of such organization, and who have talked of changes of the vital properties as if it were possible that alteration could take place in them without any change of the structure on which their existence depends. But his own notions on this subject are not free from much obscurity, and even positive contradictions. He says, "*For the physiologist who embraces at one view the whole series of organized beings, there exists no other vital properties than those which are inherent with all bodies endowed with life, at all the epochs of their existence, and which are absolutely indispensable for their preservation. In proportion as the organization becomes complicated, its acts become more numerous; but it is not because those properties are multiplied,—it is because the organs are more varied and execute more difficult functions.*" If the vital properties are regarded as the causes of organization, there must be a diversity in them consonant with the diversities of the organization produced, or effects must take place without causes. If they are considered as the effects of organization, we only shift the difficulty, as will be presently shown. It might be supposed that Mr. Bégin may regard the vital properties as the properties of a principle that is attached to organized bodies, on which they act as with machines variously constructed; but that such are not his notions, will appear by his definition of irritability. He says, "*Thus defined, the vital properties are reduced to one single power, or rather to one single fact, which I call IRRITABILITY, after Glisson and Gorter.*" (Here he converts his obscure inferences into a FACT.) Irritability he defines, "AN APTITUDE POSSESSED BY CERTAIN BODIES TO RECEIVE THE IMPRESSION

OF OTHER BODIES WHICH ARE FOREIGN TO THEM, AND TO MOVE THEMSELVES IN CONSEQUENCE OF THIS IMPRESSION."—"Irritability," he then immediately adds, "*is a quality, a condition, an inherent property, of all matter endowed with life: it depends on the organization of that matter; it arises, is developed, and disappears, with the organic texture: but its nature is as inconceivable as gravity, extent, and all the other physical properties of bodies.*"

According to this reasoning, it appears, then, that all the vital properties are reduced to irritability; are but irritability; that irritability is dependent on organization, and that it is identical, though the organization is various, and executes various functions. The only means, then, of accounting for this diversity of functions are, either diversities of the *mechanism* of the part, (the validity of which proposition Mr. Bégin does not admit,) or the existence of *some other properties*. These other properties are considered by Mr. Bégin as the results of the laws of the *living chemistry* (*chimie vivante*) of Broussais: and thus he satisfies himself with mere words, instead of ideas or precise inferences. As—and the proposition is admitted—the laws of this *living chemistry* are different from those of the chemistry of inorganic bodies, that in which the difference consists, and which is peculiar to life, must be either a peculiar entity which is a cause of life, and, being a cause, must therefore enter into life as one of its constituents, and consequently must become a property of life;—or it must be the effect of life, and, being an *effect proper to life*, it must have its proper or peculiar causes; which peculiar causes (in spite of an abuse of terms,) must be properties of life, or *vital properties*. Thus, by this mode of reasoning, a diversity of vital properties is demonstrated.

It is true that Mr. Bégin assumes a distinction between *functions* and the mere development of vitality, and he simplifies the idea of life so far as to make it consist in "an aptitude possessed by certain bodies to receive the impressions of other bodies which are foreign to them, and to move themselves in consequence of this impression." But he cannot, by this assumption, get out of the dilemma in which we have shown him to be placed. This simplification of life is an untenable assumption: it is a gross absurdity. How can an *aptitude to move on the receipt of certain impressions*, combined with any mechanism, account for any one *proper vital act*? Besides this, his distinction of *functions* and the *simple act of vitality*, supposes effects to exist of which there are not the causes. For, as the diversities of functions are not explicable either by diversities of *mechanism*, or by diversities of *vital properties*, and (as it has been above shown) his *living chemistry* being referable to *vital properties*, there remains nothing that can be the causes of those diversities of functions: therefore, according to his own reasonings, *they must originate from nothing*.

That Mr. Bégin should have got into such a dilemma, by attempting to reduce the vital properties to *one single principle*, does not much surprise us. These, however, are but uninteresting topics to most readers; and we should not have dwelt so long on this part of the

work, had it not been the production of such a physiologist as one of the writers of the article "*Irritabilité*," in the *Dictionnaire des Sciences Medicales*.

The second chapter of the work before us, is on "the Varieties of the Animal Organization;" a title somewhat too comprehensive, as the disquisition relates only to what have ordinarily been termed diversities of *temperament*, and *idiosyncracies*. Mr. Bégin employs the latter term in a new sense, and he makes a distinction between temperaments and idiosyncrasies, that he has, perhaps, derived from BORDEU. He proposes, besides marking the temperaments of individuals,—which should be characterized by the relative prevalence of the *sanguineous*, *nervous*, or *lymphatic* systems, throughout the body generally; that we should designate the organs or functions which predominate in the economy, and regard such a predominance as designative of the *idiosyncracies*. Thus, a person may be said to have a sanguineous temperament and pulmonary idiosyncrasy; or a nervous temperament and an hepatic idiosyncrasy; or a lymphatic temperament and a mucous (from a predominance of activity or irritability of the mucous membranes) idiosyncrasy; or one of the above-mentioned temperaments with fibro-articular idiosyncrasy, (in which the ligaments of the joints are especially disposed to disease; and which idiosyncrasy constitutes, according to Mr. Bégin, what has been called the *gouty diathesis*;) and so with the other combinations of the *three* (above-enumerated) general, and the more-numerous particular, varieties of the organization in individuals. There seems to be nothing objectionable in this proposition, whilst it is qualified to favour conciseness in the narration of particular histories of disease.

The importance of the influence of peculiarities of temperament on particular cases of disease, has not, perhaps, been sufficiently considered of late, by the generality of English practitioners. DARWIN probably carried this doctrine too far, as well as his sympathetic theory; and hence, very likely, the improper discredit into which they have both fallen: for the abuse of principles, however good, by excess in their application, very commonly leads to an unmerited degree of neglect of them. The fate of Darwin and his medical writings has, however, been somewhat extraordinary. He obtained an overwhelming reputation during his life,* and he is already almost forgotten; and his works have contributed much to the unjust odium of medical theory that is now prevalent, whilst they present many of the most admirable examples of its utility.

Mr. Bégin says, "the doctrine of the temperaments is one of the most important parts of medical theory. It is the foundation of the most valuable indications for medical practice. Men differ so much according to the diversities of their constitutions, that the otherwise

* And this not merely with the public, but with the profession also; many proofs of which might be adduced. As one instance, we may mention that the physician who, next to Darwin, had obtained the greatest celebrity in the West of England, carried his daughter to Derby to place her under Darwin's care, with a confident expectation that he could cure her, though she was in the last stage of tubercular consumption.

best-narrated histories of disease would be incomplete, and barren to our contemporaries and to posterity, if we neglected to designate the peculiar organization of the subjects of them, before we described the causes, the symptoms, and the termination, of the maladies to which they relate."

Amongst the temperaments and idiosyncracies, some are born with us,—at least, the predispositions to them are transmitted by generation; others are produced either by the revolutions which ordinarily take place at different epochs of life, or result from certain modes of living, climate, &c. To overthrow an unfavourable temperament, and correct a vicious idiosyncrasy, and replace them by more advantageous conditions, are the objects of physical education and medical gymnastics, rather than of the ordinary practice of medicine; and are, therefore, not considered by Mr. Bégin in this work. He directs his attention to the exposition of the consequences of the particular conditions above alluded to; when they actually exist, and endeavours to draw some practical indications from his views. The sanguineous temperament, he says, always accompanies the muscular idiosyncrasy, and it is commonly accompanied with gastric and genital idiosyncracies. Inflammation, in the subjects of it, but rarely passes into the chronic state, or affects secondarily the *white vessels*.* The heart is particularly liable to be affected by sympathy, and it is one of the conditions in which the most precise indications may be drawn from the state of the pulse; the most important of which are passed in review by Mr. Bégin. He dwells especially on the important fact, that a very high degree of excitement of the capillary vessels and pain, instead of increasing the action of the heart, "chain" the motion of that organ, which then becomes tumultuously agitated, and dilates with difficulty: in which state, blood-letting, abstinence from food, and cold drink, serve to develop its movements; a result which is frequently found to take place in a very sudden manner, after the reduction of a strangulated hernia. On discussing the question, "What is the cause of the fullness and largeness of the pulse observed in certain cases of irritation; whilst others render it small, hard, contracted, and almost convulsive?" he says, "when the inflamed parts are soft, spongy, easily dilatable, and not abundantly supplied with nerves, the pulse is full, developed, the artery pulsates forcibly, and raises the finger applied to it: such are the pulsations which characterize inflammation of the laminous tissue of the lungs, the liver, the spleen, and all the parenchymatous organs." The occasional deviations from this condition have not escaped his attention, and are afterwards considered. Broussais supposes that the larger congeries of capillary vessels, when irritated, establish an obstacle to the circulation which renders the pulse full and hard. This explanation is opposed by Mr. Bégin; because we find such a pulse when no particular obstacle to the circulation can be supposed to exist, as after vigorous exercise, and in one arm alone when this member is exclusively exerted; and because such

* See the exposition of the Doctrine of Broussais in this Journal for January 1819, for an explanation of this expression.

a pulse is present with slight degrees of inflammation of the mucous membrane of the stomach and small intestines, when no considerable obstacle can be supposed to exist; whilst the pulse becomes small, hard, and contracted, when the inflammation has acquired a greater degree of extent and violence. Mr. Bégin thinks that the diversities of the character of the pulse should be attributed to relative diversities of sympathy with the organs affected, from which they arise; and this explanation is not at all inconsistent with the facts that a full soft pulse ordinarily accompanies inflammation of loose parenchymatous structures, and a small hard pulse that of firm extended membranes. In the former case, there is but little pain, and the capillary vessels are freely dilated; circumstances which sympathetically affect the heart, so as to excite and develop its movements. In the latter, there is severe and often intense pain; the free dilatation of the capillaries is prevented, and the heart and arteries are affected so that the dilatation and contraction of the former are impeded by the intensity of the suffering; and the latter are contracted, by a sort of erethism, on the column of blood which traverses them. Every part, Mr. Bégin further argues, has its peculiar sympathetic relations with the heart; and hence the connection of particular states of the pulse with diseases of particular organs, and even particular forms of structure. Hence, different kinds of pulse accompany inflammation of the mucous, muscular, and serous, tunics of the intestines.

On treating of the nervous temperament, the author digresses somewhat from his proper course, and advances some suppositions respecting the cause of the first inspiration of the infant, and the feelings which lead him to take food. He attributes the former to the uneasy sensation excited in the lungs by the presence of blood in them that has not undergone the modification it had received in the placenta; and he thinks that spirituous frictions on the skin only tend to establish the act by rousing the brain, and rendering it sensible to the call made on it by the lungs. He exercises his imagination in a similar manner, to account for the instincts and passions of animals. We are all acquainted with Darwin's attempts in this way, especially with his reasonings on the pleasure with which men regard the breast of a pretty young woman; to which, however, there happens to be this objection, that a man who had been *brought up by hand* has no such sensations on looking at a *spoon*.

Mr. Bégin discusses, also, in this section, the nature of consciousness and of delirium; and then treats of the influence of the nervous system on the rest of the body in the ordinary actions of life. In this part of the work there are several erroneous indications, or, at least, several assumptions which are not at all well established. He says, the tendons, ligaments, cartilages, and bones, are not penetrated by nerves;* and he adds, that "the free communication of the nervous

* This statement is so much in opposition to the most probable inferences from known facts,—as the occurrence of pain in ligaments, &c.—that we think it prudent to adduce Mr. Bégin's own words on this occasion: he says, "Les parties qui sont passives dans l'économie, et qui ne sont utiles qu'à raison de la résistance de leur texture, ne sont pas pénétrées par les nerfs; tels sont les tendons, les ligaments, les cartilages, les os."

extremities with the cerebral centre, is not indispensable to the nutrition of the organs in which these extremities are expanded. The *analogical proof* (this is Mr. Bégin's expression) of this fact, is furnished by animals entirely devoid of nerves, and by the tissues which in others want them, the nutrition of which is complete and very active. The direct demonstration of it, consists in the continuance of the phenomena of nutrition after all the nerves of a part have been destroyed, or after they have ceased to perform their functions, as is seen in some cases of paralysis of the limbs with total loss of sensation." Whether nervous influence is necessary for nutrition in animals supplied with nerves, is a matter of doubt; but we are surprised to find Mr. Bégin neglect some of the plainest points of anatomical demonstration, and talk of the continuance of nutrition in a part after the total destruction of its nerves. It is not possible wholly to destroy the nerves of a part, or even to interrupt their communication with their respective centres, without making a similar destruction of the arteries; for the ganglionic nerves are so intimately blended with the coats of the arteries, that it is impossible to divide them and leave the vessels continuous. The fact, too, from which Mr. Bégin infers a proof of nutrition without nervous influence, only warrants an inference that the nerves serving for sense and motion are not necessary for nutrition. The notion that this function is subservient to the ganglionic nerves, is not at all opposed by it.

Before a man attempts to frame a theory, he should be well acquainted with the grounds of human knowledge, or he may possess a high degree of intellectual power,—as a talent for acute and original observance, a great facility of comparison of ideas, and a very extensive range of abstract reflection,—and yet produce nothing that will bear the touchstone of truth, whilst he is himself unable to discern the difference between hypothesis and theory. We recommend Mr. Bégin to peruse a book which is, we have reason to believe, becoming pretty well known in Paris,—the "General Indications" of Mr. PRING; a work which is interesting to all who are engaged in the pursuit of natural science; to metaphysicians; and to moral philosophers; as it develops a system of Principles and Indications which cannot fail to promote the attainment of truth, and which must be particularly interesting to physicians, in consequence of the author having applied those Principles to the subjects of Physiology and Pathology in a particular manner, and hence determined, with a degree of precision that admits of no disputation, the extent of our real knowledge in those sciences; whilst his Indications—which are the production of a fertility of thought and originality of views as extraordinary as the solidity of the judgment by which his principles are established,—mark out new routes for the acquisition of knowledge that may occupy, for ages hence, the most active Spirits of our kind in their development.

Mr. Bégin seems to be unacquainted with some very important and well-known facts in physiology: after noticing the return of sensibility in a part which had been deprived of that faculty by a division of the nerve distributed to it, he says, "we cannot suppose that re-

union of the divided nerve takes place." He uses but very little reserve in drawing such inferences as suit his purposes; for he says that, even supposing that a re-union of the nerve did take place, "a multitude of facts lead us to believe that the necessary communication could not be effected through the cicatrix."

The rest of this section comprises nothing proper to Mr. Bégin, nor any opinions of Broussais that have not been already noticed in this Journal.

A considerable part of the section on the *lymphatic temperament*, is occupied in arguing against the opinion that the lymphatic system is in a state of relative debility in individuals of this temperament. Mr. Bégin infers, from its being especially developed, that its vitality is particularly developed; and hence, in conformity with a generally-received axiom in physiology, that it is particularly liable to disease. Mr. Bégin remarks, that, in subjects of this kind, not only is the lymphatic system particularly developed, but that this is the case also with all the tissues supplied only with colourless fluids, as the cellular tissue, &c.; a thing which must readily be preconceived, if what is very probable be admitted, that the whole of the soft structures of the body are constituted merely of congeries of vessels containing either red or white fluids.

One of the most common and most remarkable of the coincidences, of a morbid kind, with the lymphatic temperament, is a want of due solidity, and even, in some instances, a softening of the bones. Mr. Bégin only ventures to give some hints respecting the etiology of this affection, by remarking the well-known fact, that, whenever a spot of ossification appears, there are vessels containing red blood running to that point; and he infers that red blood is necessary for the constitution of bone. But, as animals which are devoid of red blood have bones as firm as those which have such a fluid, (a fact which Mr. Bégin denies,) this inference is not very probable, though it does not want the support of several facts which seem to indicate, in a slight degree, such a notion: thus, ligaments and periosteum ossify on being kept in a state of inflammation for a certain length of time.

The same causes, the author observes, that excite intense inflammation in sanguineous subjects, and in those of the nervous temperament what are called nervous diseases, give rise to hardly sensible inflammation in persons of the lymphatic temperament, and the sympathetic phenomena of such inflammations are but feebly developed. Hence, in the first, the same external causes will produce a degree of inflammation of the mucous membrane of the small intestines that will give rise to fever; and in the last only a mucous diarrhœa, without fever. In these individuals, the lymphatic vessels have the greatest tendency to become the especial seat of irritation; and hence arise the lesions of structure described on a former occasion* in this Journal.

The next chapter is on the *idiosyncracies*; but nothing is adduced here but such obvious inferences from what has already been stated in this article, as render any detailed extracts unnecessary. There are,

* See *London Medical and Physical Journal*, January 1820.

however, some further remarks respecting the temperaments, that indicate a multitude of truths that may not be so readily discernible by every reader. Mr. Bégin says, "the temperaments above enumerated are remarkable in this, that any one portion of either of the three several systems, to the development of which they relate, cannot be irritated without all the parts of the same system being, for that reason, more disposed to contract that same irritation. An intimate sympathy unites with each other each of the vascular and nervous extremities of the body; and they cannot be much affected in one organ without being similarly affected in every other organ. Thus, when a sanguineous subject has inflammation in any part, the whole circulatory system is deranged, and the disposition to take on inflammation is increased in every organ. If several tumefactions take place in the lymphatic system in a person in whom this system is particularly developed, the lymphatic glands throughout the body will thence be particularly disposed to irritation; and what are called nervous diseases in any organ, in nervous subjects, increase the general susceptibility of the nerves, and they become affected with diseases similar to the one primarily established more readily than ordinarily. It may, then, be inferred, as a general law, that the appearance and renewal of inflammations, of *neuroses*, and of lymphatic tumors, communicate a new activity to the system especially affected in those diseases respectively, and augment the predominance of the sanguine, nervous, and lymphatic, temperaments. The history of morbid diatheses constantly furnishes proofs of the truth of this assertion; and it is in conformity with this law of the economy, that lesions which are primitively local become *repeated* in diverse parts of the body."

The third chapter treats on the *mucous membranes*. These structures constitute a very important series of organs in the pathology of Broussais; and, as it is through them and the skin that all extraneous matters must obtain admission into the system, there can be no hesitation in allowing them to perform a very essential part in the production of the greater proportion of diseases. They are, however, regarded in too exclusive a manner in the work before us; for, after noticing their offices, and giving some examples of their sympathetic relations, Mr. Bégin enters on the consideration of diseases in general, without treating of the laws which regulate the functions of the other structures of the body besides the sanguineous, nervous, and lymphatic systems, any further than in a very partial way, as the phenomena which they develop are brought forward in connection with those dependent on the properties of the mucous membranes. This, however, is a necessary consequence of the manner in which Mr. Bégin regards the structure under consideration. It is, he says, "the fundamental part of the living economy; all the others are in a manner modelled on it; their functions, and consequently their organic forms, vary according to the wants of which it is the source, and the more or less complete manner in which it prepares the nutritive substance." It is true that all the soft parts of the body are probably constituted of nothing more than developments of nerves, vessels, and the most simple animal fibre, which, perhaps, enters as a constituent into the

whole: but, whether or not this be the case, it seems certain that each of what Bichat chose to regard as the elementary tissues of the body, as the cellular, serous, and fibrous, besides those already enumerated, has peculiar relations with other parts of the system in states of disease as well as in that of health; and the exposition of their laws should, consequently, form part of a General Treatise on Physiological Pathology. The sympathies, too, are not treated on in so complete a manner as we might have expected: some of those of the mucous membranes are stated in this chapter, and others are noticed, in a casual way, in subsequent parts of the work. It would, we think, have been better to have adduced what is known respecting their laws in one distinct section; and thus the particular illustrations of them brought forward in the consideration of particular diseases, would have been more perspicuous. BARTHEZ, the late Professor of Medicine at Montpellier, took a very lucid view of this subject; and the inferences he drew from it present many very important indications for the treatment of disease. We shall deviate a little more than usual from our analytical course, in order to give an exposition of the view of Barthez, which, we suspect, will be new, as well as interesting, to a considerable proportion of our readers.

As all the parts of the animal economy have a more or less intimate degree of vital relation with each other, it is necessary to mention, in the first instance, the sense in which the term sympathy is here employed: it is considered, then, that *a sympathy takes place between two organs when an affection of one sensibly and frequently occasions a correspondent affection of the other, without this succession being, with probability, imputable to chance, to the mechanism of the organs, or to their concurrence of action in a generic mode of function or affection of the living body.* This definition excludes from the class of sympathetic actions several which WHYTT, and subsequent writers, have generally confounded with them, and which are termed, by Barthez, *synergies*. Thus, action of the abdominal muscles accompanies the contraction of the bladder in the expulsion of the urine; and this connection has usually been called sympathetic, but it is *synergetic*. There is a concurrence of action in the abdominal muscles and the bladder, having a common and especial object, which is a natural function of the body,—the expulsion of the urine. A similar concurrence of active power exists between the abdominal muscles and the large intestines; between the same muscles and the glottis, exerted in coughing, certain efforts, &c. A similar synergy (*co-operation*), for the fulfilment of an especial function, sometimes exists in disease; and hence some morbid actions may be classed amongst the synergies. This distinction of synergies and sympathies, is not a mere scholastic prolixion: it is qualified to establish some important practical indications; for, as the secondary affections of the former class have a specific tendency to the fulfilment of some object, it is obvious that no attempts should be made to remove them when the accomplishment of such an object is desirable: whilst those of the latter class, being merely an extension of disease, and having no such specific tendency, may commonly be removed with advantage; though they sometimes

are connected—and probably as *causes*—with a disappearance of the primary affection; and when this happens, and the secondary affection occurs in a part the health of which is of less importance to the welfare of the patient than that of the primary, it may not be advisable to attempt their removal, at least not without some precautionary measures; as the primary affection sometimes recurs on the disappearance of the secondary, and, apparently, as a related consequence of its disappearance.

The sympathies are arranged by Barthez in the following orders: 1°, *those of organs having no particular sensible or material connection*; 2°, *those of organs which resemble each other in their structure and functions*; 3°, *those of organs having particular connections*,—that is to say, connections formed between two or more organs by an intermediate texture, as vessels and nerves common to them; or of a membranous or muscular structure which unites and renders continuous several adjacent organs; and those of organs assembled together in one system, which differs in its structure or functions from the other parts of the body.

The most remarkable of the sympathies of the first order, is that of the organs of voice, the neck, and the thyroid gland, with the genitals. The change which takes place in the *timbre* of the voice at the epoch of puberty, and which does not happen in eunuchs, is generally well known. The diapason of it commonly falls an octave at this period; and hence many who, when boys, were qualified to sing the *contr'alto* in concert, are calculated only for the *bass* after they have arrived at puberty. The neck of women, and especially the thyroid gland, enlarges much and rapidly at the same epoch, particularly after the indulgence of the venereal passion; and hence the observation in the verses of Catullus:

Non illam natrix orienti luce revisens,
Hesterno collum poterit circumdare filo:

and the experiments commonly resorted to by the people in Italy, to ascertain whether or not, as they say, a girl is a virgin.

A similar sympathy is observed in some diseases, as the *mumps*, and perhaps in some ulcerative affections of the genitals.

Other sympathies of this order are often manifested in disease of the liver consequent on some injury of the head: pain concentrated in a spot in the head in some affections of the uterus; but the most frequent of this kind are those of the stomach with several remote parts, which were treated on and illustrated by Dr. RICHARD HARRISON, in one of his Gulstonian Lectures, inserted in a late Number of this Journal: next to these, are those of the intestines; instances of which are so common and well known, that the production of examples is unnecessary.

The sympathies of the second order are, also, very frequently witnessed; as in the general occurrence of inflammation in one eye when it has been established in the other: and RICHTER mentions having seen amaurosis of an eye, previously perfectly healthy, succeed to the same affection in the other, when it happened in this as a consequence of external injury. The sympathetic contraction and dilatation of the

pupils in the healthy state, is generally known. MORGAGNI relates the case of a child, twelve years of age, who had suffered convulsions in several parts of the body, which, at length, became confined to one of the hands, the fingers of which were contracted. When an attempt was made to extend these fingers, the otherwise healthy hand was instantly affected with convulsions, and became violently contracted. If only a single finger of the diseased hand was extended, the corresponding finger of the other hand was affected with convulsions, which continued as long as the attempt at extension was continued. THEDEN (in his *Neue Bemerkungen und Erfahrungen*, u. w. s.) relates the following, somewhat analogous, story. A vesicatory was applied to the right arm of a woman that was paralytic. The plaster produced no apparent effect in the part to which it was applied, but it excited redness and severe pain in the corresponding part of the other arm, during the whole time it remained on the paralytic one. The paralysis, however, now disappeared from the right arm, and affected the left. A vesicatory was then applied to this extremity, the action of which was manifested on the right, causing redness and pain, as on the former occasion. On the paralysis being cured, vesicatories were not found to be attended with any such particular effects. The character and talents of Theden are such as to claim credit for this story.

FRANK (a son of the Professor at Vienna) mentions having seen the case of a woman who had paralysis of one of the arms, and who determined to use efforts to move this arm at the same time that she moved the healthy one: she soon, apparently by this means, recovered the power of motion in it. Frank says that he has tried the same practice with other patients, and that it has sometimes been attended with success.

It is remarkable, that when one of the symmetrical members has attained, by habit, increased facility in the execution of certain movements, the corresponding member has acquired a similar facility in the execution of movements in the contrary direction, and not in the same direction. No plausible explanation has hitherto been given of this curious fact.

Other sympathies of this order are frequently manifested between parts which, without being symmetrically placed in the two lateral divisions of the body, have a similarity of structure and function. These are commonly witnessed in the skin, and in the fibrous and mucous membranes. FANTON mentions the case of a man having acute fever, in whom the portions of the skin on which there had recently been blisters, but which were quite healed, became again inflamed, and suppurated, when other and distant parts had blisters produced on them. The occurrence of inflammation of fibrous membranes in different parts of the body when it has been produced in one part, even by causes acting merely locally, as mechanical violence, is a very frequent phenomenon. WILLIS mentions having seen enlargement of the lymphatic glands in the neck occur, in consequence, he thought, of very great compression of the inguinal glands by a truss. Cases similar to this are related by BARTHOLIN and LE DRAN. The disordered state of the mucous membrane of the tongue which attends

disease of that of the stomach, is another instance of sympathy of this kind.

When several successive alternations of affections have been established between two organs, naturally not particularly sympathizing, by any accidental causes, those organs contract a habit of correspondence of such affections.

The sympathies of the third order are very numerous; we shall merely give an example of each species of them, by way of illustration. The most remarkable instances of those dependent on particular nervous and vascular connection, occur in the abdominal and thoracic viscera, as between the liver and the stomach and small intestines; the stomach and the heart, &c.; which seem to be explicable by the common origin and close alliance of the nerves of those organs. Those from the continuity of membranes, in vomiting produced by irritation of the fauces; in the pain at the extremity of the urethra from stone in the bladder; itching of the nose from worms in the intestines; various affections of the skin from disorder of the mucous membranes. The progressive extension of diseases of the skin, as well as of mucous and serous membranes, seems often to be dependant on the same laws. There seems, however, to be a sympathetic connection between the skin and some internal parts, that is not, apparently, accountable for on this basis: thus, titillation about the ribs causes violent action of the diaphragm, which is not produced by titillation in other parts; and Barthez says, that an ointment in use in France, called *unguent d'arthanatu*, (composed of violent purgatives,) produces vomiting when it is applied to the skin about the region of the stomach; purges when applied about the navel; and causes a great flow of urine when rubbed on the loins.

The sympathies of the whole of the digestive organs, and of the system of urinary organs, are examples of the species last enumerated. It did not, however, constitute any part of our intentions to enter into many details on this subject. We have adduced examples sufficient to show how the whole of the phenomena of this kind may be arranged in conformity with the views of Barthez, and we shall have occasion hereafter to notice many others of the most interesting kind to the medical practitioner, as we proceed in our analysis of the work of Mr. Bégin, who notices several which will, probably, be novel to many English readers, although this subject has been treated on by WHITT, DARWIN, and Dr. ANDREW WILSON, in an extensive manner.

We should remark here, that the foregoing view of Barthez is not adduced as one that is free from obscurities. Some of the phenomena regarded as sympathetic, may be really synergetic, according to the definitions of those terms given above: the alterations that take place in the neck and voice at the epoch of puberty, may tend, with the development of the organs of generation, to the fulfilment of some common object; though we cannot discern what that object is, nor discover the train of relations to it. A person, by exercising his imagination, might conceive that the change to the grave and sonorous voice in man, is only in conformity with other of his distinctive

characteristics from woman : but why there should be so much greater a development of the cellular texture about the neck than in other parts of the body in woman, is not so obvious. It is not enough to say that it contributes to what is called beauty, because what is morally beautiful is generally found to tend also to some relative physical good ; and it is, probably, because certain objects tend to such good, that they are regarded as beautiful.

We return from this long digression to the work of Mr. Bégin, and resume our analysis of it where he treats of the sympathetic relations of the mucous membranes. Mr. Bégin makes no distinction of sympathies and synergies, and therefore comprises under the former appellation many phenomena which relate to the latter. We shall, if we produce in our course any observations of this kind, be content to use the terms employed by the author, and leave those readers to make the distinction above mentioned that think it of sufficient importance to engage their attention.

Mr. Bégin first notices the sympathetic relations of several parts of the mucous membranes and the brain, by which painful impressions originating from the mucous membrane of the fauces when there is want of drink, from that of the stomach when there is want of food, and from that of the lungs when there is want of air, are perceived by the sensorium, and by this medium determine the actions proper for their removal. What has been called spasmodic asthma, he says, arises from irritation of the mucous membrane of the lungs, which produces a spasmodic contraction of the glottis, and, by this means, the great difficulty of inspiration which attends that disease. The organic lesions of the lungs, heart, and pulmonary arteries, connected with it, and found after death, are, Mr. Bégin adds, consequences, not causes, of the asthma. This etiology, though it is but hypothetical, has much appearance of probability.

One of the most remarkable of the sympathetic effects of irritation of the mucous membranes of the stomach, cesophagus, and pharynx, is the production of the act of vomiting. Mr. Bégin argues that the membrane of the pharynx and cesophagus has the sympathetic relation with the abdominal muscles necessary for the production of the act of vomiting independent of the stomach, because the proper actions of those muscles are excited by irritating those portions of membrane in an animal whose stomach has been removed, and when the eighth pair of nerves has been divided on each side ; which division, he remarks, is followed by paralysis of the stomach. Mr. Bégin thinks that the only sensible action the stomach is capable of, is a peristaltic motion similar to that of the intestines ; and that the course of this action is reversed in vomiting, as well as in what may be termed *regurgitation* : (to distinguish it from vomiting properly, which is attended with action of the abdominal muscles, which does not accompany *regurgitation*, as we may readily observe in children, in whom this act is very common.) What has been termed *elective vomiting*, (in which substances not fit for digestion are thrown from the stomach, whilst such as are adapted for that process are retained,) seems to be only a sort of *regurgitation* ;

the stomach being excited to anti-peristaltic action only just in those parts where some improper and irritating substances are present.

The sympathy of the mucous membrane of the stomach and intestines with the skin, is next noticed by Mr. Bégin. "At the instant," he says, "when irritation is developed, and when congestion is established, in the mucous membrane of the parts above mentioned, the cutaneous surface becomes cold, a general shivering affects the patient, the vital actions seem hardly to extend to the external parts of the body: such is the onset of the greater part of idiopathic fevers, which are only *phlogoses* of those membranes. But this state of debility and torpor is soon dissipated: the irritated organ re-acts on the external parts; the vital actions are then roused in them, and are soon raised above their habitual standard; the skin becomes burning; it is dried by an arid heat or covered with an abundant sweat, according as the organs of digestion or of respiration are especially affected."—"The sympathies of the mucous membranes," he adds, "are more pronounced with the portion of the skin which corresponds with them, than with the other parts of the surface of the body. We observe that the heat is greater in the throat, the neck, the thorax, the epigastrium, the umbilical organ, the hypogastrium, according as the membranes which line the pharynx, the larynx, the trachea, the bronchiæ, the stomach, the intestines, the bladder, and the uterine, are the exclusive seat of irritation." These extracts develop the principal points of some views that were more fully exposed in the Exposition of the Doctrine of Broussais, inserted in some late Numbers of this Journal, and in the first of the Gulstonian Lectures of Dr. Harrison. The rest of this section is occupied with some considerations on the sympathies of the mucous membranes with the nervous system; but nothing is adduced that had not been noticed under the heads "Physiology" in the Historical Sketches of the Progress of Medicine, lately inserted in this Journal.

The fourth chapter treats on *diseases in general*; but we have arrived so near the limits of the present Number, that we must defer an abstract of it till the next, when we shall complete the analysis of this work.

Medical and Physical Intelligence.

TRANSACTIONS OF SCIENTIFIC SOCIETIES.

ROYAL SOCIETY of London.—Jan. 18. A PAPER by Dr. DAVY was read, giving an account of his inquiries relative to the urinary organs, and secretion of two species of common rana in Ceylon: from which it appears, first, that the bladder of the bull-frog and brown-toad (the two species in question) is a genuine receptacle of urine, which it receives from the cloaca, in which the ureters terminate; and secondly, that their urine is not at all analogous to that of other animals of the order amphibia, being very dilute, containing

urea and certain salts, but no appreciable quantity of lithic acid. This peculiarity of urine, so well adapted to the size and structure of the bladder, is the more remarkable, as the favourite food of these animals is the same as that of small lizards, whose urine is of a butyraceous consistence, and nearly pure lithic acid. Hence, and from other facts mentioned by the author, he adduces the conclusion that the nature of urine, in every instance, depends much more on the peculiar action and structure of the secreting organs, than on peculiarities of diet or of the circulating fluids.

Dr. FOUQUIER, of Paris, to whom we owe the introduction of *nuxvomica* into use, has lately been trying the efficacy of another powerful remedy—the *acetate of lead*, when given in larger doses than those in which it had hitherto been administered. “When a physician must renounce all hope of curing his patients,” says Dr. Fouquier, “there often yet remains for him this consolation, that of alleviating their sufferings and prolonging their existence.” The profuse sweats in phthisis contribute much to exhaust the patients of that disease, and hasten the fatal termination, especially when accompanied with colliquative diarrhœa. Dr. Fouquier thinks that he has found in the acetate of lead a mean of repressing this debilitating evacuation. He first employed that medicine in the solid form, in pills of a grain each; but he has since found it more convenient to administer it in solution, in a mucilaginous mixture. He relates the histories of twelve cases, arrived at the last stages, in which this medicine was used, in one to the extent of twelve grains as a dose, without finding after death, in the patients who died in the hospital, any affection of the intestines which might be attributed to the mineral. In one or two individuals only were some slight colic pains manifested; and these did not appear to be owing to the medicine, as they were accompanied with diarrhœa, and did not vary whether or not the use of it was continued. In a few cases there was constipation, for the removal of which glysters became necessary.

WE give following abstracts of two cases, related with several others of more or less interest, by Dr. VENTURI, first physician *della città di Sanseverino*, that seem to us to present some very interesting facts for the consideration of theoretical pathologists, as well as medical practitioners.

A girl, twenty years of age, was, in 1812, suddenly affected with very severe rheumatic pain in the upper and middle part of the back. A physician prescribed a spirituous embrocation. When this had been used two or three times, the pain disappeared from its first seat, and fixed just about the union of the last dorsal and first lumbar vertebrae. The use of the embrocation was continued; but the pain soon became almost insupportable, and seemed to be deeper seated. After a few days' very severe suffering, the patient began to experience a sense of formication in the lower limbs, that was soon followed by a sense of weight, and then, gradually, complete loss of sense and power of motion in them. Dr. Venturi saw her first after the lapse

of four months from the occurrence of the paraplegia. The patient had then a *chlorotic* aspect, and had not menstruated for four months; the urine and feces passed unconsciously and involuntarily; and the lower limbs were flaccid, emaciated, insensible, and motionless. A dull and deep pain remained in the part of the spine above designated. The pulse became accelerated, and presented a vibrating character, towards the evening, when there was an increase of heat over the surface of the body generally, excepting in the paralytic limbs; the temperature of which was always below the natural standard. A gentle sweat came on towards morning, with which the fever disappeared. There was a periodical discharge of a large quantity of mucus from the uterus, accompanied with a sense of heat in the part, as if the uterus were affected with a sort of catarrh. Dr. Venturi commenced the cure by the abstraction of about ten ounces of blood from the back, in the region of the part affected, by means of leeches, and the administration of three grains of kermes mineral, in six doses, daily, which were accompanied by a draught of infusion of arnica-flowers. After three days, the state of the patient having suffered no change, the dose of the kermes mineral was doubled, and two scruples of mercurial ointment were rubbed along the spine. This friction was repeated nineteen times, in thirty-four days. The warm bath was also employed. After the fifth friction with the ointment, the patient became conscious of the passing of her urine and feces, and in two or three days more the evacuation of them was entirely under the dominion of the will. After the ninth friction, the patient felt pain about the great toe of the right foot, like that from a recent blister; this sort of pain then extended over the foot, and was soon followed by a return of sensibility to external impressions over the whole limb. Two days afterwards, the return of sensibility occurred, in a similar way, in the left extremity; and in a few days more, the power of motion in both of them. The mercurial inunction was now omitted. Some occasional painful spasmodic contractions of the affected limbs now occurred, which were the only inconveniences experienced by the patient. These soon subsided, under the use of the warm bath and the infusion of arnica. It was thought proper to treat the patient for another month with steel medicines and infusion of gentian; at the end of which time not the least vestige of the disease remained.

. The other case above alluded to, was one of *amaurosis*.

A youth, thirteen years of age, of a slender habit of body, had been subject, from time to time, from his infancy, to ophthalmia. In the year 1813, he had a new attack, but slight to external appearance, and confined to the right eye, but accompanied with deep and very acute pain, which ceased during the whole of the night, but returned towards morning, and continued without intermission till the evening. During the paroxysms of pain, the affected eye appeared unusually injected with blood, the tears flowed copiously; the temporal artery on that side beat with violence, and the corresponding venous ramifications were tumid. At first there was intolerance of light, but afterwards the eye became more and more insensible in this respect; and on the lapse of about two months the sight of the affected

eye was entirely lost, the pain having ceased, and no other morbid appearance of the eye being manifest than a little inordinate dilatation of the pupil. At this time the same inflammatory symptoms occurred in the other eye, and it was feared that they would be followed by similar results, and, consequently, that total blindness would ensue; the stimulus of light producing at length but a feeble impression on the eye secondarily affected. Such was the state of the patient when he was first seen by Dr. Venturi, which was five months from the commencement of the disease. Many and opposite kinds of medical measures had been employed; as purgatives, sarsaparilla, china-root, valerian, ether, musk, camphor, opium, various collyria, vesicatories to the arms, and a seton in the neck.

Dr. Venturi remarked a great disposition to morbid excitement about the head: a slight degree of bodily exercise was sufficient to produce great heat in it, with copious sweating. He determined to treat the case by sedatives. Ten leeches were applied about the temple and external angle of the left eye. During the paroxysm of pain, a pledget wetted with iced water, frequently renewed, was kept on the eye. A spare diet was ordered; and the internal administration of oxide of zinc and extract of cicuta, in quantities of four grains of the former and eight of the latter daily. The quantities were gradually increased to fifteen grains of the zinc and twenty of the cicuta: but then, after a little time, vomiting being excited, they were reduced to the original extent. The paroxysms of pain soon ceased, and left the eye last affected in the natural state. At this time a favourable change took place in the right eye; the sense of sight began to re-appear, and it was after two months perfectly re-established. The patient presented for some time the curious phenomenon of a transudation of blood in the spot occupied by the cicatrix of the seton, whenever the excitement in the head, above mentioned, took place.

We have received the following account of two interesting operations from Mr. RICHARD WALKER, of Oxford.

The modern operation for the cure of aneurism by the application of a single ligature, has been very adroitly performed here lately in two instances: one, in which the external iliac artery was tied for an aneurism of the femoral artery, toward the upper part of the thigh; and another, in which the femoral artery was tied for the popliteal aneurism.

The first subject, being an old man of sixty, in a very weak emaciated state, promised but small hopes of success: his life, however, I may venture to say, has been preserved by the operation, the aneurism being in a very advanced state; but the circulation was too languid to preserve the leg, which was amputated below the knee; a direct line of separation between the living and dead parts having there taken place.

The latter instance, being in a healthy young man, succeeded completely, without the occurrence of the least untoward circumstance. The ligature became perfectly detached and was taken away on the twentieth day. The aneurismal tumor is much diminished, and the

patient enjoys the perfect use of his limb, which is free from sensations of numbness or coldness.

I mention these cases, occurring in a provincial situation, principally to inculcate additional confidence, if needful, in such operations, being well assured, from my knowledge of operations in general, that, in instances of the former kind, provided the subject be a tolerably fair one, the operation being judiciously performed, with due attention afterwards, there can be little doubt of complete success; neither mortification nor hemorrhage from ulceration being to be apprehended. In cases of the latter kind, under the before-mentioned favourable circumstances, scarcely any doubt of success.

It unfortunately happens that some persons are particularly predisposed naturally to aneurism: such is the case in the latter instance; there being, at the time of the operation, an aneurism, in a recent state, in the other ham: but this was justly considered no sufficient objection to the operation; which will probably be required, and some time hence may be performed, for this affection in the other ham.

N.B.—It is well known that, in the old way of operating for the popliteal aneurism, mortification and death were so commonly the consequence, that, latterly, amputation in the first instance was deemed the better alternative.

I have ventured thus promptly, as a professional spectator, to transmit the above account of these operations: it is not improbable that a more circumstantial account of the case in which the iliac artery was tied may, some time hence, appear from the operator himself.

A JOURNAL has just been established at Paris, by Dr. MAGENDIE, "*for Experimental Physiology.*" With such a title, however, the Number that has been published contains articles of a strictly anatomical nature, and some histories of cases of diseases, as well as papers relative to vegetable chemistry and pharmacy. There existed already an excellent Journal for Physiology, (the *Deutsch Archiv für die Physiologie* of Professor MECKEL,) which comprises papers collected from the literature of Europe in general, as well as original articles by the conductors; but that of Dr. Magendie is intended, it appears, to contain only original papers; and, as he has taken up the cultivation of this part of medical science with so much zeal, he may continue, as he has done in the Number that has been published, to supply his Journal to a great extent by the results of his own researches. There are several very good articles in the present Number; and, as it may be interesting to our readers to know with what auspices this Journal has made its first appearance, we shall give an abstract of the whole of its contents; but we have vacant space left in the present Number only for one of the shortest articles, which is that relating a novel experiment respecting *rabies* in the dog. After an account of the notions that have been entertained of the nature of this disease, Dr. Magendie expresses some doubts of the success of any attempts to cure it by ordinary means, for the following reasons. "The most active substances, the most powerful narcotics, have no action on

either man or animals affected with rabies." "I do not speak merely of substances introduced into the stomach, the action of which might be prevented or attenuated by so many circumstances," Dr. Magendie adds: "I speak of substances injected into the veins, the effects of which should be equally prompt and energetic. For example, I have several times introduced opium, in large quantities (ten grains), into the veins of rabid dogs, without producing the least narcotic influence; whilst a single grain of the aqueous extract passed into the veins of a dog in health, immediately produces sleep, which often continues for eight or ten hours. The same phenomena are observed in our own species. Mr. Dupuytren and myself have injected about eight grains of the watery extract of opium into the crural vein of a young man having hydrophobia, without any apparent result.* Rabid dogs have also supported the introduction of prussic acid in their veins, without experiencing an instant of relaxation in the progress of their disease.

"If substances as active as those above mentioned remain without effect, what hopes can reasonably be founded on vegetables which perhaps have no action on the animal economy, even in the state of health?"

These and other reflections led Dr. Magendie to attempt some new experiments, and to put in practice the following one. A large dog, of the mastiff kind, which had been decidedly mad at least two days, was muzzled and tied down on a table.

"I laid bare the jugular vein," says Dr. Magendie, "and commenced by taking blood from it to the extent of about a pound; after which I introduced, by means of a syringe, about sixty ounces of water, at the temperature of 40° centigrade, (105° Fahrenheit.) Towards the middle of this injection, the vessels were much distended; so, reckoning from this moment to the end of the injection, I suffered the blood to flow from the superior extremity of the vein, while I continued to introduce the water by the inferior. There escaped, in this manner, about ten or twelve ounces of blood mingled with water.

"The injection being concluded, I had the dog carried to his place, taking the greatest precautions; but these had become useless: the animal was calm; he laid down in a circle as it were to sleep, as soon as he was left to himself, which he had not done since the commencement of his disease. He no longer growled; his eye had become calm, and there was nothing menacing in his aspect: he only showed his teeth when attempts were made, by a long stick, to take away the straw on which he laid. I watched him for an hour; after which I went away, leaving one of my assistants to take charge of him.

"When I had terminated this experiment and had time to reflect respecting its consequences, some degree of regret accompanied the

* Dr. Breschet and myself inoculated a dog, under the skin of the forehead, with the saliva of this patient, and the animal became rabid at the end of about a month. Two dogs, who were bitten by this, also became mad after forty days. The latter bit several other dogs, but without any ill consequences.

pleasure I felt on seeing the symptoms of the disease so perfectly calmed: I feared that I had introduced too large a quantity of water into the veins. I knew that, if the injection had been carried too far, laceration of the pulmonary vessels would take place, and the animal would die from an infarction of the lungs, similar to that which so often causes the death of man. However, I was a little disposed to hope that the great size of the animal would permit his blood-vessels to contain, without bursting, so large a quantity of fluid. But it was not so: to my great regret, my assistant came, in the course of the day, to tell me that, in about five hours after the injection, the animal began to suffer great difficulty of respiration, which continued to increase for half an hour, when he died. To this time the dog had been very calm; he had not growled, and he had continued to lie as it were in a state of quiet sleep."

"The dog was dissected with the utmost care. No lesion of the brain, or of the spinal marrow, was found; the digestive organs were also in the healthy state, except that the salivary glands were red and appeared to be more voluminous than ordinary. The lungs were in a state of infarction; that is to say, their tissue was filled with aqueous blood: the bronchiæ and the trachea were filled with a brownish spume; their mucous membrane seemed to be inflamed."

SOME curious and very interesting observations have recently been published by Dr. KERNER, of Wurtemberg, respecting the probable existence of a species of animal poison not hitherto known. Dr. Kerner says that the smoked sausages, which are so favourite a food with the inhabitants of Wurtemberg, often cause fatal poisoning. The effects of the poison occasionally manifest themselves every spring, in the month of April, in a more or less alarming manner. He states, that of 76 persons who became sick from having eaten those sausages, 37 died in a short time, and several others remained ill for years. The sausages made with liver appear to be the most dangerous. He is about to publish a tract on this subject. At present he remarks, that the poison which is generated in raw, minced, and seasoned flesh, smoked after having been stuffed in membranes, &c. is different from most others in not affecting the brain and the spinal marrow; whilst its influence is exerted on the lymphatic system, and sometimes the patient does not feel his heart beat for several months, although the pulsation of the arteries is unaltered.

Mr. CADET, of Paris, says that he has been required by the police authorities, five or six times within the last fifteen years, to analyze meats bought at the sausage-shops at Paris. Those meats had proved poisonous, according to the statements of several physicians, and every means was taken by the legal authorities to discover the cause of these results. Mr. Cadet analysed all the meats; examined all the vessels in which they had been prepared; analysed the matters vomited, or found in the stomachs of the patients after death; without being able to find any trace of mineral poison by any of the tests in use. There was not an atom of copper, arsenic, or antimony; nor

any evidence of malevolence or negligence. The shopkeepers were never brought into a court of justice; but similar accidents have recurred from time to time at Paris. The police officers have visited the pig-dealers, and have been satisfied that the pigs were fed with nothing but wholesome substances; the use of poison for rats, with which animals the places abounded, was interdicted, and every precaution taken that could be suggested. "What then," adds Mr. Cadet, "is this poison which is found in sausage-meats? Is it prussic acid? Is it a new matter? It is not the result of decomposition, or at least of putrefaction, because it exists in meats perfectly well preserved."

To the Editor of the Medical and Physical Journal.

MY DEAR SIR,

You are aware of the circumstances attending the transmission to Mr. Brande of the annexed Reply to an Article contained in his Journal for January last, purporting to be a review of my work on Prussic Acid. The result of my application to that gentleman for the insertion of this Reply in the next Number of that Journal, will be best shown by the following laconic correspondence.

Saville-row, Wednesday evening, 7th February.

Dear Sir,

I send you a Reply to the Article contained in your last Number, purporting to be a review of my work on the Prussic Acid. Dr. Hutchinson, a particular friend of mine, has been kind enough to undertake to deliver it into your own hands, to prevent mistakes; and he is instructed to request you will have the goodness to name an hour in the evening of to-morrow, when he may call for your decision as to whether you will admit, or not, the said Reply for insertion in your next Number. In case of your declining to insert it (a circumstance which I am far from contemplating), I have requested my friend to bring back the MS. without any further comment.

I have the honour to be your humble servant,

A. B. GRANVILLE.

W. T. Brande, Esq. F.R.S.

Two days afterwards, Mr. Brande returned me the manuscript of my Reply, with a short note, of which the following is the beginning:

Thursday, (not delivered till Friday night.)

My dear Sir,

You must surely be quizzing me to suppose that I should insert the inclosed, &c.

I am always yours, faithfully,

W. T. BRANDE.

Dr. Granville, Saville-row.

How far, by the refusal of an act of justice and impartiality demanded of him, and by the language in which that refusal is conveyed, Mr. Brande has or has not identified himself with my reviewer, and thereby rendered himself obnoxious to all and each of the charges I

have brought forward, and *proved*, against the latter,—I leave you to determine. It is enough for me to observe, that Mr. Brande's conduct as the Editor of the Quarterly Journal, in this affair, is to me a matter of great astonishment; and that, as a member of the Royal Institution, I shall take the earliest opportunity of protesting, either at a general meeting, or to the board of managers, against that Society lending its name to a Journal, in which an attack, involving matters of personal consideration, is admitted; and the reply, showing the injustice and unfairness of that attack, rejected.

Under these circumstances, you will perhaps allow me the opportunity of giving publicity to my Reply through the medium of your Journal; and I remain, my dear Sir, yours, &c.

A. B. GRANVILLE.

Saville-row; Friday evening, 9th February.

A Reply to an Article inserted in the 20th Number of the Quarterly Journal of Science, edited at the Royal Institution; purporting to be a Review of Dr. Granville's Treatise on the internal Use of the Hydrocyanic Acid.

In a Letter to W. T. BRANDE, Esq. F. R. S. the Editor.

DEAR SIR,

You will readily remember, that, when you informed me of having received a "severe" critique on my work on Prussic Acid for insertion in your Journal,—of which, however, you assured me that no use would be made, if I no longer entertained the opinion I expressed respecting the acid prepared at Apothecaries' Hall,—I instantly replied, that you were welcome to admit and insert the article in question, if you thought proper; for, as my opinion of the acid prepared at the Hall, at the specific period at which I was writing, had been formed upon such ocular and experimental demonstration as would have warranted still stronger expressions of disapprobation on my part, I could not tamely surrender my humble judgment to the terror of any review of my book, however severe. The only concession I claimed in return was, that any reply that I might think it necessary to write to the article you mentioned should be equally honoured with insertion in your Journal; and to the justice of this claim you readily acceded.*

In order to bring these circumstances more particularly to your memory, it will be well to mention that the conversation above referred to took place on the evening of the 14th of December, 1820, at Somerset House, a short time before the meeting of the Royal Society; and that it was again repeated by you, in a more cursory, yet impressive, manner, immediately after the Society had adjourned, as we were both in the act of leaving the meeting-room.

Had the review been "severe," yet just, correct, and candid, I should have preserved a becoming silence, and endeavoured to profit by the admonitions bestowed on me, however harshly; but, as it possesses none of the latter qualities, I can scarcely be expected to suffer it to pass unnoticed.

When I state that the review is the reverse of being just, correct, and candid, I am advancing what, I trust, I shall fully prove to yourself and the public; and this circumstance induces me to declare, *in limine*, that although, by designating that review as "severe," you implicitly acknowledged that you had read it, yet, from my acquaintance with your character and urbanity of manners, I am free to assume that you had not paid any very particular attention to the structure of that article, and the assertions it comprises; or it would never have appeared in your Journal, to which I am proud in having been one of the earliest, and certainly not the least zealous, of its contributors.

My observations, therefore, can in no way whatever apply to you as the editor of that Journal, except where it is expressly so stated; but are directed to the writer, of whom the letter O stands as the representative.

To render my reply as perspicuous and as concise as the nature of the subject will admit, I shall, whenever I have an opportunity, adopt the form of *positive answer* to the assertions of the reviewer rather than argumentative discussion, for which I have neither the necessary talent nor any very particular inclination. No answer, or reply will be given to which I am not prepared to append a proof in support of its meaning; this being, in my opinion, the surest mode of conducting a defence.

The circumstances which induced the reviewer to notice my work are stated by him to be these:—
1. "The first part of it (the work) affects scientific arrangement; and the subject of which it treats was first brought before the British public in this Journal.

2. "We wish to point out an error or two into which the Doctor has fallen.

3. "And to advertise him of two or three samples of bad taste which have probably escaped his notice."

To which I then reply, *seriatim*,

REPLY 1.—The purely scientific part of the subject of Hydrocyanic Acid was not first brought before the British public in the Quarterly Journal of Science; neither was its application to the purposes of medicine first adverted to in that Journal.

Proofs.—A masterly account of the discoveries and investigations of Gay-Lussac on that subject was given in the Annals of Philosophy for December 1815, at which epoch the Quarterly Journal was not in existence! And a paper specifically written for the British public, respecting the use

* The reader will have seen, from Mr. Brande's reply to my letter given above, in what manner he has redeemed his implied pledge.

of Prussic Acid as a medicine, was inserted in the Medical Repository two years and a half before the subject was noticed in the Quarterly Journal.

Corollary.—O's first asseveration, therefore, is 'incorrect.'

REPLY II.—Of the one or two errors into which I am said to have fallen by the reviewer, *one only* is mentioned, after all, by him in the course of his critique, at page 402, respecting the specific gravity of prussic acid; and that is *not* an error of the author, but a typographical fault.

Proof.—See the *errata corrigé* prefixed to my work, in which that fault is actually rectified.

Corollary.—O's first accusation against me, therefore, is 'unjust.'

REPLY III.—Of the two or three samples of bad taste of which O was anxious to 'advertise me,' one only is brought forward by him in his review; and that one sample of bad taste is *only* made to appear as such by artfully coupling together two short garbled quotations from my book.

Proof.—The anecdote related in my book, from which the quotations of the reviewer are taken, relates to a matter of fact, which he has not dared to repeat; or what he has called 'bad taste' would have appeared to be 'plain truth.'

Corollary.—O's conduct, therefore, is 'wanting in candour.'

After sketching a short and rapid account of the history of prussic acid, taken in some instances *verbatim*, though without acknowledging it, from my work, the reviewer proceeds to assert (page 401) that—

1. I have adverted to the history of that substance *superficially*.

2. That I have given the different processes for preparing the acid *without sufficient remarks upon their principles*.

3. That I have passed judgment upon the merits of those processes, *not always tempered with mercy*.

To which assertions I beg to apply the following answers:

ANSWER I.—The chemical history of Prussic Acid is *not* adverted to superficially in my treatise. It is more fully given than in many recent works on chemistry. It is a hundred times more extended than that substituted by the reviewer for the edification of his readers.

Proof.—My chemical history of Prussic Acid begins from the discovery of Prussian blue, and terminates with the latest researches respecting the component parts and real nature of the acid itself, by Gay Lussac, whose notions and atomic theories are fully given; while the intermediate epochs of this interesting history are duly noticed; the labours of several eminent chemists, particularly those of Morveau, Scheele, Berthollet, &c. as well as those of M. Forreft in this country, are mentioned; and continued reference made throughout the section, as well as at the conclusion of it, to various works in which the subject has been treated in the most satisfactory manner. The historical account above alluded to occupies *twenty-four* printed pages of my work; while not one-third of that space, in the most approved modern works on chemistry, has been dedicated to that subject, excepting in the laborious and classical System of Chemistry by Dr. Thomson. The chemical history of the same substance submitted by the reviewer himself, occupies just *twenty-nine* lines.

Corollary.—O's assertion, therefore, respecting my *superficiality* is incorrect.

ANSWER II.—It is *not* true that I have given the different processes for preparing the acid 'without any sufficient remarks upon their principles.'

Proof.—My description of Scheele's process, written with what perspicuity I could master, is followed up by a complete *rationale* of the various steps of that process, which had been ambiguously interpreted by others. Of Vauquelin's process I observed, that its simplicity would render any remark of mine upon it an act of supererogation; and on the third, or Magendie's process, consisting in the mere dilution of the concentrated acid with water, I made enough remarks to attract the notice of the reviewer, who has grounded upon them a whole and long paragraph concerning their pretended incorrectness!

Corollary.—O's assertion, therefore, is again in this instance 'unjust.'

ANSWER III.—The judgment I passed on the different processes, is so far from being 'not tempered with mercy,' that in two cases it is given in a strain of eulogium; and in the third no judgment is given at all!

Proof.—Speaking of that of Scheele, I state, 'By this method the acid is obtained at an uniform degree of concentration;' and again, 'this acid is perfectly good for the purpose of practice;' while, on the subject of that of Vauquelin, I affirm 'that the acid prepared according to his method is of a proper strength for medicinal purposes.'

Corollary.—O's assertion, therefore, in this case, is again wanting in candour.

And here I cannot help observing to you, my dear Sir, that this advocate for mercy so far forgets his own precepts, that, throughout the review of my work, sentiments are uttered, and expressions used, that are very distantly allied, indeed, to that heaven-born virtue. As a proof of this assertion, I need only mention that the very processes of two such eminent chemists as Scheele and Vauquelin, which he affects to defend from my unmerciful judgment, are by him dismissed in the most peremptory language of condemnation; the one as furnishing an acid of 'variable composition;' the other for being 'extremely objectionable.'

Speaking of the very curious but difficult branch of chemical inquiry respecting the formation of prussic acid by the combination of animal matter, contained in the third section of my book, the reviewer says that 'I should either not have meddled with it, or given a clear epitome of what is known upon the subject.' In answer to which I have to observe—

1st. That I have *not* meddled with the subject, which is still involved in absolute obscurity, any further than by repeating an uncontroverted fact, which, instead of being quoted in a garbled manner, should have been fairly transcribed by the reviewer; and, 2dly. That the whole of what is known on the subject of the formation of prussic acid by the combination of animal matter, is detailed in the said section, although that *all* be but little, and betray 'a poverty in the land,' as the reviewer poetically expresses it. And I challenge him to point out any book on chemistry in which more, or even as much, is to be found on that subject, that is not conjectural.

Proof.—The very paragraph with which the section in question of my work begins, and which the reviewer imperfectly quotes as unintelligible, is given in the identical language of Berthollet, from whose *Essai de Statique Chimique* it was collected. Neither Thomson, nor Murray, nor Henry, nor Klaproth, nor Orfila, nor Lagrange, no, nor even yourself, in your manual, have alluded to the inquiry in question in the slightest degree, so that the 'epitome of what is known' may be well comprehended in the two pages that I have dedicated to that subject.

Corollary.—The reviewer, therefore, is on the present occasion, as on all the preceding ones, wanting in justice as well as candour.

But I have done more. I have given an extract from my notes taken while attending a course of lectures on animal chemistry delivered by Vauquelin, in which an endeavour is made to place the above subject in a somewhat clearer light. Yet what is the conduct of O on this occasion? With

the same nonchalance with which he had just before talked of the unintelligibility of two paragraphs borrowed from Berthollet and Thénard, he assures his readers that the extract from Vauquelin's lectures is not a little more intelligible; and dismisses it without any quotation in support of his affirmation. Now, as I mean, throughout this letter, to substantiate by *proofs* what I advance on the score of O's candour and justice; and as I hesitate not to assert that the charge of unintelligibility against Vauquelin is as gross a defection from truth as that which characterises the charge brought against his process for preparing the hydrocyanic acid, I shall beg leave to quote *once more* the passage in question, in order that the reader may judge whether or not it be *unintelligible*. * When animal substances are exposed to heat with a mixture of alkalis, hydrogen, carburetted, and carbonic gas, are obtained, besides a residuum, which, if washed in water, will be found to contain prussic acid. The alkali therefore seems necessary to form the prussic acid, by attracting together the principles of which it is constituted.* If this be unintelligible, then plain language is not capable of expressing common ideas; but if, on the contrary, the passage be found perfectly comprehensible, and such, indeed, as will be met in substance in the works of most men of eminence who have written on the same subject, then the conclusion regarding the reviewer's candour is unavoidable.

The reviewer's charge (at page 406) of my having unnecessarily separated the account of the physical properties of the prussic acid from its chemical history and preparation, is of too trivial a nature, and absurd, to need refutation. Every book on chemistry is full of examples of such practice; and Gay Lussac himself has followed no other method in his admirable essay on Hydrocyanic Acid.

In the succeeding paragraph of his critique, relating to the physiological experiments made with the pure hydrocyanic acid by several authors and myself, the reviewer remarks, that, 'as Mr. Brodie's investigations upon this subject are the most satisfactory that have hitherto been made, and as they are not even alluded to by me, he shall decline troubling his readers with those I have detailed.' To which this is my reply:

1. Mr. Brodie never made any experiment with the pure hydrocyanic acid.

2. Previous and subsequently to Mr. Brodie's investigation respecting the action of various poisons on animals, Coullon, Emmert, Magendie, and others, had and have instituted experiments with the pure hydrocyanic acid, or with substances containing it, not only upon animals, but upon the human system; which, in a work of practical utility, and not simply of philosophical speculation, could not but be preferred to every other experiment.

Either, therefore, O knew all this; and, in such a case, where is candour and truth in concealing it?—or he knew it not; and in that case it was his duty, ere he undertook to criticise the book, to have made himself master of its subject.

The eighth section of the work relates to the means of detecting prussic acid, and preventing its poisonous effects: 'in neither of which,' says the candid and just reviewer, 'do we remark any thing either very new or very important;' but respecting which I must beg leave to ask him two questions.

1. Is it not *very important* to determine the symptoms of poisoning by this acid, and to ascertain the best means for counteracting its deleterious effects? These objects have been accomplished, as far as they could be, in the said eighth section.

2. Is it not *very important* to be acquainted with the means of detecting the presence of prussic acid, particularly in cases of death from that substance? And have these means, or the mode of conducting the investigation, been pointed out to the public before the appearance of my work, by any chemist, English or foreign? Is not that new, which is not to be found elsewhere? Is any thing of the kind contained in the works of Fourcroy, Chaptal, Thénard, Thomson, Murray, Orfila, Henry, Children, or even in your own Manual of Chemistry? No.

Then what becomes of the justice, correctness, candour, and, I may now add, the knowledge (in these matters) of the reviewer? Perhaps some evidence of all these qualities is to be found in the remaining part of the Review, relating to the most important as well as to the largest portion of my work, which is dismissed in ten lines and a half!

I am now arrived at that part of my reply upon which I enter with feelings of great reluctance, because it alike involves charges of a heavy nature against the reviewer, and obliges me, from a sense of what is due to truth, publicly to deny the correctness of opinions said to be your own.

My charges against the reviewer are, 1. Misrepresentation, or concealment of facts. 2. Ignorance of the subject on which he has undertaken to pronounce. 3. Unworthy insinuations against the author whose book he reviews. Each and all of which charges, in pursuance of the plan I have followed throughout this Reply, I shall proceed to substantiate by positive proofs; leaving, however, to the public, the task of drawing, in this instance, the corollaries that must necessarily follow.

The first charge, or that of misrepresentation or concealment of facts, is supported by the following evidence.—1. The reviewer tells his readers that the formula of Dr. Magendie for diluting Gay Lussac's acid is not given in my book, (p. 402); whereas the *fact* is, that, at p. 20 of my Treatise, I have inserted that formula thus: 'Dr. Magendie dilutes the concentrated acid of Gay Lussac with six times its volume, or eight times and a half its weight of distilled water.'—2. The reviewer, in the same paragraph, informs his readers that the number 9.20583 is quoted by me as the 'medium density' of Magendie's diluted acid: whereas the truth is, that I distinctly used the word *weight*, meaning the absolute weight, and not the *medium density* of a mixture of 8.5 of water and 0.70583 of concentrated acid.—3. The reviewer says, I have 'fallen into some sad errors respecting the specific gravity of the pure acid'; and merrily and triumphantly quotes a typographical fault, by which I am made absurdly to state the specific gravity of the acid to be 70.583; whereas in the *errata*, which every candid reviewer would have turned to on seeing such an absurd mistake, the printer's error is actually rectified, and the specific gravity correctly given thus—70583.—4. The reviewer states, 'that the Doctor insinuates, though he must know better, that the acid sold at Apothecaries' Hall is *always* turbid, yellowish, and *impure*.' This is not true. The Doctor never expressed such an insinuation; nor did he use the two words, 'always' and 'impure,' which the reviewer, with utter disregard to propriety, has attributed to him, and has even marked in *italics*.

* It is not a little curious that one of the most important works on Chemical Science written in the English language, should contain a passage nearly similar in import to the above, on the subject of Prussic Acid. Having described the process of heating blood and alkalis to procure the acid, the author proceeds to give the following explanation of that process:—'This process consists essentially of two operations; one, the impregnation of the alkali with that peculiar principle contained in the blood, which gives the power of striking a blue colour with iron, and is called the prussic acid; &c.—*vide Aikin's Dictionary of Chemistry, art. Prussian Blue.*

The following is the only passage in which I passed any degree of condemnation on the acid prepared at Apothecaries' Hall: 'I know, besides, that the acid thus prepared is of a turbid yellowish colour, instead of being colourless and transparent, and that it deposits a considerable sediment; both which circumstances *seem* greatly to militate against its purity.'

The second charge against the reviewer, or that of ignorance of the subject on which he has undertaken to pronounce, is thus substantiated. 1. The reviewer states, at p. 400, that he is 'not quite clear' whether Gay Lussac gave the name of *cyanogene* to the base of hydrocyanic acid, 'because it burns with a bluish-purple flame, or because it is essential to the production of Prussian blue;' whereas Gay Lussac is positive as to the latter reason; and every chemist acquainted with the subject knows it to be so.—2. The reviewer asserts, that the acid obtained by Scheele's method 'is of variable composition, because Prussian blue is not always of equable purity;' but this is not true in practice. Mr. Garden has prepared prussic acid according to this method for the last three years and a half, with an invariable precision of result and equality of strength, which, to judge of its constant specific gravity, is stronger than that prepared at Apothecaries' Hall.—3. The reviewer has acknowledged his inability of obtaining *pure* prussic acid by Vauquelin's method, which he asserts to have tried frequently. But the want of success in his case must be ascribed to his ignorance in chemical manipulations, *as you well know*; for you have seen pure prussic acid prepared according to that method.—4. The reviewer says he cannot understand the following passage of my work, containing the well-known hydrodynamic axiom, that 'the weight of fluids is equal to their volumes multiplied by their densities.' I can only observe in reply, that, if he is a stranger to mathematical language, he has only to refer to Dr. Young's Lectures on Natural Philosophy, or Biot's, or Brisson's, or any other author's, work on the same subject; in all of which that very axiom is mentioned and explained. In the instance above alluded to, the passage was translated *verbatim* from a note in Dr. Magendie's pamphlet on the subject, of which I was commencing.—5. The reviewer, making himself quite merry, and rioting in the pleasure of the discovery of 'the Doctor's sad errors,' asserts that the specific gravity of a mixture of six volumes of water at 1, and one volume of acid at 0.70883, such as Magendie employs, *must be* 0.9900; whereas any person acquainted with the common rules of alligation, would know that $\frac{6 \times 1 + 0.70883}{7} = 0.9379$. To

which I add, that such is nearly the actual specific gravity of the mixture in question, ascertained by repeated experiments; the 'increase of density resulting from the mixture of the pure acid with water' not being so 'great' as the reviewer boldly asserts.

The third charge against O, namely that of throwing out unworthy and unwarrantable insinuations against me, will be best substantiated by a quotation of his own expressions. 'We should have conceived it more decorous on the part of Dr. Granville, finding the above preparation objectionable, as he has asserted it to be, to have stated the objections to the Apothecaries' Company, instead of publishing their process with a view to depreciate it, and to employ it as a vehicle of a puff-blow in favour of the doctor's chemist, Mr. Garden.' I despise too much the individual, who, without the slightest degree of evidence in support of them, can assume and publish too such inferences, and direct them against the moral rectitude of an author—to be disposed to take any other notice of the above disgraceful insinuations against my character, than to express my utter astonishment, that you should have suffered it to appear in your Journal! Did you know of any thing in my conduct during the six or eight years of our acquaintance; or any passage in my work which could have led you to admit, for one instant, the propriety of the aspersions of my reviewer? The only passage that relates to Mr. Garden, the chemist, and which is contained in the same paragraph of my work, which seems to have given such mortal offence to O, is this: 'I have not had an opportunity of trying this acid (the apothecaries'), as I am satisfied with that which Mr. Garden prepares for my patients.' Is *truth*, then, synonymous with *puff-blow* in the moral lexicon of my reviewer?—As to its being or not "*more decorous*" for me to have stated the objections I entertained against their acid, to the Apothecaries' Company themselves, instead of publishing those objections; I have yet to learn that that worshipful body have any claim to the services of any physician. Let them look to their own business, and see that their own officers do their duty. O gives a sort of manifesto in his review, from the Apothecaries' Company, in which they declare, that they "*have no secrets*;" if so, why should he feel sore at my having published their process of preparing the hydrocyanic acid? and how, I would furthermore ask him, can the publication of such a process, "*depreciate it*," as he rather awkwardly states, if the process be inherently perfect?

A few more words on the subject of certain opinions ascribed by the reviewer to yourself; and on the defence set up by that gentleman in favour of the apothecaries' acid of 1819 and 1820, and I conclude.

You are said in the Review, to have reported to the Laboratory Committee of the Apothecaries' Society that, 'having tried the methods of Scheele and Vauquelin you found them uncertain in their products, and more especially in the latter case, the specific gravity of Vauquelin's acid *always* exceeding that of distilled water.' In opposition to which opinions, I assert, 1. That Scheele's method, with common precaution, yields, by far, the most equable and the purest acid for medicinal purposes, of a specific gravity inferior to that quoted by you as the density of the acid prepared according to your own formula, and which is stated to be 0.995, although I have found it to be as high as 0.998 in two specimens procured at the Hall a few weeks back—whereas, 0.993 is the invariable density of Scheele's acid at an uniform temperature. 2. That by Vauquelin's method, prussic acid, possessing all the requisite physical and medicinal properties, colourless, transparent, and of the specific gravity of 0.998, and, consequently, under the standard density of distilled water, can be procured—is annually procured to a large amount by the French chemists, who sell no other—and has been procured by me at two different times, since your assertion to the contrary has been published by the reviewer. You have not, I suppose, forgot, that on the evening of the 18th ultimo, I shewed you, at the table of the Royal Society, a specimen of the acid so prepared by me, as well as another prepared, according to Scheele's process, by Mr. Garden, which you admitted to be 'as good specimens as could be desired, and the purity of which, to judge of their specific gravity marked on the phials containing them, seemed to be quite unobjectionable.' It was on that same occasion, that I remarked to you, that the accuracy of Vauquelin was too well established, to allow us for a moment to suppose, that he would have recommended a process which, according to your expressed opinion, 'is uncertain in its products,' and the specific gravity of which 'is *always* greater than that of distilled water.' To the justice of this remark you then assented. The specimens alluded to were shewn that same evening, and in the same place, to Dr. Holland and Messrs. Phillips and Faraday, to whom, as well as to you, I exhibited two other specimens of the acid, procured at Apothecaries' Hall in 1819 and 1820. One of these, presented a fluid of a dark-brown colour—turbid when shaken—but transparent when suffered to rest so as to give time to a copious blackish sediment to subside: the other offered a fluid of a muddy coloured appearance, though by no means so striking as in the former

case. From these two specimens, obtained at two different periods from the Hall, I then declared you, and to the other gentlemen above mentioned, what I again repeat on the present occasion, that the description of the Apothecaries' acid, contained in the second edition of my work, was taken; and I may, I think, call upon you to say, whether under such circumstances, I should not have been justified in passing a *severer* judgment upon the merits of that acid, than that which my reviewer has qualified by the expression, 'not tempered with mercy?' and to which I omitted to advert in another part of this reply, because I considered the present as a better opportunity for so doing.

The fact is, that both you and the reviewer—the one verbally, in conversation with me—the other in writing, at page 404 of his Review, have admitted the justice of my remark as to the appearance of the acid prepared at Apothecaries' Hall; and it remains only for me to say, in conclusion, that if my reviewer can assert as he has done at page 404, that 'the occasional yellowness and turbid appearance of that acid is *rather an indication of its purity than otherwise*,' his logic will certainly fail to convince his readers, as it failed to convince myself. If, however, by the admission that the acid is occasionally yellow and turbid, and presents moreover, a sediment, its decomposition is also admitted (which the reviewer has virtually done); then a preparation liable to such fatal objections, ought not to have been defended as a 'very uniform and very pure product;' nor sold as *pure prussic acid* by the Apothecaries' Company, to supply 'the occasional demand' for that article, as they have unquestionably done in the case of the two specimens in my possession, which were nearly as objectionable in their appearance when first procured, as they are now, when 'age' and 'purity,' says the reviewer, are to be considered as the two causes of those objectionable appearances. For the information and caution of every medical practitioner, I publicly exhibited, last night, at a meeting of the Medico-Chirurgical Society, the two latter specimens, as well as those alluded to in the course of this letter; which have been prepared agreeably to Scheele's and Vauquelin's methods—and will shew them to any other member of the profession, who may be desirous of inspecting them.*

As to the tone and style, generally, in which O's review is written, I can only assent to the judgment passed upon them, in a scientific circle, by one of the first philosophers in the country;† to whose opinion both you and I must readily bow—which judgment, given in the presence of two or three other gentlemen, but addressed to me in particular, went to condemn both as equally derogatory from the dignity of a reviewer, and injurious to the journal in which such a Review has been admitted.

I have the honour to be your humble servant,

A. B. GRANVILLE.

Saville-row, Wednesday, 7th Feb. 1821.

* The relative value of the acid prepared by Mr. Garden, and at the Hall, agreeably to Mr. Brande's formula, will be further illustrated by the following facts. A pupil of St. Bartholomew's Hospital assured me, the other day, that the Apothecaries' acid had been administered to patients in that hospital in doses of twenty-four drops at a time, without the slightest obvious effect. Mr. Travers mentioned, at the Medico-Chemical Society, a few nights since, that a servant of St. Thomas's Hospital having inadvertently swallowed a mixture containing from ten to twenty drops of acid, which he mistook for an aperient draught, fell down on the floor, as if shot by a cannon-ball, and continued it for several days. The acid used in this hospital is prepared at Mr. Garden's.

† This last paragraph has undergone a trifling verbal alteration, since the manuscript was submitted to Mr. Brande, and rejected—in consequence of that gentleman having erroneously interpreted its original meaning, and with a view to avoid any such misinterpretation.

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From January 20 to February 19, inclusive.

Day of Month.	Moon.	Rain gauge.	THERM.			BAROM.		DeLue's Hygro.	WIND.		ATMOSPHERIC VARIATION.		
Jan.			48	50	40	30.35	30.53	62.62	WSW	W	Fine	Cloud.	
20			38	42	39	30.63	29.65	60.60	W	SW	Fog	Fine	Cloud.
21			40	42	38	30.62	30.62	60.60	W	WNW	Cloudy		
22			40	41	38	30.72	30.73	60.60	NE	NE	Cloudy	Fine	Cloud.
23			39	41	33	30.07	30.61	59.61	NE	N	Fog	Cloud.	Fine
24			38	42	38	30.59	30.57	62.63	NNE	SE	Fog	Cloud.	
25			39	41	38	30.57	30.50	62.60	NE	ENE	Cloudy		
26	☾	.03	38	40	36	30.40	30.36	60.63	ESE	SE	Fog	Cloud.	Rain
27		.05	37	39	33	30.25	30.27	61.60	S	SSW	Rain	Cloud.	Fine
28			36	38	38	30.21	30.25	59.62	SSE	SSW	Fine	Fine	Cloud.
29			45	48	42	30.50	30.30	63.65	SSW	SW	Cloudy	Fine	
30			46	49	45	30.37	30.40	64.64	SW	WSW	Fine		
31													
Feb.			49	50	45	30.39	30.25	63.63	SW	W	Cloudy	Fine	
1	●		47	50	37	30.20	30.27	63.60	WSW	SW	Cloudy	Sho'ry	Fine
2			41	45	35	30.32	30.17	58.58	WSW	WSW	Fine		
3			38	42	32	30.21	30.33	57.58	WNW	NNE	Cloudy		Fine
4			33	38	32	30.61	30.76	57.55	N	SW	Fine		
5			35	41	32	30.76	30.71	54.53	SW	SW	Cloudy	Fine	
6			35	43	34	30.70	30.68	54.54	SW	SW	Fine		
7			38	45	33	30.58	30.42	56.55	SSW	SE	Fine		
8			34	42	36	30.22	30.10	54.57	SE	NE	Fog	Fine	
9	☾		40	45	35	30.25	30.46	60.60	NE	ESE	Cloudy	Fine	
10			37	42	34	30.34	30.25	57.57	E	E	Cloudy		
11			34	38	34	30.24	30.31	58.58	NE	NE	Cloudy	Fine	Cloud.
12			33	38	34	30.31	30.25	57.57	NE	E	Cloudy	Fine	
13			35	38	34	30.23	30.31	57.57	NE	WSW	Fog		Cloud.
14			34	38	31	30.36	30.50	57.57	WNW	E	Fog	Cloud.	
15			33	36	31	30.51	30.42	56.57	E	SSE	Cloudy		
16			33	36	30	30.37	30.24	58.59	NE	NNE	Fog	Cloud.	
17	○		34	37	30	30.18	30.26	58.58	NW	W	Fog	Cloud.	
18			32	35	31	30.31	30.30	57.57	NW	WNW	Fine		
19													

The quantity of rain fallen in the month of January,
is 2 inches and 9-100ths.

Corrections to be made in the PREMIUM to this Volume.

Pages xli. and xlv. for Dr. Gordon read Dr. Copland; and add to the list of Papers referred to in page lxxxiv. "on certain Painful Affections of the Intestinal Canal, by Dr. Powell."

THE LONDON
Medical and Physical Journal.

4 OF VOL. XLV.]

APRIL, 1821.

[NO. 266.]

NOTICE.

The Proprietors acquaint the Medical Profession, that the GENERAL INDEX to this Journal is published, comprising an Analytical Table of its Contents, from VOLUME ONE TO FORTY inclusive; carefully arranged in Alphabetical Order, with References to the whole of the cited Authorities, under their nominal Characters, &c. and forming an indispensable APPENDIX to the Journal; in one large 8vo. volume, price 21s.

Original Communications, Select Observations, etc.

On Vaccine Inoculation. By Dr. KINGLAKE.

IT is much to be deplored that objections are cursorily taken up, and urged, against the anti-variolous power of vaccine disease, in a way that is far too capricious and illogical to be in any degree conclusive on the question. Every instance of apparent failure of vaccine inoculation in preventing small-pox, is regarded as bearing additional testimony against the influence of cow-pox in subduing the natural liability to the variolous disease. Instead of investigating the cases which are said to have failed with an earnest and diligent endeavour to detect the real state of the fact, too much credit is given to rumours on the subject, which, on close inquiry, are almost invariably found to be either wholly groundless or to rest on gratuitous assumptions.

The prevailing reputation of the insufficiency of vaccine inoculation for the purpose for which it is instituted, is in this neighbourhood such as almost to challenge the possibility of defending its efficacy from the wide-spreading opposition with which it is assailed. It is obvious that those who are chiefly occupied in opposing the vaccine practice, are persons who are not competent to examine the subject on any scheme that could confer validity on the unfavourable opinion that is adopted respecting it. Popular feeling on a medical subject, is more likely to be erroneous than correct; and, whatever may be the current of general belief on these occasions, it can scarcely be restrained by reasoning or counteracted by facts.

The vaccine practice had to contend originally against prejudices of long and established growth. The general feeling was in favour of inoculated small-pox, which, compared with

the natural disease, was a greatly diminished and a very manageable affection. It was hardly probable that this great practicable benefit would be relinquished, to substitute in its stead a novel and an untried mode of effecting a similar object of security by means apparently far-fetched and arbitrary, or, at least, not intelligible to the public understanding. In this state of change and abandonment of an old acquaintance, it was not extraordinary that disappointment should be anticipated, and that occurrences at all savouring of uncertainty or indecision should be precipitately regarded as proofs of failure and insufficiency. Thus is the popular feeling at the present moment disposed to condemn and reject the vaccine practice, and to accumulate, in the most vague and questionable manner, testimonies against its anti-variolous influence.

The rumours of insufficiency in the vaccine disease as a permanent security against the infection of small-pox, are extensive, and propagated with a degree of confidence that will require the utmost address of the medical faculty to repress and overcome. These reports have certainly originated in misapprehension of the nature of the disease that has imposed itself for small-pox. It has been erroneously imagined that the vaccine disease was to afford an exemption from almost every variety of herpetic complaint: whenever, therefore, an eruptive distemper has occurred, it has been, by the aid of the most equivocal and fanciful resemblances, misconstrued into small-pox. Essential differences as to the appearance, stages, and duration of the disease, have been excluded from consideration, for the purpose of deciding on the identity of variolous affection. In the persuasion that every cutaneous eruption, whether confluent or distinct, must be small-pox, scarcely a disease of the skin can occur, whether affecting the general health or not, or however dissimilar its progress may be to that of small-pox, but it is indiscriminately held to be variolous; for no better reason than that the patient had been vaccinated, and yet the skin retains a susceptibility for eruptive malady!

The delusion into which the public mind has been betrayed by the occurrence of eruptive maladies in vaccinated persons, affords no sort of warranty for the opinion beginning to prevail, that the alleged vaccine preventive is fallacious and unworthy of confidence. The instances of the reputed failure of the anti-variolous power of vaccination, must be cited with the utmost minuteness; all the attending circumstances should be stated; and an analogy be made out, of the most strict and undeniable description, before either the want of vaccine efficacy, or the identity of variolous disease, can be justly recognized and admitted.

It has occurred to me to see many of the cases affirmed to be

variolous after vaccination, but in no one instance has the affection appeared to be sufficiently genuine to authorize a conclusion that it was really small-pox. The resemblance was but very faint in any particular circumstance; if, indeed, it could be considered as presenting an outline of the disease, the filling-up, so as to afford it a cognizable likeness, would be a work of imagination, wholly inconsistent with the natural appearances sought to be imitated.* Things which are naturally different cannot be identified; the subsisting dissimilarities will be shown in corresponding aspects, which never can assume a similitude amounting to sameness.

It is probable that the vaccine disease, supplied as it is by a morbid process taking place in the animal economy of the cow, is in reality of the same kind as the variolous affection occurring in the human species of animal life, and that the two vary only in the temperaments of the different animal organizations in which they are respectively generated. The variolous disease may as well originate in the cow, or any other animal, as in the human frame, whether its source be in any relative conditions of the atmosphere, or in any latent properties connected with the standard health of the vaccine animal. The disease may also be extended to the cow by contagion. The warm head of a person infected with the small-pox might, in the act of milking, impart the disease to the cow; or it might, in turn, be communicated to the milker of that animal. In either case a disease results, which in the cow is called cow-pox, and in the human species small-pox. A difference fully equal to what is present may be supposed to arise in the same disease, occurring in animals so dissimilarly constituted, nourished, and habituated, as the human and brute kinds of organic life are. If the diseases be essentially alike, differing only in accidental circumstances, then it is natural that, if the one could be trans-

* *December the 10th.*—A case of varioloid disease after vaccination has been just shown to me in this town, that approaches nearer to the true variolous character in its confluent form than any I had previously seen. The eruptions on the face had every where ran into each other, and had induced as large and as unsightly an intumescence of the whole countenance, as occasionally happens in the most violent degree of natural small-pox. The pustules on other parts of the body were distinct, less than the true variolous size, and without any central depression. The contents of the eruptions were puriform, and even the parts in both arms where vaccine inoculation had been some years before performed, were clustered with pustules. The throat also had been sore, and loaded with an inordinate secretion of mucus. But, with this aspect of real variolous disease, the constitutional excitement was not seriously disturbed. The febrile action was restrained, and the whole of the morbid process exhibited a mitigated and altered character from that which would have attended unvaccinated small-pox. Violent as this case appeared to be, it still seemed to be divested of the genuine variolous virulence, which probably had been so subdued or modified by vaccine influence as to have rendered a disease comparatively devoid of danger, which, in the natural and unchanged state, would have been imminently hazardous, if not almost inevitably destructive of life.

ferred from the brute to the human frame, it would neutralize or destroy the inherent susceptibility for subsequently taking the kindred affection, and thus prove an effectual preventive, such as the vaccine disease is held to be.

It probably would be impossible to induce in the animal excitability of the cow a disease closely resembling the variolous ailment, the nature of the animal being such as, perhaps, to be insusceptible of the aggravated form which that affection assumes in the human species; yet the attenuated and mild form under which the disease appears in the cow may be nevertheless the peculiar malady that constitutes small-pox, and, in as far as it is communicable, it avails in securing against either its spontaneous or artificial production in the virulent aspect under which it is apt to show itself in the human frame. If the diseases called vaccine and variolous possess radical identity, of which there appears to be much probability, the only question as to the preventive power that the one has over the other, of practical importance, is, whether the vaccine disease, or, in other words, the small-pox occurring in the cow, is not too mild, too destitute of virulence, permanently to destroy the susceptibility existing in the human frame for being affected by the variolous disease.

If the natural capability for being infected by small-pox can only be subdued or extinguished by a degree of morbid impression that could never wear out, then it may be reasonably supposed that an highly-attenuated or weakened state of the disease, whether resulting from the animal economy of the cow or any other animal, might furnish an insufficient security against future infection. The diseases would appear to be, in radical quality, one and the same; if, therefore, the milder or vaccine form be preferable to the human or variolous character, it deserves to be adopted under such precautions and observances as would be likely to render its desired influence infallible.

The utmost caution is necessary in the practice of vaccination, to ensure the preventive influence against small-pox that is intended to be produced. If the vaccine disease has only a power of destroying susceptibility for variolous affection when it is applied in its most genuine state, it behoves the advocates and promoters of the substitute to look narrowly to that real quality by which alone the object in view can be accomplished. It should be sedulously noticed if there be, in the affection that has been induced, any variations, discrepancies, or incongruities that did not accord with the customary and characteristic form of the vaccine disease. The matter that is furnished by a vaccine pustule might, when inserted under the skin, induce inflammatory excitement, and that too of the areolous character,

that would have very much the aspect of the real disease; yet it may not come up to the standard for destroying the inherent susceptibility for small-pox; or, to be more explicit, it may not be the vaccine modification of small-pox that will prevent the human form of that malady from subsequently occurring.

Much assiduity and nice discrimination have been employed in delineating the precise state and character of the vaccine disease, that would counteract or resist a future appearance of variolous affection; and the directions given on this subject are sufficiently copious, practical, and efficient, to answer every useful purpose. The limpid state of the fluid resulting from vaccine inflammation, is that in which the most active and most anti-variolous quality of the disease is exerted. If the fluid be suffered to advance to the purulent state, the specific properties on which its genuine influence depends are either deteriorated or wholly annihilated. Infectious fluids acquire the power of communicating infection from a precise arrangement of their constituent parts. The vascular action of organic life, under circumstances issuing from the combining and arranging processes of animal chemistry, will furnish material power capable of producing a specific effect; and this power can only be known by the peculiar influence which it is enabled to exert.

Vaccine matter, from the time of its insertion under the skin until its final exsiccation, may be capable of producing a vast variety of disease, if employed at all the different stages of maturation; but it is well ascertained that it is only at a particular period of this process that the product has the power of exciting the peculiar disease that eradicates the native susceptibility for taking variolous affection. The numerous failures and anomalies that have occurred in the practice of vaccination, have chiefly resulted from the inoculation having been performed with matter destitute of the specific properties necessary for generating the real disease. Local inflammation, suppuration, and eschar, have been the demonstrable signs of final vaccination; but, unhappily, in many instances, they have not eventually proved the true *criteria* of efficient vaccination. The practical difficulty, and indeed objection, attending vaccine inoculation, is the precise and limited, though in different constitutions often variable, period, at which matter possessing the specific quality of the disease should be taken. The progress of inflammatory action at the place of insertion is not in all temperaments the same: in some it is earlier than in others, which requires that an experienced and discriminating observation of the actual state should at all times be made, to obviate the fallacies that would be likely to result from an imperfect condition of that fluid. To provide against the degeneracies to which all specific arrangements of animal substances are

liable, when detached from the circumstances in which they originate, it would appear to be advisable not to rely for an indefinite period on the vaccine matter that had been transferred through a very extended succession of human subjects. The diversity of temperamental influence that may have been exerted on it, may possibly disturb and lessen the precise condition of activity and power on which its anti-variolous efficiency depends. Recurring, therefore, at short intervals to the cow, the genuine source of supply, for fresh and unaltered matter, would be the best security against the diminished power, and eventual insufficiency, of a fluid that may have become, in some measure, effete and inert, by having been too long employed in human inoculation.

The reports of failures of the protecting influence of cow-pox against small-pox, have been so extensive and so various as to have ideally furnished almost every conceivable diversity that could occur in the variolous disease ensuing the vaccine inefficacy. It has been affirmed, that unmitigated and unaltered small-pox has presented in all its terrors; that the worst forms, and most inveterate symptoms, of that disease have arisen after vaccination. Hear-say authority on medical, as well as on all other subjects, is questionable, and should never be cited in justification of what is merely asserted to be true. It has not occurred to me to see an instance in which vaccine influence had been exerted in the system, where the aspect of the variolous disease that afterwards shewed itself sufficiently resembled the genuine character of that affection, to admit of regarding it as that ailment. That an approach may be made to the appearance of a given disease, is very probable: indeed, the radical similarity of most diseases, and the apparent affinity of many of them, must necessarily furnish the sort of general similitude that might go to particular instances of sameness; but this is not the identity that is insisted on, as obtaining between original small-pox and that affection which occasionally happens after vaccination. Vaccinated small-pox, if the phrase may be allowed, is so modified, so reduced in virulence, and so circumstanced in appearance, as to be essentially different from the primary and unneutralized disease.

If small-pox, in its natural and unadulterated character, can either be prevented, or so changed in its nature as to be divested of its virulence and rendered comparatively mild and harmless, the great object aimed at in substituting for it vaccination is answered; and, although the cutaneous surface, and even an irritable temperament, may not be secure against subsequent variolous excitement, yet it will be unaccompanied with danger, and, of course, not deserving any serious apprehension. It could not be justly imagined that a disease like that of small-

pox could ever be so annihilated in its power as to be incapable of exciting the irritable conditions of vital action; but, if that excitement be devoid of risk and attended with no considerable inconvenience, the affection has been disarmed of its malignity, and mankind have derived all the exemption from its destructive ravages that vaccination, or any other substitute, could have, reasonably, been expected to afford.

The anti-variolous power manifested by vaccine disease, has succeeded to an extent that evinces a certain force of that power, and proves that, were it applied in all the relative circumstances necessary to render it efficient, it would be as nearly infallible in its influence as the variable nature of diseased action will admit. If, in a majority of instances nearly reaching the whole, the vaccine disease be found to prevent the occurrence of small-pox, can it be justly held that it is an insufficient and an insecure substitute for that affection? It may be undeniably affirmed, that the examples of entire failure of the vaccine over the variolous disease are so few and so equivocal, as to afford no valid objection to the practice of vaccination. The benefit derived from substituting cow-pox for small-pox, is that of having a mild disease instead of one that is in its nature virulent, and occasionally attended with extreme danger. If, then, this amelioration can be obtained in a morbid excitement to which there is a natural liability, is it not a positive advantage, a comparative good, worthy of preference, and deserving of fearless adoption?

Vaccine disease may not exempt the skin from eruptive affections, nor does it seem capable of altogether extirpating the inherent susceptibility for variolous excitement, but it fails not to lay the disease under such restraints, and so to modify its power, as almost to annihilate its force and character. When it supervenes vaccine disease, its onset is slight, its duration short, and the attending pustular inflammation is in general insufficient for the suppurative process. The eruptions appear in diminished size, remain visible only for a few days, and then finally vanish. There is but little or no concomitant fever; indeed, hardly sufficient, generally speaking, to disturb either the healthy sensation or function of any vital organ. The alarm, then, would appear to be groundless concerning the incompetency of vaccine to subdue the natural susceptibility for variolous disease. It is fully equal to that effect with regard to all that can be correctly considered as specific, peculiar, and exclusively constituting the original nature and form of that affection. Persons who have undergone the morbid process of vaccination are, with respect to the variolous disease, in a state of security very similar to that of those who have been subjected to inoculation for small-pox. The efficiency of

variolous inoculation, like that of vaccination, will much depend on the specific properties of the matter employed for communicating the disease; and, when the affection has fully taken place, a new infection, to a limited extent, may possibly occur. The skin will take on the variolous excitement in two or more instances; but then, as after vaccination, it is in so subdued a form as not to exhibit the genuine character of original disease. Much obscurity exists in the laws by which inherent susceptibility for being only once affected by certain specific diseases, is governed. It seems to be the property of all substances exerting a stimulant agency in the animal economy, to become diminished in power by repetition and long continuance. Diseased excitement of every variety evinces this property to a given extent, but in particular instances, as small-pox, cow-pox, measles, whooping-cough, &c. it is more strikingly and positively exemplified. The primordial aptitude, therefore, for taking on disease, as shown in the innate susceptibility for small-pox, would appear, if not wholly extinguished by either variolous or vaccine affection having once actually occurred, to be at least so reduced and altered as not to admit of a state of disease that could assume any serious degree of constitutional violence.

Enough has been noticed of the nature and character of vaccine disease to remove all doubt as to its efficacy in preventing small-pox. Indeed, the proofs of this power have been so abundantly conclusive, that it would be superfluous to insist on a fact so obvious and undeniable. That which has occurred, therefore, in a manner so indubitable, will take place again in similar circumstances; so that natural correctness will be found to be perfectly steady and undeviating, whatever may be the vacillation and inconsistency of speculative opinions. Were the laws of nature to yield to the unreflecting impatience, to the prejudiced notions, and to the erroneous calculations of man, instability and disorder would soon predominate over fixation and harmony. Knowledge can only be acquired by patient and accurate observation. When the closest attention to facts has ascertained unquestionable truths, neither surmises nor rumours, however speciously authenticated, can invalidate the verity of such intelligence. The inadequacy of vaccine disease to supplant the inherent liability to small-pox, may be affirmed and maintained under a large share of apparent proof; but, if the natural law that governs the question at issue should oppose such a conclusion, it will be unavailing to attempt to withstand its irresistible validity.

The varioloid affection that has occurred, in numerous instances, after vaccination, as well as after small-pox itself, differs widely in appearance, and probably also in reality, from

genuine variolous disease. The opinion that varicella, or chicken-pox, has the same origin as small-pox, or that they are only different modes of the same malady, is not warranted by the one form of ailment preventing that of the other. The most violent degree of small-pox does not incapacitate the system for being affected by chicken-pox; nor does the latter destroy the natural susceptibility for assuming the former. They cannot interchange and become a substitute for each other, as might be effected if they were one and the same disease, and drew their respective existence from the same source. Unlike the vaccine fluid, the vesicular lymph of the chicken-pox will not, by inoculation, at all affect the natural liability for taking small-pox. Were the diseases identical, that would be the case; for, whether the one or the other form be brought into effect, the ground or nature of the affection would be similar. The influence of vaccination in obviating a subsequent accession of variolous disease, justifies the inference of there being at least an analogy subsisting between the diseases, approaching so nearly to identity, if not positively the same, as to render an attempt to distinguish them extremely difficult, and, in a practical consideration, unimportant.

If there be any varioloid quality in chicken-pox, it is too ineffective to produce any of the characteristic appearances and influence of the variolous disease. The intensity and quality of excitement necessary to generating the specific powers of small-pox have not existed in chicken-pox; it is, therefore, a state of disease varying too widely from variolous affection to admit of being correctly denominated a milder form of the same ailment. The vaccine malady has strikingly furnished a substitute for variolous disease, that either wholly supersedes the natural disposition to that complaint, or so lessens its original violence, as to change it into a disorder of no comparative danger. The pustular and vesicular characters of variolous and varioloid affections, whether ensuing vaccination, chicken-pox, or small-pox itself, evince the different degrees of inflammatory excitement attending those dissimilar forms of disease. The pustular inflammation of the small-pox is of the phlegmogenous kind, and is sufficiently powerful to induce suppuration, and to furnish the purulent fluid contained in the pustules: in the vacciulous and varicellous vesicles, the concomitant inflammatory action is of an erysipelatous nature, and terminates in a limpid effusion under the cuticle, and not in maturation. These differences are strongly marked, and demonstrate a variation in the force and condition of the morbid action obtaining in the several cases, that proves a very dissimilar state of disease. Were there a radical identity subsisting between the three different forms of disease distinguished by the several

names of variola, vacciola, and varicella, and the mildest of them could be substituted for the most violent, it would be (as appears in the adopted practice of vaccination) an enviable advantage, to prefer, in a choice of evils, that which would be least injurious in exerting a preventive influence against a severer form of disease.

If varicellous affection were as unequivocally influential in subduing the innate susceptibility for small-pox as vaccine disease, whether happening naturally or induced by inoculation, it would be just to regard it as another modification of variolous malady; and it would be immaterial which of the two should be adopted as a substitute for the more violent disease sought to be prevented. Whether chicken-pox be in its nature an original and distinct disease, or whether it be an offspring of small-pox, is but of small moment, compared with the decisive fact of its not being capable of counteracting the usual occurrence of that affection. If it really possesses an affinity to small-pox, it is too remote and inoperative to admit of being an effectual obstacle to that disease, and must therefore, with reference to any protecting power that it may have against small-pox, be considered as an independent distemper. Vaccine disease is characterized by the strongest kindred resemblances to small-pox, and has, in a degree equally extensive and gratifying, proved either an adequate bar against that affection, or has so altered and modified the natural susceptibility to it, as to have rendered its eventual occurrence exceedingly moderate, safe, and manageable.

Considering vaccine inoculation both retrospectively and prospectively, looking to its merits and demerits from all that has yet occurred on the subject, its just claims would appear to be imperative on the intelligent and humane advocates of that salutary practice, not to suffer their well-founded confidence to be in any degree shaken by the prevailing clamours and prophecies concerning its inefficacy in lastingly preventing the accession of small-pox. If popular doubts and alarms should be in any measure countenanced, or even connived at, by medical practitioners, the result will soon be irreparably injurious to the just pretensions of vaccine inoculation. Happily for the vital interests of mankind, the real anti-variolous power of the vaccine disease has not yet been impugned in a way to prove that efficient vaccination is not almost invariably preventive of small-pox; and that, when it does not altogether counteract the occurrence of that disease, it so changes its character, and so divests it of virulence, as to cause it to appear under a form too mild and tractable,—in fact, too slightly varioloid,—to warrant any anxiety for its termination.

The contributions made to the general amount of mortality

by the destructive ravages of small-pox previously to the introduction of the vaccine preventive, are truly appalling, and should elevate the best feelings of philosophy in defence of a practice that either wholly supersedes the small-pox, or modifies that mortiferous disease into a comparatively safe and harmless malady.

Medical practitioners have vast power in controlling and directing public feeling on the momentous subject of vaccination. All difficulties, all anomalies, occurring in the practice, should be, if possible, satisfactorily explained. No questions of uncertainty and solicitude should remain unanswered. Ample experience has been already afforded of the preventive power of the vaccine over the variolous disease, and its title to unsuspecting confidence would appear to be too well established to justify any reasonable distrust in its capability of either wholly resisting the occurrence of small-pox, or of so mitigating its virulence as to render it an affection of scarcely any calculable importance.

Taunton; December 15th, 1820.

Some Remarks on Mr. Smith's Statistical Inquiry respecting the frequency of Stone in the Bladder in Great Britain and Ireland.

By CORNELIUS TRIPE, Member of the Royal College of Surgeons in London.

IN the first part of the eleventh volume of the Medico-Chirurgical Transactions, there appears a statistical inquiry into the frequency of stone in the bladder in Great Britain and Ireland, by Richard Smith, Esq. senior surgeon of the Bristol Infirmary.

The substance of this paper, as it relates to the frequency of the disease, has been derived from the different county hospitals through the United Kingdom, in consequence of circulars, which were addressed by Mr. Smith to the surgeons of those establishments upon this subject. The information thus derived is erroneous to a very great extent.

Mr. Barnes, one of the surgeons of the Devon county hospital, in replying, for himself and his colleagues, to the inquiry which was addressed to the surgeons of that establishment, states the number of calculous patients which have been submitted to operations in that hospital, in the interval between 1806 and 1816, to be sixteen or eighteen cases; in the interval between 1811 and 1818, nine cases. Mr. Barnes also observes as follows, "This hospital may be considered as embracing *very nearly all* the stone-cases amongst the poor; for in this county, and some part of the adjoining counties of Somerset, Dorset, and particularly Cornwall, operations for the stone out

of the hospital *are very rare.*" He says again, "The population of Devon, in 1811, was 383,000: one may fairly calculate the hospital to have within its circle the stone-operations, with very few exceptions, of a population of 600,000; the population of Cornwall exceeding 200,000."

Within the last ten years, I have, in my private practice, operated for the stone on thirteen subjects of various ages; one of whom was a female, aged twelve years. The greater number of these patients were incapable of rendering any pecuniary return for the operation; and all of them were cured, excepting my last case, a boy twelve years old, who died in forty-eight hours after the operation, of consecutive inflammation. I have very lately sounded two male children, and shall, according to every probability, ere long operate on them. I have also known, within the last ten years, of two male adults who have died of the disease, without any operation having been performed: the existence of the disease was proved by dissection.

Mr. Hammick, the principal surgeon of the Royal Naval Hospital, whose name must be sufficiently familiar to the profession, and from whom (I feel a pride in stating it,) I derived the earliest principles of my professional education, first drew my attention to this subject a few days since. He, having witnessed, with my other friends, all my operations, was, in consequence, struck with the inaccuracy of Mr. Barnes's report, which he had then just read in the volume which has been referred to. He has also requested me to state, that he is quite certain there has been in these towns, within the last ten years, a number of cases of stone performed on by different surgeons, which, taken together, quite equal those which I have enumerated in my practice.

Thus it will appear, that the number of stone-operations in the private practice of an individual, during the last ten years, as far as we can judge by Mr. Barnes's report, is equal to the number of cases which have been operated on at the county-hospital within the same period. Add to this the cases operated on by other surgeons in these towns, and the total will then double the number which have been operated on in that hospital.

To say the least of Mr. Barnes's report, it exhibits a deficiency of necessary caution in reporting for the whole county of Devon, as he has done, without previously instituting an inquiry on the subject, of some one or other of those professional gentlemen who are practising in so important a part of the county as this is. The towns of Plymouth Dock, and Stonehouse, being very near each other, contain together about 70,000 inhabitants; and they are surrounded by an extensive

country, spreading into Cornwall, from whence diseases which are of an extraordinary character are constantly arriving, to obtain professional assistance. Had he condescended to make this inquiry, he would never have assigned, as he has done, to the institution of which he is a surgeon, the almost exclusive merit of treating this disease; stating that the Devon hospital embraced nearly all the stone-cases for the counties of Devon, Cornwall, &c. and that such operations any where, except in that hospital, were extremely rare in those counties.

May not some of the reports which have been furnished from other places be marked with similar want of caution? I would therefore submit, with deference, the impossibility, under the view of the error which has been taken, and the suspicion which such a circumstance is capable of casting over the reports in general, of any just opinions or inferences being drawn from the inquiry, as it at present stands.

I beg I may not be misunderstood as insinuating a suspicion that any individual who has contributed to that inquiry could be guilty of intentional omission. But it appears to me, that the surgeons of county-hospitals may not be aware to what extent the improved science of surgery has become diffused, in the present day, over the face of the country; and therefore I would suggest that hereafter, if information be required by any member of the profession, who may be actuated with such laudable motives as Mr. Smith has evinced, in addition to the mode of inquiry which he has adopted at the different county-hospitals, an invitation should be inserted, in the periodical Journals, to the profession in general, to contribute the results of their experience on the subject required.

I will only add, that, instead of the opinion prevailing here that the disease has become more rare, the question is frequently asked, "How is it that the stone in the bladder has become so frequent an occurrence, compared to former times?" We answer, that, to the best of our belief, the disease is not more frequent now than it has hitherto been, but that such cases were formerly not noticed, or were sent to the county-hospitals for cure; and hence, also, probably arises the difference in the comparative number of cases which we have seen published from that institution.

Plymouth Dock; February 14th, 1821.

Letter addressed to the Medical Profession generally, relative to Vaccination. By Dr. EDWARD JENNER.

PRESUMING that you are conversant with the practice of vaccine inoculation according to the instructions which I have formerly published, and that you may have seen, in

addition to my general observations, those which I have since made and promulgated, respecting the "Varieties and Modifications of the Vaccine Pustule, occasioned by an herpetic and other eruptive states of the skin," I take the liberty of requesting to be informed whether the observations acquired in your own practice coincide with mine? That is to say, whether the vaccine vesicles, under these contingent circumstances, go through their course with the same regularity as when the skin is free from diseases of this description?

Secondly, Whether, on the other hand, such individuals are more liable to resist the legitimate action of vaccine lymph, when inserted into the arms, than those who are free from such eruptive affections?

Thirdly, Whether you have met with cases of small-pox, or what has been termed the varioloid disease, after vaccination: and, if so, whether in such cases you ascertained those deviations at the time of vaccination in the progress of the pustules on the arms, which I have described as liable to take place when the skin is affected with herpetic and other eruptions?

As you may not have the paper before you to which I here allude, nor the short series which followed it, I will point out the periods of their publication, and where they are to be found. The first was published in the Medical and Physical Journal, No. 66, for August 1804, and gives an outline of the subject, of some extent. It points out the fact, that a single serous blotch upon the skin, existing during the progress of the vaccine vesicles on the arms, may occasion such irregularity and deviation from correctness, that vaccination, under such circumstances, cannot be perfectly depended on.

I have found abrasions of the cuticle to produce the same effect; such, for example, as we find in the nurseries of the opulent as well as the cottages of the poor, behind the ears, and upon many other parts where the cuticle is thin. Happily we find no irregularity in the vaccine vesicle in an uncontaminated skin; but we find it if the skin is beset with these herpetic blotches, or even simple serous oozings from an abraded cuticle. It is not to be considered as of less consequence when occupying a small space; a speck behind the ear, which might be covered by a split-pea, being capable of disordering the progress of the vaccine vesicle. Dandriffe may be considered as a malady of this class, the incrustation on the scalp being formed from excoriation beneath; and however slight, for there is every gradation between a thin scurfy layer of a dirt-looking substance, or even patches of this thin crust, and tinea itself. However, fortunately for the safety of the vaccine practice, and fortunately, too, for the ease of the practitioner, all these affections of the skin may be removed with very little

trouble.* Sore eyelids are also impediments to constitutional vaccination.

The second paper relating to this subject was given by the late Dr. Willan, in answering to the following interrogatory, addressed to me by himself:† “What are the changes produced in the vesicle, when a person is affected during vaccination with the shingles, the vesicular ringworm, or impetigo?”

To this question I made a full, and, I believe, a satisfactory reply. Its purport will be shown by quoting a few sentences from it. “To answer this question in its fullest extent would lead me through a wide field of observation, which I mean to go over at a future time; but the following answer may probably convey to you as much information upon the subject as you may now require.” “Vaccination, under the circumstances you mention, usually produces a striking deviation from the perfect character of the vaccine vesicle at some period or other of its progress, but more frequently in its early than in its declining stages; indeed, it is commonly perceptible in a day or two after inoculation. It would be difficult, perhaps impossible, without the aid of drawings, to give a correct description of the varieties which an herpetic state of the skin is capable of producing, from those trifling deviations which prove no impediment to the vaccine security, up to that point of imperfection in the vesicle which affords no security at all. Perhaps I commit an error in saying “no security at all;” for it strikes me that the constitution loses its susceptibility of small-pox contagion, and its capability of producing the disease in its perfect and ordinary state, in proportion to the degree of perfection which the vaccine vesicle has put on in its progress, and that the small-pox, taken subsequently, is modified accordingly.‡ When no deviation takes place in the ordinary course of the vaccine vesicles, or when it is inconsiderable, the herpetic blotches or vesicles, of whatever kind they may be, often assume (sometimes as early as the third or fourth day after the insertion of the vaccine fluid,) a new character, not unlike the vaccine,

* The most effectual application which I know for subduing these cuticular diseases that produce impediment, is the unguentum hydrargyri nitratis, as much lowered with unguentum cetacei, or any other bland ointment, as the irritability of the subject may require. The dandriffe demands a double process: the first consists in removing the incrustation, the second in subduing the oozing. There are skins that will not bear unctuous applications: the desiccative lotions may then be made use of two or three times a-day; such as those prepared with the sulphate of zinc or superacetate of lead, &c.

† It was published in the year 1806, in his *Treatise on Vaccine Inoculation*.

‡ Further observation has confirmed this opinion, and has also developed much other curious matter respecting the spontaneous blending of the herpetic with the vaccine fluid, through the medium of the constitution, when under the influence of herpes.

and, keeping pace in their progress with the vesicles on the arm, die away with them, leaving the skin smooth."

These two papers comprehend, first, the simple fact of important deviations being produced by diseases in pre-occupation of the skin; and, secondly, a general account of the characters of these deviations, and their differing degrees of influence upon the vaccine protection.

Some further observations were published by Dr. Wilson Philip, of Worcester, in an Appendix to his work on Febrile Diseases, who requested some information from me on this interesting subject. This letter goes more into detail than the former, though its purport is the same,—namely, to guard the practitioner against the insidious influence of a diseased skin, when he vaccinates. It will be an object of future consideration to enter more generally into the minutiae of this subject; but a sketch like this does not afford scope for the completion of such a design. Let me advise every practitioner not to confine his cautions, nor to narrow my meaning, to one class of eruptive affections. In short, every disease of the skin which may be called *serous*, or one that sends out a fluid capable of conversion into a scab, has the power of exerting this modifying and counteracting influence; and I have also seen purulent fluids exert a similar influence in producing deviations. If I was asked what were the other actual impediments to perfect vaccination, as a general answer I should say, that I scarcely know any other except spurious matter,* or impediments too obvious to require my naming them here, such as deranging the vaccine vesicle in its progress, by incautiously robbing it of its contents, or producing a new action by external violence.

Berkeley; Feb. 1821.

Case of Hydrophobia. By WILLIAM MINES, Surgeon.

ROBERT MERRY, footman to Robert Sheriffe, Esq. of Diss, applied to me for medical aid, December 1st, 1820, eight o'clock, A.M. complaining of having had a very restless night: he could get no sleep, from unpleasant symptoms about his throat, with extreme difficulty of swallowing even his saliva; and at times his breathing was much oppressed. His pulse was under

* I am happy to see that these interruptions are now discovered in Germany; as appears in Professor Hufeland's Journal for June 1819, an extract of which is given in the London Medical Repository, vol. xiv. p. 502.

In addition, see Bateman's "Synopsis of Cutaneous Diseases," pp. 222, 223; Cross's "History of the Variolous Epidemic at Norwich, 1820," pp. 60 *et seq.* 196 and 288. I was lately puzzled to find the cause of irregularity in a vaccine vesicle, the skin being free from any apparent eruption: upon minute inquiry, I discovered a whitlow on the thumb, in which suppuration had taken place.

70, soft and regular, without fever and, apparently, any inflammatory action. A draught, with camphor julep, &c. was poured out for him, which he appeared very anxious to take; but struggled much from every attempt, declaring he could not swallow. However, with great difficulty a tea-spoonful or two was got down, which brought on considerable spasm about the throat, so that all subsequent efforts to get down food or liquid of any kind were fruitless, as every attempt brought on most violent startings; and even the name or sight of water was horrid to him; and, when poured from one vessel to another, it instantly threw him into strong convulsions.

Full forty ounces of blood were taken from the arm without producing syncope, or even any particular change or diminution of symptoms, and the pulse varied very little from the loss of blood. During the bleeding, and for a short time after, he several times called out he thought he was the better for it; but, as the symptoms did not abate, and the blood had not the slightest inflammatory appearance, it was not thought advisable to repeat the bleeding.

Five o'clock, p.m.—Dr. Shorting saw this patient; and, after witnessing the symptoms as before described, believed it to be a true case of hydrophobia, and, although no reliance could be placed in any thing, was anxious that mercurial frictions should be used largely, and considerable doses of laudanum given by injections; neither of which produced any sensible effect.

During this disease the patient was at intervals perfectly calm and rational, answering all questions, till nearly the last, when his convulsions became so strong that it was thought necessary to put on the strait-waistcoat for safety; to which he made no resistance. There had been no slaver about the mouth, till nearly the last, when, half an hour before his dissolution, he vomited up several spoonfuls of froth; and a few minutes after died without a groan or a struggle, (six o'clock, A.M. Dec. 2d,) labouring under this dreadful disease in its active form only twenty-two hours.

This poison must have been received about three weeks prior to the attack, at which time the servant had waited upon a terrier that died with the disease, having been bitten by a mad dog passing through this town the latter end of July last; which terrier had likewise worried and slavered a spaniel over, that the servant assisted in wiping and cleaning; but from neither of the animals did he receive any bite, nor had he apparently any abrasion or scratch of the skin.

Diss; January 1821.

P.S.—I regret I am not able to add the appearances on dissection: permission to open the body having been denied by the patient's relatives.

Histories of two Cases of acute Inflammation of the Spinal Marrow.
By Dr. PINEL (the younger). Originally read to the Society of the College of Physicians of Paris.

MARY BRISSET, twenty-seven years of age, having always enjoyed good health, though of very acute nervous susceptibility, was accused of theft in the house in which she lived as a servant, and discharged in consequence of the false suspicions entertained respecting her. Her menses, which had flowed for three days, were immediately suppressed. She was deeply affected by the unmerited treatment she had received. On the third day from this time, she was found in her bed in a state of complete suspension of the functions of the sense and the understanding. She was then taken to the Hôtel-Dieu. After remaining there six weeks, the stupor disappeared; but a state of demency remained, and she was, consequently, transferred to the Salpêtrière, August 18, 1818. At the time of her entry here, she was in the following state: Her look was bewildered; she had difficulty in articulating words; her answers were slow, difficult, and but rarely appropriate; there was great want of energy, but not paralysis, of all the limbs; she lay in a state of almost continual quietude, though she sometimes burst into violent fits of anger and impatience. The organic functions were executed perfectly and vigorously. But slight variations in the state of the patient were witnessed during fifteen months; it was only observed that she became very fat. On the 15th of January, 1820, she was seized, in the evening, suddenly, with convulsions. On the following morning, her mouth was found to be enveloped in froth; her eyes turned upwards; the jaws were spasmodically closed, except that the teeth were grated together; the trunk of the body agitated with convulsive shocks, repeated three or four times in a minute; the limbs were motionless, not at all participating in the convulsions of the body; the pulse full, frequent, irregular, and tumultuous; respiration short, oppressed, and precipitate; the alvine dejections involuntary; and the whole of the body covered by an abundant sweat, of a strong and tenacious odour, rising in vapour from the patient. The state of carus was profound. For three days, the convulsions of the trunk were continually repeated, appearing to increase in strength in the evening, and accompanied then by a febrile paroxysm. The other functions were similarly disordered. The patient died on the morning of the 18th of January, without having had any intermission of the symptoms above designated.

Examination of the body, thirty-six hours after death.

The exterior.—Habit, very lusty. The muscles of the face not contracted.

The head.—The cranium thick, and injected; the dura mater

thin, almost diaphanous; the longitudinal sinus gorged with blood; the arachnoid presented, throughout the whole extent of the frontal and parietal regions, traces of inflammation of very ancient date; the pia mater was thickened by layers of coagulated lymph, and imbued with purulent serous fluid. The cerebrum and cerebellum, though carefully examined, presented nothing particular; the ventricles contained but little serum; the cerebral substance was firm and tenacious.

The spine presented nothing remarkable in respect to its membranes; but, after having divided the dura mater of the spinal marrow throughout its whole length, it was easy to discern a pultaceous disorganization of the medulla, commencing in the situation of the fourth cervical vertebra, and terminating about the fifth lumbar. Throughout the whole of this extent, the nervous pulp was reduced to a sort of yellowish, diffuent, and inodorous, porridge; towards the lumbar region, it resumed its ordinary consistence, and was bathed in a little reddish-coloured serum. The thoracic and abdominal viscera were perfectly healthy in their appearance; the interior of the stomach was of a light rose colour; the uterus was very small.

Second Case.—Felicia Lepoigny, of a sanguine temperament, having menstruated at eleven years of age, had always enjoyed good health until her fifteenth year: at this period, she was much terrified at the entry of the Russians into the village where she resided, and at being eagerly pursued by one of them, in consequence of which she was seized with a fit of epilepsy. Paroxysms of this affection were afterwards repeated, at first only after long intervals, but these gradually became shorter, and her intellectual faculties rapidly declined. She was brought to the Salpetriere in 1816, in a state of complete idiotism, accompanied with epilepsy, the paroxysms of which recurred every fourth or fifth day. During four years, hardly any change of her condition was observed. On the 7th and 8th of January, 1820, the fits were very frequent; but after this they returned to their ordinary type, until the 23d of January, when the convulsions succeeded each other with inconceivable rapidity. Her face was red and tumid; the muscles of the eye unequally and irregularly contracted; the trunk was almost incessantly agitated by convulsions, whilst the abdominal muscles were convulsively contracted in an undulatory manner; the limbs presented incoherent, but not convulsive, movements; sensibility, and the intellectual faculties, were totally abolished; the pulse was frequent, irregular, and low; respiration short and laborious. The convulsions and the other symptoms persisted, with the same degree of intensity, during the 24th; on the 25th, in the evening, the patient expired.

Examination of the body, thirty hours after death.

The exterior.—The muscles of the face convulsively, but *equally*, contracted; the countenance is red, the capillaries gorged with blood; the hymen perfect, and very apparent.

The head.—The cranium thick, injected, and very firm; the dura mater adherent to the skull about the right parietal fossa; the arachnoid healthy, but injected. The cerebral substance, of the ordinary aspect and consistence, presents nothing to be noticed, but a general injection of its blood-vessels, and a remarkably small capacity of the ventricles, which contain a little serum. The cerebellum is somewhat soft, though apparently healthy.

The spine, on its cavity being laid open in its whole extent, presents a very intense injection of its venous vessels; the medullary substance is the seat, about the dorsal region, of a species of disorganization similar to that found in the case of Mary Brisset: the pultaceous disorganization commences, superiorly, in the cervical region below the origin of the nervous plexus supplying the upper extremities, and it terminates inferiorly in the lumbar region; between these two points, the nervous mass is reduced to a fluid and yellowish porridge: in the rest of its extent, the spinal marrow is of the ordinary appearance.

The thorax.—The lungs are healthy; but the right one presents a cicatrix at about the middle of its costal surface.

The abdominal viscera presented nothing remarkable.

Observations on the Structure and Use of the Thyroid Glands in the Horse, and on some Diseases of that Animal. By JAMES WHITE, late Veterinary Surgeon of the 1st or Royal Dragoons; and Veterinary Surgeon to the Bath and West of England Agricultural Society.

IT had already been supposed, by several anatomists*—from analogy founded on its structure, situation, and the nature of the fluid found in its cells,—that the thyroid body is a mucous gland, and that its excretory ducts open into the larynx or trachea; but the truth of this inference had not hitherto been demonstrated by any positive evidence.† I have, how-

* See GALEN, *De usu Partium*; and MORGAGNI, *Advers. Anat.* t. iiii. for a full account of the opinions of anatomists on this subject up to the early part of the last century.

† CUVIER acknowledges (in his Lectures on Comparative Anatomy) that we are ignorant of its functions; and the only remarks he makes at all relative to this point tend to show that it exists exclusively in the *mammalia*, and is present in all the animals of this class excepting the *cetacea*. Its absence in animals of this family, which appear to be devoid of voice, has led to an inference that it is connected with this function; but this inference is somewhat opposed by the fact that it is present in the porpus, which is not known to possess the power of voice.

ever, satisfactorily ascertained, by means of injections with ink, that it is a mucous gland, and that its excretory ducts open into the larynx: the termination of them may be readily seen as minute papillary eminences, especially on the epiglottis and superior parts of the larynx. The excretory ducts are continued down over the rima glottidis into the trachea, especially at its posterior part, where the bronchial membrane is thrown off from the cartilages, and the intervening space is filled up with cellular membrane.

These excretory ducts communicate with the cells in the substance of the thyroid gland, (or, more properly speaking, *glands*; for it is obviously constituted of two glands united together by a band of dense cellular texture. The division is very evident in several of the ape tribe, and in bats the two are completely separate;) and hence it is, as I shall presently show, that diseases of this gland often lead to troublesome disorders of the respiratory organs.* The secretion of those glands sometimes becomes too abundant, and is also morbid in its qualities, which, being poured into the larynx, produces irritation there, and not unfrequently ulceration, especially in the edges of the rima of the glottis; and hence the worst kinds of *roaring* arise.

* There are several cases on record that seem to show that this is equally the case in regard to the human body. Before I notice these, I should remark that it is an extraordinary circumstance that the observations of BORDEU on this subject have been so little attended to by anatomists. Bordeu says (in his *Recherches Anatomiques sur les Glandes*, § xlii.) that the first ring of the trachea is always perforated by one, two, or three foramina, about the middle of its anterior surface. This was first observed by him on dissecting a larynx before a fire: on the thyroid gland, which was extremely large, being removed, he found the ring above mentioned almost osseous, but sufficiently diaphanous to permit him to perceive, by means of the fire, two foramina, which were covered by fine membranes, easily separable. These foramina, he felt satisfied, existed naturally in the cartilage: they were situate one on the side of the other, were of an oval shape, and of about a line in extent in their shortest diameter. After this, he always discovered openings of this kind, most ordinarily two, though sometimes one and sometimes three were found to be present. They are, he remarks, very different from the small irregular holes dispersed here and there in the trachea and thyroid cartilage; and he inferred, from their situation, that they were connected with the thyroid glands. That such was the case, he became more confident some time afterwards, on dissecting the body of a person who had suffered a violent death. On examining the internal surface of the trachea, in the situation above described, without having touched the thyroid gland, he found the membrane lining it full of small holes; and on passing bristles, lightly, through five of those openings, they assembled together, three on one side and two on the other; in the openings in the ring of the trachea above mentioned; and, continuing to urge them forwards, in a gentle manner, they passed into the substance of the thyroid gland. LAMURE, of Montpellier, was present on this occasion. The same results were witnessed in several experiments on other subjects. He also says, that the thyroid gland was inflated by blowing through those openings. I have been informed that Mr. COPLAND HUTCHISON (in a paper lately read to the Medical and Chirurgical Society, on the Cure of Bronchocele by Seton,) has stated that Mr. FOURRE has mentioned a communication between the larynx and this gland, which was inflated by blowing air into the larynx properly secured by ligatures; but, whether the inflation was produced through some communicating

The disordered secretion of the thyroid glands has appeared to originate from improper feeding, especially with bad hay, or such as is made too late,—that is, after the seed has formed. The tender shoots with which grass abounds at the proper period for mowing, and which are richly supplied with saccharine juices, become then tough and fibrous, and the saccharine juice that is not directed to the seed-stalks is converted into fecula and neutral salts. Such hay, then, abounds with materials for forming acrimonious secretions, and is greatly deficient in saccharine or nutritious matter. Besides this, such hay is apt to produce a depraved and inordinate appetite, by which the horse is led to eat to excess; and hence additional means are established for the production of disorders of the respiratory organs: for the secretion of the whole of the mucous membrane of the bronchial tubes and *air-cells*,—as they are improperly named, for they are really tubes, though capable of being dilated in certain parts by injections or quicksilver, and of being made to assume an appearance which has led to this erroneous term for them,—as well as of the thyroid gland, becomes too abundant, and probably also morbid in quality; and thus some are plugged up and obliterated, while others are distended and ruptured, and the air is effused into the cavity of the thorax. These consequences, with a debilitated state of the muscles of respiration, are the causes of those symptoms called *broken-wind*, which may be produced solely by the improper use of hay, and especially of bad hay. It is commonly immediately excited by the immoderate exertions to which the animal is impelled at a time when the stomach and bowels are loaded with food and excrements and the air-vessels replete with mucus.

Having noticed the ill qualities of bad hay, that is, hay made after the seed is formed, it may be expected that I shall say something of good hay, that is, hay which is mown when the grass is breaking out into flowers: (which is generally in the beginning of June:) such grass abounds with young and tender shoots, full of saccharine juices, and, when dried and properly fermented in the mow, it is of a green colour, and contains various grasses, commonly called herbage. This is the hay, and this only, which should be given to horses and cattle.

19, Vineyards, Bath; Feb. 7, 1821.

ducts, or that the air insinuated itself into the cellular substance of the part, does not appear. He recommends that examinations of the parts in cases of monstrosities and disease, should be made, as the structure of parts has sometimes been discovered by this means, when, from their minuteness, they, in the natural state, elude our researches. Borden, in regard to this view of the subject, mentions a case in which an abscess of this gland was suddenly evacuated through the trachea, without any previous disorder in the respiration of the patient; another, where mucous matter, in great abundance, was expectorated, and which appeared to come from the same source; and several others, tending to establish, more or less generally, the same inferences.

COLLECTANEA MEDICA:

CONSISTING OF

ANECDOTES, FACTS, EXTRACTS, ILLUSTRATIONS, &c.

*Relating to the History or the Art of Medicine, and the
Auxiliary Sciences.*

Floriferis ut apes in saltibus omnia libant,
Omnia nos itidem depascimur aurea dicta.

*Extracts of the Report from the Select Committee of the House of
Commons on the Doctrine of the Contagion of the Plague, in 1819.*

Jovis, 25^o die Martij, 1819.

SIR JOHN JACKSON, Baronet, in the Chair.

Sir ARTHUR BROOKE FAULKNER, called in; and Examined.

STATE the opportunities you have had of considering the plague, and the nature of contagious distempers in general, in the course of your practice?—The only opportunity I have had of seeing the plague was in the island of Malta.

On what occasion?—In the year 1813.

When the plague prevailed?—When the plague prevailed in the island.

What situation at the time did you hold?—I was physician to the forces. I was the only staff physician employed during the greater part of that service.

Was you there at the time it broke out first?—I was.

And attended as a medical man during the whole course of it?—Not during the whole course. I did not attend officially until the army became infected. I was not permitted when the natives only were infected.

But you was present?—I was present during the whole period of the plague.

And practised as one of the principal men of the army?—After the army became infected, I practised as physician to the forces.

What is your opinion respecting the mode in which the plague, in general, is generated and communicated?—I believe it is generated or produced by a contagion *sui generis*, quite peculiar and specific, and that it is communicated only by contact or close association with the person or thing infected.

What are your reasons for believing that it is communicated only by contact or association, and not by a certain state of the air?—My reasons are drawn from the course the plague took from its first entrance into the island of Malta, until its cessation. It was communicated in the first instance in the direct line of contact. It could be traced to have been propagated in the direct line of contact in the city of Valetta, and from the city into most of the *cassals* or villages, where any history could be obtained of its introduction.

Will you have the goodness to state the instances of its communication by contact, during your own experience at Malta, from your own knowledge?—The first case of the communication of the plague was, in my opinion, from a vessel, the *San Nicola*, in the harbour, to the family of a person of the name of Salvator Borg.

The vessel lay in what harbour?—It was lying in the harbour contiguous to the city of Valetta; the harbour is called Marsamuchetts.

State the circumstances?—To prove that it was from this vessel the infection was received, I shall crave permission to read a letter addressed by myself to the governor. I am not quite sure of the verbal accuracy of the letter. After the vessel arrived in the harbour in March, the whole town became extremely alarmed, and among the rest myself. I understood, (but this I cannot pretend to vouch for,) that several merchants had remonstrated against the vessel remaining in the harbour. Participating in the common alarm, I thought it my duty, though not called upon in my official situation, to represent the consequences that appeared inevitable in permitting the ship to lie there; and therefore, though not solicited, I communicated the following letter to the governor:

"10th April, 1813.

"Sir,—Although in offering the following observations relative to the means of preserving this garrison from the calamitous distemper with which it is threatened, I should not be altogether able to elude a charge of intrusion upon the province of the health-officer, yet, being the only physician to his Majesty's forces on this station, I trust your Excellency may be pleased to see that the contribution of my opinion at such a juncture (though it has not been called for), is not unjustifiably at variance with my duty. It was my intention to have had the honour of addressing your Excellency on this subject some time ago, and previous to the malady being reported to be so close in our vicinity. The melancholy fate of the surrounding countries which have lately fallen a prey to its ravages, left in my mind no doubt as to the expediency of such a proceeding. My purpose was, however, for the present over-ruled by certain of my friends, who could not be brought to believe that the evil was in the least likely to approach so near to ourselves. If credit can be placed in the report of the day, these expectations have proved illusory, and therefore I can no longer apprehend that my addressing your Excellency on this subject should be deemed improper. I shall consequently hasten to submit for your consideration a proposal, from which, obvious as it certainly is, I am led to think considerable advantage might still be derived for the protection of Malta against the introduction of plague. With this view I would suggest, that neither the harbour of Marsamuchetts, nor the island there usually allotted to quarantine, should, upon the present emergency, be accessible to any arrivals from suspected ports; and therefore that some more distant, yet commodious, place be sought out, which, after being insulated from all intercourse with the population, should exclusively be destined for the reception of all persons with any suspicion of the disease. Some information which I have lately received will not allow me to doubt, sir, that such a place may be found; and

that the vessels, cargoes and crews of infected ships, could be disposed of in such a secure manner as to allow of the least possible risk. Unless called upon, I will not trouble your Excellency with any detail of particulars by which my proposal might be carried into effect. Whether the apprehensions about the existence of plague in our vicinity be sufficiently founded or otherwise, I cannot resist conviction that the measure I have been proposing would, in time of such peril, have been at the first of much importance. Assuming as a fact that there exists no longer any reason to doubt that the disease has made its appearance in the neighbouring quarantine ground, the dangers arising from the extreme proximity of that situation to so many adjacent shores, and to so very populous a neighbourhood as this, are, in my humble judgment, too obvious to require comment; since the points of invasion by so formidable an enemy are thereby so much more multiplied. How awfully has the experience of other times informed us, that in this, and in similar situations of exposure, the plague has found its way, in defiance of the most vigilant regulations. It is this reflection which has chiefly prevailed with me to submit the above proposition to your Excellency; by the prompt adoption of which, it appears to me, the best chance would be afforded of checking the approaches of a calamity, which even a little delay might baffle the best efforts to oppose. I hope, sir, the urgency of the occasion will atone for any imperfections in this letter attendant upon haste and anxiety; and that you will believe me, with the most zealous concern for the interests of his Majesty's army and the people here committed to your governance, your Excellency's most obedient, &c.

" A. BROOKE FAULKNER,
" Physician to the Forces."

Such was the letter I had the honour of addressing to the commander in chief.

Go on with your detail?—My apprehension of the disease finding its way into Valetta, from the lazaretto and plague-ship, arose from my knowledge that the persons appointed as guardians were taken from the lowest part of the community, and paid, as it appeared to me, very inadequately, somewhere from 1s. 6d. to 2s. a-day; and having recollected, from reading the history of a plague that visited the same island in 1675, that it crept on shore from an infected vessel unobserved, I thought I was justified in entertaining the same fears on the present occasion. Accordingly, on the 16th of this month, six days after this letter was presented to his Excellency Lieut.-General Oaks, the first infected case occurred in the town of Valetta; it was the case of the daughter of Salvator Borg. She died with well-marked symptoms of the disease, I think on the 19th April. Two other persons of the same family died on the 2d May, all with well-marked symptoms of the disease.

Will you trace the communication between them and the vessel?—From the family of Borg it made its way in a direct line into the family of one Maria Agius, a school-mistress, who, together with others she immediately communicated with, were attacked by the plague; and all of whom (with some of her scholars, I heard,) were seized or pe-

rished with well-marked symptoms of the disease. I ought to revert to that part of your enquiry which is of most consequence, namely, the communication between the vessel and the town of Valetta. I hold it as hardly requiring proof that the disease should have found its way from an infected ship in the harbour, when I consider the apparent connexion between the cause and effect, arising out of the arrival of the vessel and the almost immediate verification of my prediction to the governor; and recollect, besides, that the island had not been infected for 136 years before. I consider these circumstances as conclusive. But, in the next place, some new linen was discovered in the house of Salvator Borg, which was confidently rumoured to have been brought from the infected vessel; and it was further stated, (but of this I have no certain authority,) that when the vessel returned to Alexandria, the infected place from whence it came, there were some bales missing.

Have you any reason to believe that the family infected, among whom the disease broke out, had direct communication with the ship; and what means have you of knowing it?—When I consider what appeared to me the imperfect state of the quarantine system at Malta, I can only say, I think it an event not improbable, that some of the family might have got goods from this vessel.

Can you state that any communication took place, and what, between the family of Borg and the family of Maria Agius, where the disorder next appeared?—The families of Maria Agius and Borg were intimately acquainted with each other, and she was constantly employed in relieving the afflictions of the latter when taken ill of the plague.

From the family of Agius could you trace the progress of contagion to any other family?—For my own part I dropped the inquiry there: the *fact* of contagion became so rapidly multiplied, that it appeared to me impossible to carry the investigation in a direct line further in that populous city; but I am in possession of documents furnished to me by one of the captains of the lazaretto himself, a man of strict integrity, and many years employed in that official situation, showing that the contagion made its way in a direct line from Valetta into most of the infected cassals or villages: these documents I can produce, if required. In the next place, in pursuing the course of the disease by contagion, I should beg leave to remark, as a very important circumstance, that the means of its communication to the small contiguous island of Gozo, at a late period of the calamity, can be distinctly made out. A man belonging to an infected family in one of the cassals made his escape with a box of clothes into a neighbouring cottage; it was speedily found out that he had escaped, and he was accordingly apprehended and sent to the lazaretto. On his enlargement from the lazaretto, he returned to his cottage where he took this box of clothes, that had never been suspected to be there, but had been concealed; and he hired a boat and carried this box of clothes to the island of Gozo. The first family infected on the island was the family at whose house he arrived, and to which place he carried the box of clothes. It was a marriage present, I understood; and a priest acquainted in the

family was one of the first victims; he died with well-marked symptoms of the plague. I have not had it in my power to trace the direct communication further.

Did the individual who conveyed the clothes take the plague; was he himself infected?—I am not prepared to answer that question, but I rather think he was. Many persons were not infected by the disease, that were taken from the very bosom of those families who all died of it.

Do you happen to know whether this person had communication personally with the individual who took the complaint, as well as by giving the goods?—He lived in the family; there was a marriage about to take place, or had taken place in it.

How long after he got this box of clothes was it before the disease broke out?—Within a short time.

What length of time were the clothes locked up in the box, between the time of their being infected and the box being opened, and the clothes given to the people in Gozo?—During the term of his quarantine, I believe somewhere about twenty days; I am not quite prepared to answer when it was opened. All I can say is, that the box was carried by this man to a cottage, and was concealed from those who went to take him to the lazaretto.

Do you know whether any person in the cottage where the box was deposited, took the plague?—I cannot answer that question.

Do you know whether there was any communication, any thing that could have conveyed the disorder from the ship *San Nicola* to the family, except the linen?—I am not aware of any thing; I rest my whole evidence on what I before specifically stated, namely, the tendency of the disease to propagate in a direct line, and the circumstance of my prediction being followed in four or five days by its consummation; and the collateral consideration of the island not having been infected with the plague for 130 years.

And you conceive that to have been a sufficient cause to account for the propagation of the plague?—I should think so. I beg to be understood in giving this evidence, that I was not present myself, and therefore I cannot speak with confidence as to the linen being found in Borg's house; I did not see the linen myself. As to the other circumstance of the disease propagating in a direct line to the cassals, I had the documents from one of the captains of the lazaretto.

You have stated that, in the family of Borg, and in that of Maria Agius the school-mistress, and also in the family in the island of Gozo, the different individuals died with well-marked symptoms of the plague: do you state the well-marked symptoms of the plague to have appeared, from your own individual knowledge, or from information derived from others?—From information derived from others. With respect to the first case, I had official information communicated by the head of my department; I received an official letter from him on the subject. With respect to Maria Agius, it was a fact so notoriously known, that I apprehend any evidence on this point would be unnecessary. With respect to the infection of the cassals in a direct line, my evidence, as I have stated above, rests on the written statement of

one of the captains of the lazaretto, employed in the service during the plague of Malta.

Is he a medical man?—It is not necessary he should be so to fill that office.

With respect to Gozo?—With respect to Gozo, I have the information of some respectable authority, but I do not recollect his name: it is, however, a well-known fact.

Then, in point of fact, not one of these cases in which you state the symptoms to have existed, was from your own personal observation?—Not one. I was not officially employed: I offered my services in the native hospitals; but they were not received, on this ground, that they might be wanted for the army.

Had you any intermittent or other fever at Malta, during the plague?—I had none under my own immediate eye or care; but I understood there were a few, and those very mild, principally occurring, or indeed altogether occurring, towards the autumn or the latter end of the year 1813; but, as I have not seen any of the cases, I can only speak from hearsay.

Did those instances occur before the plague broke out?—They had, I understood, been occasionally numerous in preceding years.

The instances you state to have prevailed before the plague broke out, were they the marsh fevers?—I am not aware that any had occurred the year before; i. e. I cannot specify myself, on my own authority, that any occurred the year before; but I understood that they were of frequent occurrence in every year.

Was the plague particularly prevalent in the places where the marsh fevers prevailed?—Not at all particularly prevalent where the marsh fevers were most generally produced.

Is there any similarity between the marsh-fever in Malta and the plague?—I never could trace any series of symptoms that could lead me, in the slightest degree, to suspect any identity between them; but I have known the plague to personate, in certain symptoms, almost every possible form of fever, and I have known it to be entirely free from every kind of fever: there is no certain type to which it can be affixed.

Do you mean to say that there is no distinct symptom that marks the plague as distinct from every other fever?—I do mean to say, that fever is not an essential attribute of the plague; it is frequently mortal where there is no fever. It is an extraordinarily anomalous disease, which defies (I should rather say, has almost defied,) definition. Dr. Cullen, I conceive, has defined it best; but even his definition is not a correct one. In fact, I may say, it has hitherto baffled nosologists scientifically to define it.

Are there not symptoms that distinguish the plague in a manner that cannot be mistaken?—There are symptoms which are sometimes called characteristic symptoms, but they are not constantly present. The most frequent and constant symptom was a peculiar cast of the eye, and a certain appearance of the tongue; the eye had the appearance described by Russel, of a muddy dull colour; I believe that to be the most frequent and most characteristic symptom of the plague. As to

buboes, carbuncles, and appearances on the skin, they are not constant. In many of the most fatal cases, the patient perished before the buboes or any other eruption made their appearance; many without any other appearance than that of the eye.

What was the state of the pulse?—The pulse had been felt occasionally, through a tobacco-leaf, and was in many instances extremely rapid. The fine for feeling a pulse was several days' quarantine; if any medical man had felt a pulse, he was subject to this quarantine.

Can you show that prompt separation has been effectual in securing persons from contagion?—I can, in many instances: the instances would be difficult to detail, they are so numerous.

Did the disease extend to Sicily, or the neighbouring islands?—It did not extend to Sicily, in consequence of the prompt precautions that were adopted: and, had there been the same at Malta, my persuasion is that the disease would have been resisted *in limine*.

Were quarantine-restrictions found effectual in resisting the plague at Malta?—Wherever proper quarantine restrictions were imposed with firmness, steadiness, and promptness, they seemed to be altogether effectual in preventing the extension of contagion; but the quarantine system seemed to me so extremely lax from the beginning, for several months, that it would have been next to impossible the disease should not have been widely disseminated through the island. I may enumerate as instances of this laxity in our quarantine system at Malta, that there was no complete census taken of the population, with a view of detecting cases of infection, until, I think, the 19th May, 1813. There was not a complete and sufficient corps of trusty guards until the month of August; the people were not shut up in their houses until, I think, the month of August: that is, not universally shut up; probably partially. It is notorious that contact constantly took place in the street, previous to the organization of this corps of guards, and the shutting up of the inhabitants in their houses. I have official documents to prove all these points I have been just stating.

Was the plague ever known to be received in the lazarettos?—It has been known. I have heard many instances related to me by the Maltese, and here is evidence of one: I have brought the title-page of a book, which represents a monument raised to the memory of a grand master for having arrested the disease.

In what year?—1743.

Do you know any thing of the introduction of the former plague that ravaged Malta, and when did it occur?—I beg to refer to a paper I published on the disease, during my engagement on the plague at Malta, which was communicated to the Edinburgh Medical and Surgical Journal, and published 1st April, 1814. The passage I wish to read to the Committee, is this: "It is somewhat remarkable, that the history of the introduction of the plague, when it made such great ravages on the last occasion on the island, about a century ago, was nearly similar to what is circulated of the present; being attributed to some linen brought from a Levant vessel by a Maltese shopkeeper, which, after producing the disease in all those who first came in contact with it, ultimately disseminated the malady throughout the whole population."

During the late plague at Malta in 1813, was the disease arrested when the quarantine system was rigidly acted upon?—It was; from the moment that an adequate and regularly-organized police was established, and the inhabitants shut up in their houses, and other strict measures of quarantine enforced, (which was the case at a very late period,) in the month of August, the plague did rapidly decline.

How was the disease stopped at last?—By what I have just stated; by the organization of a sufficient corps of trusty guards and police restrictions, and by shutting the inhabitants up in their houses. I could name a number of other circumstances connected with the means of preventing it.

How were the medical attendants preserved?—With respect to the military hospital, of which I can speak from experience, the hospital in which I attended, (the pest-hospital,) they were, in my opinion, preserved by wearing a dress of oiled silk, which prevented the possibility of any contact of infected matter with the skin, and probably also by its promoting free and copious perspiration, and in consequence preventing absorption.

When was the plague stopped; at what time?—I am not quite sure whether it was in December.

Can you state when it began to decline?—It began before the month of August.

What do you conceive the hottest period of the year at Malta?—I am not prepared to answer that question; I believe the month of August. I have kept a thermometrical table; but, having it only for one year, the results of my observations are probably insufficient.

What was the temperature of the atmosphere at the period when you conceive the decline to have been apparent?—The 16th July appears to be the day when the plague was at its height; sixty-seven died that day; the thermometer was at 81 at four o'clock in the afternoon; in the morning at six o'clock at 77, and at ten at 81½. The next day, thirty-six died; the thermometer was at 82, and in one part of the day at 83. On the 18th of the month, fifty died; the thermometer was in the course of the day at 81. On the 29th, forty-one died; the thermometer was at 79. On the 20th of the month, forty-three died.

State the period at which there was a sensible decrease?—I think from the 16th of July there was an average decrease, though a very irregular kind of decrease; but the thermometer was rather higher than lower.

How far was the establishment of the police precautions to which you have alluded coincident with the decrease of the plague, with the visible decrease of the plague?—This question requires a cautious answer. The gentleman appointed at the head of the police begun to organize his system on the 3d July; the disease extended its ravages after he had organized his system in some degree; but it was not perfectly organized till the 2d August,—*i. e.* it did not shut the people up in their houses, nor was there an absolute prohibition of contact; but, as soon as it did enjoin absolute prohibition of contact, and shutting the people up in their houses, the disease declined.

Then it appears the decrease was not very visible till these precautions were put strictly in force?—Not so visible.

State, from the period at which they were strictly enforced, namely, the 2d August, what was the average decrease from that time?—On the 2d August, 50 persons died; on the 3d August, 48; on the 4th, 27; on the 5th, 47; on the 6th, 43; on the 7th, 35; on the 8th, 37; on the 9th, 24; on the 10th, 26; on the 11th, 28; on the 12th, 26; on the 13th, 31; on the 14th,—

Instead of putting the result of every day, look at your table, and see when the decrease became considerable?—I have brought it down since the 16th July until 14th August, to less than one-half; I have brought it down to 31 instead of 67, in less than a month.

From the 16th August did it go on gradually decreasing, till it disappeared?—It went on decreasing on the average, but not regularly.

At what period did it finally disappear?—On the 19th October: in my note of this day is stated the last occurrence of a case of plague in Valetta.

Did it continue in other parts?—In some of the cassals for a considerable time.

Have you any doubt whatever, that the decline of the complaint was produced by the prohibition of intercourse among the patients?—I feel satisfied that it was very much owing to the prompt measures of police; and my reason is, that the thermometer rose inconsiderably in point of fact, while the disease was decreasing fast.

Is there any other cause to which you can attribute the decrease and cessation of the plague?—I really do not see any other cause.

In your observation of the plague, has it appeared to you that it subsists only in a given temperature, neither in very great heat or very great cold?—During my residence at Malta, it did not appear to me that the temperature of the air had any thing to do with it. In my own opinion, I do believe that a very high or a very low temperature would check it.

Will you state the opinion you entertain, from the best sources derived, what degree of heat is not consistent with the plague?—I can only speak from my reading on that point. I believe that materially below 60, or probably at 60 degrees of heat, it cannot subsist; but it is bare conjecture.

You had never any opportunity of observing the plague except at Malta?—No.

Was there any thing remarkable in the state of the thermometer at the time the plague broke out at Malta?—Nothing, I believe.

What was the temperature at that time?—The thermometer, on the day the first case was reported to have taken place, was at 64; that was on the 16th April.

What was the state of health in the island before the plague broke out?—To the best of my knowledge, nothing remarkable. If there was any thing remarkable with regard to its climate, it was that there was nothing very remarkable, for the people were wondering that there was nothing remarkable in the state of the air to produce plague:

there was nothing anterior to the breaking out of the plague at all leading to any reason why it should exist, unless by contagion.

At what time did the first case of plague appear among the soldiers?—Without referring to my official letter, I cannot exactly state.

What month was it?—In June.

Was the soldier who was first attacked with plague, in barracks or quartered in the town?—I shall be under the necessity of consulting my medical register; but I have no doubt he was in barracks.

Could any communication, personal communication, be traced between the soldier first infected and any other soldiers who were afterwards infected?—I do not know that any had been attempted to be traced; for, when soldiers live in the gregarious manner they do, it would be in vain to make the inquiry.

Were the soldiers kept within their barracks previous to the appearance of the plague among them, and their communication with the inhabitants prevented?—In some of the barracks it was prevented, in some not; and it is material, in order to prove the contagious property of the disease, that, in those barracks where a strict quarantine system appeared to be kept up, the plague was excluded, though they were in an unhealthy part of the town; whereas, in other places that were more elevated and airy, but where there seemed not to be the same precautions observed, the disease was brought in.

Then the soldier first infected was in one of those barracks that had a more free communication with the town: had you the charge of the sick in the barrack in which the first case occurred?—I had partly the charge of them in this barrack, but not in the first case.

When did your charge of the plague patients commence?—It was relative to that point I wished to consult my register; but, as far as I can state with confidence, I was not employed officially to prescribe till De Rolle's regiment was infected. I think I was called in to see some other cases, but I had them not under my charge.

Were you yourself, in attending the sick, those who were ill of the plague, in contact with them?—Personally close to them, as nearly as it was necessary for me to approach them.

Had you the plague?—I believe not; though I have some doubt respecting that.

Who were the persons employed under you in the care of the sick?—Orderlies and such like.

Did any of them catch the plague?—Not one.

Were they necessarily in contact with the individuals who had the plague, and with their clothes and bedding?—Necessarily.

Were any precautions taken with the view of preventing them from catching the plague in the discharge of their duties?—Oiled-silk dresses were enjoined, by command, to be worn by every person in attendance about the sick.

Was it complied with?—It was, I believe, in point of fact. They were also enjoined a prompt ablution after touching the infected; they were obliged to wash their hands. In short, every means were adopted to prevent their catching the infection, with the addition of ablutions.

Was one of the precautions rubbing the body with oil?—It was; but

there are the best reasons for supposing that it had no share in preventing the infection.

What reason?—It had been employed with all the attention possible in the garrison, but yet the disease made its way. It had been employed by those who attended in carrying out the dead, and who, I believe, almost all perished: there were very few instances of such persons who did not perish.

When you describe the plague as contagious, do you mean that it is communicated by the breath?—I am not prepared to say whether it may not, by closely inspiring the breath of an infected person; but this, in my judgment, is a kind of contact. My opinion is, that it is principally communicable by the touch, but I think it can be communicated in the former way also: few would be hardy enough to try the experiment.

If it can be communicated by the breath, how can wearing oil-skin dresses or ablution prevent the breath of the infected persons getting into the mouth or nostrils from the infected?—We know that, at a certain distance in other diseases, the contagion from fomites may be so diluted by the atmosphere as to become innoxious.

Was the progress of the plague among the troops rapid?—It was not rapid.

Not so rapid as among the inhabitants?—Not at all: in all, not above twenty, I believe, died up to the middle of October.

The progress of the disease, I mean?—In some it was rapid, and in some it was not; there was every variety.

What was the per centage of those in garrison attacked?—I am not prepared to say.

Was it a greater or less proportion than the inhabitants?—A less proportion.

The precautions applied to the troops were more rigid than those that could be applied to the population?—Certainly. I was going on to state some circumstances as to the degrees of precaution used in each of the military barracks. The Sicilian regiment, though situated in a very infected part of the island, a place called Florian, escaped by the promptness and vigilance of Colonel Rivarolla. De Rolle's regiment, which was in the healthiest spot, was invaded by the disease, and evidently, in my opinion, in consequence of their barrier admitting a contact with persons on the outside. It was a barrier at which you could shake hands with any body on the outside. In the 14th regiment, which was near the most unhealthy part of the town, there was but one person suspected, and his disease was immediately arrested. The public prison, and public general hospital, escaped. The convents in Valetta escaped, with the exception, I believe, of one; and the introduction of the disease to that one was accounted for. The prison and these public institutions escaped, I conceive, very much by the voluntary attention paid by their inhabitants to a strict system of quarantine.

Did the plague cease in the military hospitals before it ceased in the town?—I am not prepared to answer that question.

Were the soldiers in barracks prevented from holding communica-

tion with the town, after the plague had ceased in the barracks?—I believe so: that would greatly depend upon the commanding officer.

I ask you whether, in point of fact, they were?—Throughout the whole, they were interdicted as far as possible; the commanders of regiments issued orders to prohibit intercourse, but they were not strictly obeyed.

Did the plague find its way into any of the barracks or regiments where these orders were not strictly observed?—It got into De Rolle's regiment particularly.

You have stated that these three cases of Salvator Borg, Agius, and Gozo, were cases you received from information. Now, I observe, in many cases that came under your own knowledge, it did not communicate to persons. Do you not consider that the natural time of the decrease of the plague, is the fall of the year?—It is my own opinion that it would cease if the temperature was low: I think that the plague is incompatible with a certain temperature, high or low.

Then it is not the particular period of the year?—No.

You stated it to appear prior to August, but the restriction was not till the 2d of August?—Not in its full rigour, but an improved system was acted upon in July.

The ship *Nicola* returned to Alexandria with her whole cargo, did she not?—If I may except those bales which were rumoured to be missing.

She was not allowed to land her cargo?—No.

Who navigated her back?—Probably the remaining part of the crew.

Was she not navigated back by Maltese?—She might.

Do you know whether they took the plague?—They arrived in safety.

Did they who assisted in landing the cargo?—I believe so.

Are there any eruptions in the skin, in the plague?—If we can call eruptions, what are termed blains and carbuncles.

Do you consider a bubo or carbuncle to be an eruption?—I should think so; it is freely applicable to the term: its etymology is from *erumpo*, which is to break forth.

Does not eruption mean a cluster of pimples?—In common acceptation it may.

Do you think the plague comes under the description of Exanthemata?—I think it does. I believe that Dr. Cullen has given the best account of the disease.

You have stated that the contagion in plague is what is called *suu generis*; do you consider it different from contagion in the small-pox?—Different from every other known contagion.

Have you ever heard of the plague in England?—I have read of it.

In what year?—The last was in 1665.

Have you ever heard of plague since?—Never, as imported into England.

Do you consider the plague can be propagated from goods as well as persons?—I think so.

Have you any reason for thinking why the plague has not been in-

introduced from goods in the quarantine establishments?—In the first place, quarantine restrictions since 1709 have been a great deal more rigid; indeed, they did not exist in England at all previous to that period. I conceive that the intensity of the contagion may have been greatly blunted by the length of the voyage, and the length of time that passes after the shipment of goods. Besides, we know that other countries have a good system of quarantine, which is in favour of the plague not being imported here.

Do you consider the plague of 1665 to be the true Levant plague?—From the description I have read of it, I am inclined to think so.

Do you consider the reason why the plague has not been introduced in England, has been more from the length of time in the voyage, than the quarantine establishment?—I mean both conjunctively,—viz. the time and the means used to free goods from contagion and to expurgate infection.

How do you account for the expurgators never having taken the plague?—I cannot account for that, but by collateral considerations.

What are those?—1st. That we have observed, in other countries, the disease has not taken place for a long series of years; not for 130 years in Malta. 2dly. We do not know what the circumstances are that constitute aptitude in the receiver, sufficiently, to know why the plague has not been received into the lazarettos since 1664. But, 3dly, it does not follow, because it has not been received into the lazarettos since 1665, that it may not, by some fortuitous concurrence of circumstances, occur again here.

Are you acquainted with the opinion of the ancients respecting the plague?—I am. It has been stated that the ancients were not acquainted with contagion, but I can adduce instances, from the medical writers and the poets, to the contrary; I can produce instances from both the Greek and Roman writers and poets.

Does Hippocrates mention it?—He does not. He had not the experience to determine the point; but, with respect to the authorities that do speak of contagion, I shall beg to refer to the following, viz.

Συνδράρχειν τοῖς λοιμώτευσιν ἐπισφαλες ἀπολαύσαι γὰρ κίνδυνος, ὥσπερ ψάρας τινοῦ ἢ ὀφθαλμίας.—GALEN, lib. i. ch. 2. *de different. Februm.*

Διὰ τί ἀπὸ μὲν νόσων ἐνὶ ἡμῶν κοῦνται οἱ πλεσιάζοντες, ἀπὸ δὲ ὑγιείας ἐνδεῖς ὑγιαίνειν.—ARISTOTLE, *Prob.* lect. vii. 1.

Δίος δι' ἐμβριῶν τε, καὶ ἐνδιαττασθαι, ἢ μᾶλλον ἢ λοιμῶν ἀναπνοῆς γὰρ ἐς μετέδοσιν, ῥηὶ δὲ βαφῇ.—ARETÆUS *de Elephantias.*

Infecti quasi valitudine et contactu.—*Annal.* TACIT. b. 6 and 7.

Postea curatio ipsa et contactus ægrorum, vulgabat morbos.—*Liv.* 25 and 26.

Contagion is clearly expressed in the last eight lines of the third *Georgic* of VIRGIL; likewise in the first *Bucolic*, verse 52; where these words occur:

Nec mala vicini pecoris contagia lædent.

There are numerous other authorities,

CRITICAL ANALYSES

OF

RECENT PUBLICATIONS, IN THE DIFFERENT BRANCHES OF MEDICINE AND SURGERY.

"I would have men know, that, though I reprehend the easie passing over of the causes of things
"by ascribing them to secret and hidden vertues and properties; (for this hath arrested and laid
"asleepe all true enquiry and indications;) yet I doe not understand but that, in the practical
"part of knowledge, much will be left to experience and probation, whereunto indication cannot
"so fully reach; and this not only in *specie*, but in *individuo*. Yet it was well said, *Vere scire
"esse per causas scire.*"—BACON.

*History and Method of Cure of the various Species of Palsy: being
the first Part of the second Volume of a Treatise on Nervous Dis-
eases.* By JOHN COOKE, M.D. F.A.S.; Fellow of the Royal College
of Physicians; and late Physician to the London Hospital. 8vo.
pp. 215. Longman and Co. London, 1821.

THE intentions of the author in the publication of this series
of Treatises on Nervous Diseases, were stated by us in the
review of that on Apoplexy; and the judgment we then formed
of the value of the work, has certainly not been considered too
favourable on further consideration. We have ourselves found
the preceding volume of much utility, for the purposes of præ-
tice as well as of pathological inquiry; and, to students espe-
cially, there cannot be a doubt but that it will prove eminently
useful. The most important part of the work is the summary
it furnishes of the best pathological principles and therapeutical
precepts respecting the subjects of it, that prevail in the pre-
sent day: but the historical view of those points presents mat-
ter by no means devoid of interest to those who desire to
contribute to the improvement of medicine. The history of
this science displays a region far too extensive for the investi-
gation of persons of ordinary habits of inquiry, and unless a
man is acquainted with what has already been done and
thought, he is likely, if he attempts to attain any thing that is
new, to employ himself in researches which have already pro-
duced every thing of which they are capable; to frame hypo-
theses which have already been entertained, and overthrown by
the opposition of facts of which he is ignorant; or, at much
expense of time and intellectual labour, to arrive at conclu-
sions which he might have seen demonstrated in a few words by
some extant author. Had (for example) some of our modern
physiologists known merely what is stated in the writings of
GALEN, they could have saved years of fruitless labour that might
have been otherwise advantageously exerted. It is not, however,
a mere train of unconnected notions, such as constitute the ar-
ticles in the generality of *Dictionaries*, and even a considerable

proportion of those of one of such pretensions as the *Dictionnaire des Sciences Medicales*; it is a perspicuous and methodic, or (to use a metaphoric expression,) a digested, account of the facts and opinions promulgated in former times that is qualified to be of the utility above indicated: there is as much diversity in the value of the two, as well as in the talents requisite for their construction, as there is between such a use of extensive erudition as is manifested in the works of KAIMS, and the quotation of at least a score of authors in every page, without which the writers of a continental nation think their books would not make a respectable appearance, any more than their professors would without their chains and ermine. The application of those remarks will be perceived on a reference to the works under consideration. The present volume is, however, even better constructed than the preceding one: there is in it a greater proportion of original reflection; and a more abstract character is given to the citations, by which the conciseness of the work is increased, whilst its utility is not lessened.

In the first chapter the author treats of the "Definition, Distinction, and general History," of Palsy. He shows that the ancients very generally considered apoplexy and palsy as diseases of the same nature, but different in degree: apoplexy being an universal palsy, or palsy a partial apoplexy; and hence, as we stated in our review of the preceding Treatise, HIPPOCRATES speaks of apoplexy in the leg, &c. After having cited the definitions of palsy given by ARETÆUS, BOERHAAVE, CULLEN, YOUNG, and GOOD, Dr. Cooke proposes the following one. "It is a disease in which there is a diminution, or an entire loss, of the power of voluntary motion, or of sensation, or of both, in some particular part or parts of the body, without coma." A concise account is next adduced of the precursory phenomena, and then the symptoms are thus detailed in a general way.

"Palsy chiefly consists in the loss of the power of voluntary motion, for sensation, in a greater or less degree, generally remains; nay, in certain cases it is morbidly increased. I have seen several instances in which paralytic persons have felt very violent pain in the parts affected, particularly in the shoulder and arm. These remarks might be confirmed by quotations from various authors. I never saw a case of palsy in which sensation was entirely lost; and an eminent physician of great experience asserts that a total loss of feeling in this disease is extremely rare. The other senses are often but little injured; sometimes they remain wholly unimpaired, and several instances might be adduced in which they appeared to be preternaturally acute. Dr. Heberden attended a paralytic person, whose sense of smelling became so exquisite as to furnish perpetual occasions of disgust and uneasiness; and he mentions one case in which *all* the senses became exceedingly acute.

"The vital and natural functions in palsy are generally but little affected. The actions of the heart and lungs are indeed sometimes more languid, and the secretions and excretions less regular than in a state of health; but this is not usually the case."

It has generally been considered that a diminution of temperature of the affected parts, is a necessary consequence of paralysis; and it has been supposed that this effect has arisen from less heat being developed in them from want of due nervous influence. Another explanation of this phenomenon has been proposed by Dr. ABERCROMBIE: he is inclined to believe that paralytic parts do not necessarily become colder than natural, but that the variation of temperature in them arises from their having lost the power of regulating their temperature; so that they *cool* when exposed to a temperature less than that of the body, and *become heated* when exposed to one above it, more readily than healthy parts. He says, according to Dr. Cooke's statement, that he had long ago observed that paralytic limbs are sometimes warmer than sound limbs, but without being able to account for it; and he relates, in support of his views of this subject, an instance in which "a medical gentleman, on visiting a paralytic patient, was astonished to find the paralytic arm so intensely hot that he could not touch it. He was at first very much surprised, but found, upon inquiry, that the patient had, by the advice of a friend, applied to the arm a quantity of very hot bran, or something of that kind very hot, which had been removed a short time before his visit."

Dr. Cooke remarks on this point, that, "if Dr. Abercrombie's notion be correct, that paralytic limbs lose the power of preserving their temperature; or, in other words, if their power of resisting changes of temperature be lost; it appears to me that the temperature of such parts would be less than that of other parts exposed to a medium of heat inferior to that of the human body; which is always the case in temperate climates."

The other remarkable phenomena in the affected parts, are the wasting of them, and the sense of formication often experienced. After having discussed these points, Dr. Cooke adverts to the depression, imbecility, and sometimes almost annihilation, of the intellect; and he refers to several of the curious cases on record, in which the memory especially has been defective. Some persons have, indeed, not preserved the consciousness of self-identity; and it is a curious circumstance that the forgetfulness has especially existed in regard to nouns substantive and the knowledge of languages. In many cases of this kind, the patients have only been unable to pronounce the words, whilst they have remembered the names of things, and could, in some instances, repeat the letters of a name distinctly. This was the case with Professor BROUSSONNET: he could not

pronounce the name of his daughter, after an attack of palsy; but he could articulate, in proper order, the letters of which it was constituted.

Dr. Cooke says, "In cases of persons recovering from palsy, I have often observed that the parts most distant from the head are first restored to sense and motion. In hemiplegia, it almost always happens that the power of the leg returns long before that of the arm. I have even seen more than one case, in which the arm of the affected side has remained paralytic for several years after the restoration of the leg." Similar remarks have been made by others, and the facts they indicate are worthy of the consideration of physiologists. The restoration of the power of sense and motion is, we believe, evinced in the same way in paraplegia: it was so in the case related in the last Number of this Journal, by Dr. VENTURI.

A very interesting fact, relating to this subject, has just been mentioned to us by Dr. HARRISON. A boy had had nearly complete paralysis of one leg for nine years, from a curvature of the lumbar portion of the spine: as far as related to motion, he could only throw the limb a little backwards, and had not the least power of motion of the toes. The distortion of the spine was almost wholly removed immediately, by one operation, and the boy could *instantly* afterwards move his toes. He is gradually regaining the power of motion in the muscles of the limb generally.

Dr. Cooke treats, in the next instance, of the *distinction* of paralysis: he adopts that of Cullen, into hemiplegia, paraplegia, and partialis. He thinks the addition of the species *venenata* objectionable, as well as the others founded, agreeably to the system of SAUVAGES, on supposed causes of the disease.

The second chapter treats expressly of the history of Hemiplegia. PAULUS ÆGINETA, Dr. Cooke says, seems to have been the first who applied this term to paralysis affecting one side of the body. It is, he continues, "in a great proportion of cases, preceded by an apoplectic fit, which is sometimes so slight and transient as to have escaped general notice; but the attentive observer will almost always perceive certain symptoms indicative of the stroke,—particularly distortion of the muscles of the mouth, drowsiness, forgetfulness, and dullness of apprehension, in a greater or less degree." The stomach and bowels have been generally supposed to be in a torpid state in this disease, chiefly from their apparent insensibility to the agency of medicines, and the costiveness which accompanies the disease; but Dr. Cooke remarks that this torpor may be merely apparent, and that "it would appear, from the experiments of Magendie, and the observations of

other physicians, who have found inflammation of the intestinal tube produced by ordinary purgatives under these circumstances, that the stomach and bowels are, in fact, particularly irritable; vomiting and purging not taking place from such medicines, merely because the action of the muscles, necessarily for those functions, cannot be excited." These phenomena are more frequently obvious in apoplexy than in hemiplegia simply: it is but rarely that the abdominal muscles are not excited to action, in the latter affection, by irritation of the bowels. Retention of urine in apoplexy, is often witnessed from the same causes. It is true that involuntary evacuations of the feces and urine often happen in paraplegia, and sometimes in apoplexy: but here the sphincters of the rectum and bladder have lost their powers, and the contraction of the intestines and bladder, respectively, are then sufficient alone to effect the expulsion of their contents, which is not the case when the sphincters exert their ordinary functions: here the concurrence of action of voluntary muscles is requisite.

HOFFMANN, from having seen enlargement of the liver with hemiplegia of the right side, hypothetically inferred that the viscera of the diseased side are disposed to become affected because they borrow so many branches from the external nerves. The influence of the cerebral or spinal nerves on secretion or the circulation,—it does not seem easy to determine which,—is shown in a remarkable manner by a case cited by MORGAGNI, (from the *Ephem. Nat. Curios.* c. 3, obs. 64:) it was that of an old man who was affected with palsy in the right side, and at the same time with jaundice; the jaundice being confined to the paralytic side so accurately, that even the right part of the nose was yellow, whilst the left retained its natural colour. Morgagni, although his alternatives in the way of explanation are generally so numerous, could only remark on this curious case,—“*quando idem flavum sanguinis serum non minus per sinistrum latus, quam per dexterum; nisi forte credas, per laxiores hujus fibras lentius promotum magis inficere potuisse.*”

The palsy which follows apoplexy, Dr. Cooke says, is generally a complete hemiplegia: but there are many anomalies in this disease, some of the most remarkable of which are adduced. FABRICIUS speaks of palsy in one arm and in the foot of the opposite side; and RAMAZZINI, HEISTER, and SENAC, of loss of feeling only in one leg, and loss of motion only in the other. Sauvages enumerates, amongst the species of hemiplegia, one which he denominates intermittens: “It is that hemiplegia,” he observes, “which comes on every day, and, after some hours, recedes with an accession of quotidian fever.” Loss of sense only, and of motion only, in individual instances, are not very rare. Dr. Cooke concludes his citations of this

kind with a citation, from the seventh volume of the *Medico-Chirurgical Transactions*, of the cases of DE SAUSSAURE and VIEUSSEUX; that related by Mr. KERATRY, (which was inserted in a late Number of this Journal;) and an extraordinary case, that occurred to his own observation.

"An officer of high rank in the army, who is now about sixty years of age, was, in the year 1795, affected with a diminution of power in the right hand. This complaint increased, notwithstanding a variety of modes of treatment, till the year 1800; when, after a course of mercury, recommended by Mr. Cline, its further progress was stopped, since which time the disease has remained stationary. The peculiar circumstances of this case are the following: The muscles of the left arm, from the shoulder to the elbow, are much wasted, and greatly diminished in power; while the muscles of the fore-arm are not at all diminished in size, and but little in power. The state of the right side is just the reverse: the muscles of the upper arm being of their natural size, and possessing their full power; whilst those of the fore-arm are very much wasted, and their motion, especially that of the fingers, almost entirely abolished. In all other respects, this gentleman appears to be perfectly well. No cause for this disease can be assigned; nor did any method of treatment afford the smallest relief, till the mercurial course was adopted, when the progress of the disorder was arrested in the year above mentioned. Since that time no attempts to remove this complaint have been made, yet it does not increase."

After having described the ordinary immediate consequences and final termination of hemiplegia, the author treats of Paraplegia. VAN SWIETEN remarks that it was the custom, in his time, in the medical schools, to call that disease paraplegia, in which voluntary motion ceases in all parts below the neck. Dr. GOOD applies the term to paralysis "of the lower half of the body on both sides;" and it seems to have been very commonly used in this sense by modern writers. It is but rarely that cases corresponding with the definition alluded to by Van Swieten, are really witnessed; for an injury seated so high in the spinal marrow as to produce such an affection, generally rapidly destroys life, by its influence on the function of respiration. In one case which we witnessed, where the spinal marrow had been much injured about the junction of the second and third cervical vertebræ, by a fall, the patient lived two days, dilating his chest, for inspiration, only by the intercostal, scaleni, and other muscles attached to its superior part; the diaphragm being, apparently, quiescent. Dr. Cooke mentions the accuracy of the knowledge of GALEN, of the influence of lesions in various parts of the spinal marrow on the rest of the body, and the proofs he gives of his acquaintance with paraplegia from disease originally of the parts constituting the spinal column.

Some erroneous views seem to have been taken of the seat of the cause of paraplegia, from disease of the brain having been found in cases where it has existed, without its having been considered that the real cause of the paraplegia might have been in the spinal marrow, whilst the cerebral disease was merely a casual contingency; as Dr. Harrison argues in a late paper inserted in this Journal. This chapter terminates with a concise account of a case of that remarkable form of disease referred to by SAUVAGES, in his Nosology, article *Scelotyrbe festinans*. The case above alluded to first appeared as an occasional paroxysm of an inability to walk slowly or to stand still, though, if the patient set out and run, he could proceed to some distance without falling to the ground. There was evidently no affection of the brain in this case; for there was no vertigo or disturbance of the senses, and the power of motion in the arms was not diminished; so that, when the fits came on, (which they usually did after the patient, a man forty-five years of age, had walked for two or three miles,) in situations where the patient could grasp with his hands any thing sufficiently firm, he could support himself upright. When they occurred in such a place as an open field, he used to quicken his pace gradually to the most rapid walk, then to that of running, which became quicker and quicker until he arrived at some resting-place, or, if this were distant above three or four hundred yards, until he fell to the ground. A sense of weight in the lower limbs succeeded these paroxysms, and they had been preceded, for two or three years, by uneasy feelings about the loins and sacrum, leading the patient to suppose he had hemorrhoids, though no evidence of them could be perceived on examination. The case degenerated into a constant diminution of the power of motion, and, at the end of four years, purulent matter formed about the sacrum; the paraplegia became complete, and the patient soon afterwards died.

CUVIER seems to have explained the most remarkable phenomena proper to this affection, when he says that "several quadrupeds, with whom standing on two feet is very difficult, can nevertheless walk thus for a certain length of time with tolerable facility, because, in general, the act of walking is much less difficult than that of station; the same muscles, in the former, not being in so constant a state of contraction; and because it is easier to correct the vacillations by other contrary and alternative vacillations, as may be done in walking, than to prevent them entirely." We find, too, that a drunken man can keep on his feet whilst he is staggering forwards, though he would fall down if he were to attempt to stand still.

The fourth chapter treats of Partial Paralysis, that is, palsy "which affects less than half the body, or some one particular

part or organ." This species includes, in conformity with the author's definition of palsy, diseases of organs that consist in a loss either of sensation or of motion only,—as paralytic affections of the nerves of sight, hearing, &c. and want of power of motion in the eyes, loss of speech, &c.

Cases of paralysis of one or more muscles in various other parts of the body, without any obvious lesion from which they may originate, are not unfrequently witnessed. After having noticed these varieties of this species of disease, Dr. Cooke enters into a discussion of the question how it happens that sensation and the power of motion are not both lost, if one of them be lost, "since they both depend upon the nerves?" More modern physiologists have done nothing more than adopt the explanation of Erasistratus and Galen on this point, which Dr. Cooke seems to think, himself, is the most plausible one that has hitherto been proposed.

The fifth chapter treats of the Causes of Palsy. "The chief *predisposing* causes of general palsy," the author says, "are those which I have enumerated and explained in the first volume of this work, as the predisposing causes of apoplexy; such as advanced age, hereditary feeble constitution, and especially a leucophlegmatic, pituitous, or dropsical habit." The *exciting* causes of palsy also resemble those of apoplexy. After having discussed those points, the author examines the hypothesis of Dr. SERRES, contained in his paper in the *Annuaire Medico-Chirurgical*. Dr. Cooke thinks that "the conclusions which he [Dr. Serres] has drawn from his reasoning, are too general, and by no means strictly logical;" and he shows that they are "controverted by facts which lead to positive conclusions, not to negative conclusions," as those of Dr. Serres are. We need not follow the author through his examinations of this subject, and his discussions respecting the seats of the causes of paraplegia and partial paralysis.

The subject next noticed is the circumstance that the palsy in hemiplegia generally affects the opposite side to that in which the apparent cause of it exists in the brain. On this point, too, Dr. Serres evinces his injudicious generalization, and asserts that the paralysis is always on the opposite side to that of the disease in the brain. Of the incorrectness of this assertion, we had the most satisfactory proof a few days since. A girl, eleven years of age, who had been a patient of the Welbeck-street Dispensary, under the care of Dr. OUTRAM, had paralysis on the right side only, for six weeks, when she came under our care. Her death took place at the end of about four weeks longer. Two large cavities, containing coagulated blood, considerably altered in its appearance, were

found in the right hemisphere of the cerebrum; whilst the left hemisphere was devoid of the slightest appearance of disease.

It has generally been found, in paralysis from disease of the spinal marrow, that the palsy has been on the same side as that affected, when the medulla has been injured on one side only; but that this, too, is not universally the case, is shown by a case quoted from PORTAL by the author.

The explanations given of the phenomena above alluded to, in hemiplegia, by ARETÆUS, (that it arises from a crossing of the nerves at their origin;) LANCISI, (from a decussation of the fibres in the corpus callosum;) SOEMMERING and HALZER, (from a crossing of the fibres of the brain immediately below the origin of the lingual nerves;) Dr. YELLOLY, (who seems to think that SANTORINI is most correct, in considering that the supposed decussation is in the tuberculum annulare, whilst he refutes the inference of GALL on this point;) are passed in review; and the author concludes his considerations on this subject by remarking, that "notwithstanding the observations and reasonings of anatomists and physiologists on this subject, much obscurity remains; yet, on the whole, I think it seems more probable that a decussation of nerves takes place in the tuberculum annulare than in any other part. If the minute structure of the brain were better developed,—if it could be shown to consist of converging fibres,—we might better understand how injuries done to one side of the brain, especially in the higher parts of the hemispheres, might produce palsy on the opposite side of the body; but, though such a fibrous structure of the brain has been supposed to have been seen by Leuwenhoek, Bidloo, Cowper, Gall and Spurzheim, and others, its existence has not been satisfactorily proved."

The sixth chapter treats of Dissections, Diagnosis, and Prognosis. This is constituted, as regards the first subject, of accounts of the observations of BONETUS, LIEUTAUD, WILLIS, MORGAGNI, Dr. ABERCROMBIE, Dr. SERRES, ROUCHOUX, RIOBE, and PORTAL. The only novel observations adduced are these of Mr. CHARLES BELL, who says, in relation to the alterations that take place in nerves in parts affected with palsy, that "nerves, if not employed, degenerate into a sort of cellular membrane."

After passing in review the remarks of former authors respecting the prognosis, Dr. Cooke says,

"As far as my own experience enables me to judge, the prognosis in the general palsies must be almost always unfavourable. I have seen many cases of recovery from palsy in a very considerable degree; but I do not recollect more than one or two cases, of a complete restoration, both of sensation and motion, in the whole of the side of a person who had been affected with a perfect hemiplegia. When this

species of palsy depends upon an injury done to one side of the brain which is almost always the case, I am inclined to think that the mischief is seldom, if ever, entirely obliterated, and the disease wholly removed. On the dissection of persons after palsy, either evident disease is found in the brain, or marks of the existence of former disease, which had given occasion to the complaint; and, although Messrs. Rochoux and Riobe have adduced good reasons for believing that fluids effused have been absorbed, and that cavities in the brain have been sometimes closed, yet the mischief may not have been completely removed, nor the brain perfectly restored to its healthy state; and, whilst any morbid cause capable of producing palsy continues in any degree to exist, it is natural to suppose that palsy in some degree would remain. Reasoning from appearances after death from palsy, would lead us to conclude that the disease almost always, in a greater or less degree, does remain. Instances may, no doubt, be adduced of perfect recovery from palsy; but I am persuaded that such are of very rare occurrence. If persons affected with hemiplegia do not become apoplectic in a short time, it often happens that, after a certain degree of amelioration, the disease becomes stationary, or very gradually proceeds, even for several years, before it terminates fatally."

The seventh, and last, chapter is on the Treatment of Palsy. Dr. Cooke first discusses that which should be employed in hemiplegia, which, he again remarks here, "is, in a very great proportion of cases, the consequence of apoplexy: therefore, the plan to be adopted, both for the prevention and the cure of the former of these diseases, is very much like that recommended for the latter; indeed, it differs chiefly in degree." In conformity, however, with his plan, by which each treatise is rendered distinct and complete in itself, the author discusses here the measures to be adopted as prophylactics, and for the treatment of the actual disease under particular consideration. The similarity in the modes of treatment is, however, considered only as precisely proper for hemiplegia in its early state. "When the disease has subsisted for a length of time; when the apoplectic symptoms have disappeared; when plethora, or marks of determination of blood to the head, are no longer present, our mode of proceeding should be different; and certain remedies may be prescribed, which, under other circumstances, would be dangerous. These remedies are chiefly stimulants externally applied, or internally taken." Besides the numerous physical excitants we possess, moral impressions have been occasionally employed by physicians; and there are numerous instances recorded of their efficacy, when those of the former class had failed: but, Dr. Cooke says, we derive no practical advantage from a knowledge of these facts, "the excitement of the passions not being sufficiently under our management and control."

"Of the stimulants to be applied externally, there perhaps is none more safe and efficacious than friction by the hand or by the flesh-brush. I have, in several instances, seen very beneficial effects from a long perseverance in the use of this simple remedy. Friction may be rendered more powerful by stimulating liniments, of which we have many different kinds, such as the fossil acids and volatile alkalies, combined with oil or lard, with a view of rendering them less acrid and corrosive; essential and distilled oils; preparations from resins, gum-resins, &c. Among the most powerful external applications for the purpose of restoring action and sensation, we may reckon blisters and sinapisms, especially the latter, which are amongst the most powerful rubefacients that we can employ. Blisters and sinapisms are very generally recommended in palsy, because they are considered safe and efficacious. One of the Greek physicians, however, very properly says, that, when parts are entirely deprived of sensation and motion, we ought to be careful that sinapisms do not operate too much; the patient, through loss of feeling, not being able to judge of their effects.* Some practitioners are in the habit of applying blisters, or other stimulants, to the head, immediately after the accession of hemiplegia; but I am of opinion that we ought not to make such applications in plethoric constitutions, and especially when the disease is the consequence of apoplexy, till some blood has been taken away; and I think that, when such stimulants are used, they should be applied on that side of the head which is opposite to the paralytic side; because anatomists have ascertained, as above mentioned, that, in a very great proportion of instances, the cause of hemiplegia is seated in some part of the brain opposite to the side affected. Celsus recommends, in these cases, the application of nettles to the surface of the part affected, and also mustard."†

After noticing the statements that have been made respecting warm and cold bathing, Dr. Cooke says, "On this subject I cannot give an opinion from my own experience, but, on the whole, the observations which others have made would lead me to prefer, in palsy, the application of warmth by bathing to that of cold; as the former is more under our command than the latter. If cold does not produce re-action, or if it give occasion to a very great re-action, it would, probably, do mischief." The use of electricity, with the evidences for its efficacy, is next discussed. Dr. Cooke himself says, that, from his own observations of the effects of electricity in paralytic affections, he ventures to recommend it, with due precautions respecting the cases to which it is applied, and the mode in which it is employed. He says,

"Applied in a certain manner, electricity is a most powerful stimu-

* Paul Ægineta, lib. iii. c. 18.

† Prodest etiam torpentis membri summam cutem exasperasse, vel urticis caesam, vel imposito sinapi, sic ut ubi rubere cœperit corpus, hæc removeantur.—Celsus, lib. iii. c. 27.

lant to the nervous system, and therefore much has been expected from it in the cure of palsy; but, as it is also a stimulus to the sanguiferous system, it has often been hurtful in those palsies which depend upon a compression of the brain, and especially when it has been so employed as to act upon the vessels of the head. It is only to be considered safe when its operation is confined to parts somewhat remote from the head; and as, when very strongly administered, it is capable of destroying the mobility of the nervous power, it should be used with only moderate force. Advantage is to be expected rather from a repetition of it than from its force; and it seems particularly suited to the cure of those palsies which have been produced by the application of narcotic powers.* Where electricity has been prejudicial, it has probably been too violently applied, and no greater force should be used than that which may be sufficient to remove or alleviate disease: thus, shocks should never be used when a cure may be effected by sparks; sparks should be avoided when the required effect can be obtained by the wooden point; and if the metal point be thought sufficient, it should be preferred."

When employed in conformity with those precepts, he does not recollect a single instance in which it appeared to do mischief. Dr. Bardsley expresses himself strongly in favour of the employment of galvanism in paralysis, and he relates some well-marked and decisive instances of the successful application of it in various forms of this disease.

"In the application of galvanism in these cases, Dr. Bardsley recommends the method employed by Mr. Wilkinson: for instance, in a case of hemiplegia of the right side, accompanied by vertigo, loss of memory, and involuntary discharge of urine, he began with half a dozen plates, of two inches and a quarter square, and applied the conducting wires in such a manner as to direct the galvanic influence through the brain. The sensation was powerful and unpleasant; but, by degrees, the patient was able to bear the power of a dozen plates. The galvanic fluid was likewise directed along the spine and the upper and lower extremities, in as powerful a degree as the patient's feelings would admit. In about a fortnight, this person became entirely free from any appearance of disease, except a slight retraction of the muscles of the face, which was not attended with pain or any inconvenience.†

"For a minute account of the cases of palsy under the care of Dr. Bardsley, treated by galvanism, particularly of one, most singular and deplorable, to which he calls our attention, as furnishing an unequivocal testimony in favour of the practice, I must refer to his work. Dr. Bardsley draws from his experiments the following general conclusions: 1. That galvanism, judiciously administered, is a safe and powerful remedy in most paralytic diseases. 2. That, as far as three comparative trials will allow an inference, the efficacy of galvanism in

* Cullen.

† Bardsley's Medical Sketches, p. 186, 187.

paralysis is superior to that of electricity. 3. That galvanism agrees with electricity in its sensible effects upon the body. 4. That, when the brain is required to form part of the circle, the galvanic influence ought to be very cautiously administered. 5. If no sensible advantage accrue from a steady and properly-regulated application of this remedy, after a trial of a week or ten days, in paralytic affections, especially where the brain is operated upon, its use ought to be laid aside. 6. When the pulse has become quicker and firmer; the local, as well as general, temperature of the body increased; the feelings, both mental and corporeal, somewhat enlivened; and the altered secretions better regulated; it is proper to infer, from such indications, that galvanism may be persisted in with a fair prospect of ultimate success. 7. Where both sensibility and irritability are so greatly exhausted as not to render the patient susceptible of the galvanic stimulus by the ordinary means; or where, from the unusual thickness of the cuticle, it forms a barrier to the transmission of the fluid, it will be necessary to excoriate the surface by blistering ointment, and apply the metallic points to the raw skin; but the pain and agitation frequently induced by administering the remedy through so sensible a medium, must be guarded against, by adapting the number of plates to the increased degree of sensibility. Dr. Bardsley states, that the galvanic stimulus is an efficacious, though not certain, remedy in paralytic affections; and he is induced to think that, in all cases which appear to originate solely from a diminished excitement in the sensorium, galvanism is to be preferred to electricity."

Similar inferences respecting the superiority of the efficacy of galvanism to that of electricity, have been drawn by Mr. La Beaume, whose opportunities for comparing the results of the use of the two measures have been very extensive. Galvanism, too, can be applied with more precision than electricity, from the regulation of its power being more nicely and completely manageable.

The actual cautery was much employed by the ancients in palsy, and is now much used by the French, in the way of applications of moxa. On noticing an instance of the efficacy of this remedy, Dr. Cooke says, "might not a discharge from the spine, produced by other means, have been equally successful in this case? Was not the application of the moxa preferred on account of the quickness of its operation?" From our personal observations, we are disposed to think that the moxa is superior in its efficacy, in certain cases, to every other mode of cautery. The moral impressions attending its application—from the patient seeing, or knowing, that he has a fire burning, on a part of his body, that he is expecting every instant to arrive at his skin,—are often very powerful; and these, perhaps, have somewhat to do with the efficacy of the remedy.

Of the internal remedies of a stimulant kind, the most celebrated are the *rhus toxicodendron*, *nux vomica*, horse-radish,

and mustard-seeds. Of the first, Dr. Cooke says, "The cases adduced by Dr. Alderson, illustrating the good effects of the employment of the *rhus toxicodendron* in hemiplegia, are very striking, and afford encouragement to a trial of it in those cases of palsy where the employment of stimulants is indicated." It is hardly necessary to inculcate caution in the use of this remedy, as it is the most actively poisonous plant of a species that comprises several deleterious substances. The *nux vomica* is a variety of the most poisonous species (*strychnos*) of known vegetables. Dr. Cooke gives an abstract of the recorded cases in which it has proved efficacious, subsequently to the publication of the paper of Dr. Fouquier on this subject; but we need not cite them in a particular manner, as an account of the most remarkable of them has already been inserted in this Journal. Dr. Cooke does not appear to have himself employed either this remedy or the *arnica*. He says, he thinks he has seen *cantharides* useful in several cases: of camphor, ether, lavender, valerian, castor, and "other medicines called *nervine*," he cannot speak from experience.

The treatment of *paraplegia* from disease in the spine, is next noticed. Dr. Cooke only mentions the practice of Mr. Pott, and the objections that have been made to the use of issues. We shall soon have to present the readers of this Journal with something very interesting on this subject from Dr. Harrison. The new view he has taken of the origin of the disease, has led to a mode of treatment that has been attended with very extraordinary success: spinal distortions, under his practice, are, indeed, amongst the most easily manageable and curable of all diseases of any severity of character.

From the subject just mentioned, the author proceeds to speak of the treatment of *partial palsies*, and in the first place of amaurosis. The only novel remarks on this subject, (with the exception of some statements respecting the influence of electricity, by Mr. PARTINGTON, more favourable to the use of that remedy than those of the generality of practitioners,) are taken from Mr. TRAVERS's Synopsis of the Diseases of the Eye. The precision of observation and the nice distinctions of disease and practical indications, manifest in that work, render those remarks particularly interesting: we shall, therefore, transcribe Dr. Cooke's abstract on this subject.

"Mr. Travers thinks that the treatment of amaurosis should be almost exclusively constitutional. He places no confidence in external applications,—such as stimulant vapours, drops, and ointments; spirituous and aromatic embrocations, sternutories, &c. He makes an exception, however, in favour of cupping, issues, or setons, in certain cases, and of blisters in almost all. He never witnessed any advantage in this disorder from the employment of electricity or galvanism: he

has not known any real benefit from what are called antispasmodic and anti-nervous medicines; nor from the exhibition of emetics, though, from respect to authority, he has fairly tried them in many instances.

"In most cases of amaurosis, Mr. Travers depends on the regulation of the visceral functions, and the employment of such restoratives as the system requires, and can bear. The blue pill, with colocynth, rhubarb, and aloes, and the combination of soda with rhubarb and calumba or gentian, are best adapted, he thinks, to the former purpose. The exhibition of general tonics, he says, is often indicated; and he has seen much benefit from the mineral acids, bark, steel, and arsenic when admissible, after a due regulation of the digestive functions. In recent and sudden amaurosis, Mr. Travers recommends a mild administration of mercury, but salivation, he thinks, is always hurtful; and he is of opinion that 'all cases of direct debility, and proper paralysis of the retina, are aggravated by the loss of blood.'"

Cases of partial palsy more commonly come under the care of surgeons than physicians; and hence it is, probably, that this part of the work presents a view of the medical measures that have been proposed by others, with but few original observations or practical inferences by the author.

An abstract of a report by Dr. Gordon from the minutes of the Army Medical Board, respecting the occurrence of apoplexy and palsy in the army, is attached to the work as an appendix. Dr. Cooke says, it appears to him (from the inspection of the table of returns for a period of six months,) "that the proportion of cases of apoplexy and palsy, as they occur in the army, is very small; a circumstance which may, perhaps, be explained by observing that soldiers generally quit a military life before they arrive at the age when these disorders most frequently occur; and that those who are strongly predisposed to them are, as Dr. Gordon has observed, on that account refused admission into the army."

"Dr. Gordon," he adds, "does not find that any particular make or conformation of body was observable in those soldiers who were affected with apoplexy and palsy. The chief exciting causes of these disorders were intoxication, exposure to the rays of the sun, drinking cold water, and bathing in cold water when the body was heated. Dr. Gordon remarks, that apoplexy often followed epilepsy, long continued fevers, and visceral disease, especially dysentery; and that serous apoplexy sometimes came on after a species of marasmus, denominated *cachexia Africana*; sometimes after an improper use of mercury; and frequently after blows and falls from horseback.

"The apoplectic seizure, in this climate, chiefly occurs between the ages of thirty and fifty; but in warm climates it takes place without much reference to any particular age, as it arises chiefly from exposure to the sun, and the abuse of spirituous

liquors. The appearances found after death much resemble those which I have at large described.

"Dr. Gordon observes, that, in the treatment of this disease in the army, the remedies almost wholly relied upon were bleeding, both general and topical, including arteriotomy; the application of blisters, and the administration of cathartic medicines."

A Dissertation on the Treatment of Morbid Local Affections of Nerves: to which the Jacksonian Prize was adjudged by the Royal College of Surgeons. By JOSEPH SWAN, Member of the Royal College of Surgeons, and Surgeon to the Lincoln County Hospital. 8vo. pp. 196. J. Drury, London. 1820.

"Non scribo hoc temere. Quo minus familiaria sum,
hoc sum ad investigandum curiosior."

Cic. *Ep. ad Fam.* lib. iv. Ep. xiii.

THE dearth of knowledge on the subject of this dissertation, and the urgency of the reasons medical practitioners have for desiring that their information should be more extensive; as well as the hopes entertained that late researches have contributed to supply the deficiency; were strongly expressed by the Court of Assistants of the College of Surgeons, when they proposed the question on which it is founded, only seven years after this question had given origin to one of the best monographies in medical literature. It is true that the dissertation before us relates especially to the *treatment* of diseases of the nerves; whilst the other, just alluded to, comprises, with the same object, considerations on the physiological and pathological relations of this part of the human economy: but, such men as those who proposed the question, could not have contemplated the probability of any important improvement in the treatment of diseases of organs so extensively and variously related in the system as the nerves, without a corresponding improvement of our knowledge of their functions and morbid affections; or that therapeutical precepts could be applied with any considerable degree of confidence and precision, if unconnected with particular pathological indications. Under these circumstances, the work to which the stipulated premium has been conceded, appears with claims on the attention of medical men of a very forcible kind; and it is especially incumbent on those who profess to view with a critical regard the progress of medicine, to examine it with strict severity, and endeavour to determine the precise extent of the original knowledge it is qualified to impart. These remarks are intended to serve as an apology for, perhaps, a more rigid censorial review of this work than may, as we are disposed to

believe, be most beneficially applied to the generality of books; for, it is better that some few inanities, plagiarisms, and amusing absurdities, should pass current for valuable, as well as rightful, property of their promulgators, than that so dreary and disheartening a picture should be drawn of the history of medical science, as must be done were the real state of its present relative perfection to that of former and remote ages, displayed in an unreserved manner.

Although it is, in the title-page, expressly stated that this dissertation relates to the *treatment* of diseases of the nerves, the author, in conformity with a sentiment expressed above, does not merely discuss therapeutical indications and the means for fulfilling them: he endeavours, also, to establish some inferences respecting the nature of those diseases.

Another preliminary remark which we have to make, is that, as it appears, the author has considered it incumbent on him to give a view of all that is known respecting the means proper for the treatment of "local" diseases of the nerves; and hence it is, as we infer, that some of his discussions do not contain any thing that is proffered as original.

There is nothing remarkable in the author's Preface, excepting the following statement: "When a part has been deprived of the nervous influence, by its communication with the sensorium being intercepted, the functions of the part to which the nerve is distributed are suspended, and are incapable of being reproduced until the divided portions of nerve has become reunited, except through electricity."

We shall have occasion to notice the evidence which has been adduced in proof of this assertion, when this point becomes the subject of discussion in the course of this review.

The dissertation commences with the consideration of "Diseases and Injuries of the Nerves in general." The author thinks it prudent to treat of those diseases "under two distinct heads; viz. those that affect the nerves belonging to the senses, and those that affect such nerves as are under the influence of the will."—"A third division," he continues, "might be added, which would include the ganglian system, belonging chiefly to the grand sympathetic nerve, and distributed in great measure to the thoracic and abdominal viscera; but, as I do not know that any particular researches have been made by pathologists into this part of the nervous system, to ascertain whether disease occasions any change in it to take place in the parts to which it is distributed,* and as it is not much connected with

* There is, it appears, some error (to be attributed to the printer, perhaps,) in the distribution of the words of part of this sentence; but the author's meaning seems to be obvious.

the department of the surgeon, I shall pass it over; for, as I could say very little more than what is theoretical, it would not answer the intention for which the present subject was proposed."

The author, in conformity with the view above indicated, treats, in the first instance, of diseases of the nerves of the senses; and he commences with "diseases and injuries of the olfactory nerves." On this subject he remarks that

"The power of the nerves constituting the sense of smell, may be diminished or destroyed by the frequent application of strong odours to the nose, or from an inflammation of the Schneiderian membrane. The same thing may likewise happen from pressure on the origins of the nerves by hydatids, or an accumulation of water in the lateral ventricles of the brain, or from their being involved in a diseased action going on at the under surface of the anterior lobes of the brain, or from a diminution of the foramina of the cribriform plate of the ethmoid bone. When there is an inflammation of the Schneiderian membrane, which takes away this sense, leeches may be applied to the outside of the nose; and the inside may be anointed, by means of a feather, with some cooling ointment; and purging medicines may be given. All the other diseases are generally beyond the reach of art."

These remarks are followed by an account of a case in which, the author says, "the sense of smell of the right nostril appeared to have been suspended by an inflammatory action going on about the crista galli." The author's inference here is very plausible—from the evidence furnished by the symptoms and seat of pain—but, as the case did not terminate in death, (but in restoration of the function,) or so as to admit of an examination of the parts, the inference is merely plausible. The author refers to a case in MORGAGNI (Epist. ix. art. 25,) where there was proof of inflammation in the seat alluded to accompanying similar symptoms.

The author then remarks that the functions of the olfactory nerves are sometimes so diseased as to produce a sense of unpleasant odours; effects analogous to the sensations of flashes of light and sounds, without the proper external excitants, from diseases of the optic and auditory nerves, respectively.

This is all that is advanced respecting diseases of the olfactory nerves; and, but for the practice we intend to adopt in this article, it would be superfluous to remark that it does not contain any thing that is novel.

"Diseases and injuries of the Optic Nerve," are next considered. The author says, (we quote the remarks, because we do not mean to omit to notice any of his reflections.)

"Diseases and injuries of this nerve, and its expansion the retina, are almost always attended with a destruction of its functions; so that, though every other tunic of the eye and its humours are perfectly

sound, and capable of transmitting freely the rays of light, no impression is made by them [the *rays of light* the author must mean, not the *tunics and humours of the eye*, as the grammatical construction of the passage indicates,] on the retina, which constitutes the disease termed amaurosis."

After the foregoing passage, there follows an account of some of the causes and the principal symptoms of amaurosis, as it generally takes place. This account contains nothing that is new, nor is it characterized by any remarkable degree of precision. It is succeeded by some therapeutical precepts, of a general and common-place character, and devoid of originality.

The next subjects of discussion are "Diseases and injuries of the Gustatory Nerves." The author remarks, that "the gustatory nerves are sometimes injured by being violently bruised between the teeth; and, though there is no apparent injury of the tongue, those powers of the nerve, producing the sense of taste, will be destroyed. The evidence adduced in proof of this statement, is a case related by Sir EVERARD HOME, and published in the Philosophical Transactions.* In addition to this, Mr. Swan remarks that morbid states of the tongue occur, in which things, at other times sapid, will make no impression on that organ during such a condition of it, or the impressions will be very different from what they ordinarily are; or there will be a sense of various unpleasant tastes in the mouth, without the presence of the proper external causes.

Respecting "diseases and injuries of the Auditory Nerves," the author relates a case of deafness of one ear from a supposed fracture of some part of the base of the skull; which deafness still existed at the end of "some months" from the time of the receipt of the injury.

An account follows of the symptoms of the ordinary cases of deafness, and some remarks respecting their treatment, which are equally devoid of novelty with the parts of the work previously noticed. After this, we arrive at a disquisition relative to hearing that is of a physiological character, and which the author says contains "something new on this subject." This is a transcription of a paper published in the Medico-Chirurgical Transactions,† with the addition of the history of a case serving to show that a sense of articulate sounds may be perceived by means of impressions on the nerves of the face. But Mr. Swan's notions in this respect are not so precisely novel as he imagines: the probability of the connexion of the facial nerves with the sense of hearing, had been stated by MAGENDIE, in his Elements of Physiology, (tom. i. p. 100;)

* An abstract of it is given in the eleventh volume of this Journal.

† It was transcribed in the 250th Number of this Journal.

and many curious facts tending to show that such a faculty as that supposed to be possessed by the facial nerves particularly, is also occasionally possessed by the nerves of touch in general, were published, several years since, by PFINGSTEN.* Mr. Swan thinks it probable that, by exercise and proper instruments, the powers of the facial nerves might be so much developed, that children, otherwise deaf, might be made to hear tolerably well by this medium.

The "diseases and injuries of the nerves of Touch," are so much the same, the author says, as those of the nerves with which they are connected, that he shall consider them altogether.

The second chapter treats of "Diseases and Injuries of the Nerves of Voluntary Motion, &c. in general." The author here commences by remarking, that

"In paralytic affections, the nerves of voluntary motion are generally those that suffer; and, though the nerves of feeling, or those of the skin, generally arise and are chiefly connected with them, they do not appear to suffer in the same degree that those do which are destined to supply the muscles. As, for example, in a paralytic limb pain and itching will be complained of, and the sense of feeling will remain at the time the muscles have not the power of obeying the will."

Sensation, it may be added, is sometimes abolished whilst the power of voluntary motion remains. These facts have been considered to favour strongly the notion of the nerves of sensation being throughout distinct from the nerves serving for motion: but this notion is opposed by the apparent intimate confusion of all the nerves of a limb in the plexuses. Mr. Swan is disposed to adopt the same notion that GALEN also thought most plausible, that "the muscles of voluntary motion require the nerves to be in the most perfect state to enable them to act; and that a less degree of perfection is necessary for them to perform the functions required for the sense of feeling." In support of this view he observes, that, in a great degree of pressure on the spinal marrow, there is generally a loss both of sensation and the power of voluntary motion; whilst, in less degrees of pressure, the faculty of sense generally remains to a certain extent: and, alluding to the existence of the power of motion without feeling, he says,

"Upon due consideration of the subject, we cannot, I think, be much surprised that, when the nerves have been divided or injured, as in paralysis, a great difference should exist as to the parts to which they are distributed requiring different degrees of perfection in their

* *Vieljährige Erfahrung über die Gehörfehler der Taubstummen.* Kiel, 1802.

An account of some of Pfingsten's experiments was given in an abstract of a dissertation by Professor ROSENTHAL, in the 256th Number of this Journal.

restoration, to enable them to perform their respective functions; for the parts to which the nerves producing the sense of feeling are distributed, are to be acted upon mechanically by things external to, and unconnected with, the animal; whilst those intended to serve the purposes of voluntary motion, are to be produced by a nicer stimulus, —viz. through the agency of the will; which is something so subtle as not to be entirely comprehended by us, either as to the manner in which it is formed by the brain, or how it is communicated by the brain to the parts it calls into action.

“ Though it is sometimes the case, yet it is, as I have just now stated, a comparatively rare occurrence for the nerves of sensation to suffer from paralysis; and those of voluntary motion, though arising from the same trunks, to be but little affected by it. When it does occur, I conceive that such an alteration takes place in the skin, or the parts composing it, as to prevent the proper exercise of the functions of the nerves distributed to it.”*

There is something more in this matter than is here regarded, for here are no indications for an explanation of the existence of insensibility, not in the skin merely, but throughout the whole of a limb, whilst the power of voluntary motion was perfect.† A little reflection will suggest an abundance of conjectures on this subject; but, as our senses here can make us acquainted only with effects, it is not probable that we shall ever arrive at a knowledge of the causes of the phenomena alluded to. We may suppose that the want of the power of motion may depend on alterations in the properties of the muscles themselves, with or without alterations of those of the nerves; or that (which is plausible) voluntary motion depends on the transmission of some influence from remote parts, which transmission to the limb may be interrupted; whilst the transmission of influence from it, concerned in sensation, may be undisturbed;‡ or the faculties of sense and the power of voluntary motion may be the effects of distinct properties of the

* “ This affection most commonly arises from a disorder of the digestive organs; and, if the brain is affected at the same time, it suffers from the same cause. As far as I have seen, when paralysis arises from pressure on the brain or medulla spinalis, the voluntary nerves always suffer with those of feeling; and I do not see how it can be otherwise.”

† See vol. xlii. p. 297, of this Journal, for a remarkable example; and *l'Histoire de l'Académie des Sciences*, 1743.

‡ There is a curious fact related, with different views, by CUVIER, respecting the probable existence of a sense of feeling without the presence of the brain, that, as it appears to us, has not been regarded with the attention it seems to merit. Cuvier (in an experiment to determine the functions of the inferior glottis of birds,) cut off the head and neck of a duck: “ The animal afterwards walked a few steps; and, when it was struck, it uttered several cries, which, although weaker than those it gave when it had its head, were nevertheless very sensible.” — *Leçons d'Anatomie Comp.* tom. iv. p. 454.

Many analogous statements, though less precisely applicable in the views with which this account is cited, have been made by men whose veracity and accuracy of observation cannot well be doubted; as will be seen on a reference to the physiological writings of Whytt, Perrault, Unzer, Bonnet, and Fontana. But,

same structure; one species of which may maintain its ordinary relations with the brain, whilst the other is destroyed, or has these relations suspended. Some writers have attempted to explain this point, by stating that motion depends on an active state of the nerves, whilst these organs are *passive* in sensation; but those who can conceive the nerves to be passive in this function, have an imagination qualified very differently from ours. As, however, we can attain no sensible evidence of the causes of the phenomena, no conjecture respecting them can be any thing better than a more or less plausible supposition, founded on loose analogies, and must therefore be adapted for the amusement of our curiosity rather than the purposes of the practice of medicine. We revert to the dissertation before us, where our last transcript terminates.

Mr. Swan remarks, here, for the further support of the view he has adopted, the fact, which had already been noticed, that a certain state of circulation in the skin is necessary for the perfection of its sensation. He adduces, however, some original observations relative to this point, from a structure of the nostrils of the horse that appears to him to be formed for the perfection of the sense of smell.

"Beneath the Schneiderian membrane there are numerous sinuses, and many of them of considerable size, which have frequent communications with each other, and appear to be composed of a very thin and inelastic membrane, which is very strong, and perfectly smooth in the inside: within the sinuses are contained very delicate and extremely elastic vessels, which may be called veins, as they appear to be filled with venous blood; and, by their being thus situated within sinuses of a determinate size, they are capable of being distended to a certain degree only; which provision is necessary, as their extreme delicacy would otherwise either endanger their very frequent breaking from over-distension, or be the cause of much injury to the very delicate nerves, by a too-great pressure that would be thus made on them.

"This structure, I have no doubt, generally exists in animals, and may be very satisfactorily demonstrated in the horse; and it must, I think, appear to any one examining its peculiarities attentively, that it was not formed merely for returning the blood from the nose, but that it was made for distending the Schneiderian membrane, so as to give it a proper degree of tension to enable the nerves to receive more acutely the impressions from the odorous particles when applied to the"; exactly in the same manner that it is required for the nerves of the penis to produce their peculiar sensations, that the parts connected with them should be properly distended with blood."

in the cases alluded to, what are called voluntary motions were performed: yet, as these motions are effected in some animals when deprived of brain and spinal marrow, it seems a necessary inference that they arise from causes different from those productive of our ordinary and proper *voluntary* motions.

Mr. Swan then advances the objections against the inference of a diversity in the nerves serving for sense and motion that have been often repeated; and with these the second chapter terminates.

The third chapter treats of "Diseases of the Nerves of Voluntary Motion," which are, the author says, of two kinds, active and passive.

"The active diseases are all those affections of the nerves attended by pain, and frequently by a motion of the affected part,—as *tic douloureux*, &c.

"The passive are those affections termed paralysis.

"In the active, at the part which appears to be the seat of the disease, there is an increased action of the blood-vessels, and likewise an increased heat; whilst in the passive, there is quite a contrary state.

"Those local complaints which appear to originate spontaneously, or in some cases where a slight wound has been inflicted, I believe to be only symptomatic of a general irritability of the brain and nervous system. The almost constant failure of topical remedies, and of the division of the affected nerve, must lead to the conclusion that the cause of the local diseased action, or primary affection, must reside in some other part of the body: and if we inquire into the causes of the local active affections of the nerves, it will be found that the atonic state of the body, or whatever tends to render the brain and nervous system irritable, will generally be found the most frequent."

These statements are either erroneous or, to our powers of comprehension, somewhat obscure. In the first place, "*active diseases*" of the nerves are not always attended by pain. A case is related in the Dissertation which gained the Jacksonian prize in 1813, of a tumor of the radial nerve, the formation of which had not been preceded by "pain or spasm." We do not, we must suppose, understand the author's meaning when he states that *active* diseases are frequently attended by a *motion of the affected part*. That there is always "an increased action of the blood-vessels, and likewise an increased heat," in "*active*" diseases of the nerves, is a supposition to which there are not wanting valid objections: the parts about the affected nerve in *tic douloureux* (an "*active*" disease, according to the author,) are not unfrequently colder than natural, and evidence of increased action of the blood-vessels is often wholly absent. It is now so general a custom, after having said all we know about the origin of a disease, to bring in inordinate *irritability* as the cause of all the rest, that the author can hardly be blamed for the vague remarks he has made on this point. But, in truth, this term irritability is merely a veil for ignorance that it would be more prudent to acknowledge: it is only a substitute for the *peccant humours* and *fermentations* of the older pathologists, and is, perhaps, not an advantageous one; for the latter

do convey ideas of something positive, and therefore would still keep us in the way of observation and research; whilst the former, like all admissions of occult agents, is, as Bacon expresses, only qualified to arrest and lay asleep all true inquiry and indications. When we have some precise ideas of things, there is a chance, should they be erroneous, that we discover them to be so, and adopt correct ones in their place; but when we have no precise ideas, and yet cheat ourselves into a belief that we have, by the use of a *term*, there is no ground for expectations of improvement.

The author, after the remarks above alluded to, proceeds to say that women are more liable than men to this irritability, which itself will arise from undue mental exertions, certain passions, improper regimen, and disorder of the stomach. These general preliminaries on this point, then, contain nothing that is novel. The particular discussions commence with one on "painful affections of the Nerves of the Head and Face." "These complaints," the author says, "have been variously denominated,—intermitting pain of the head; hemicrania; tic douloureux, &c.; but they appear to be all the same disease, only varying in situation and degree." After describing the ordinary symptoms of this neuralgia, and stating that it is sometimes accompanied with increased action of the blood-vessels, the author says,—

"It appears to me that the irritation of the nerve is the cause of the increased action of the blood-vessels: nevertheless, this increased action may tend to increase or keep up the irritation of the nerve.

"It has frequently happened, after an operation in which a nerve has been principally concerned, that, either during the healing of the wound or after it has become completely cicatrized, if an increased action of the blood-vessels is produced, as is shown by inflammation about the part, the painful sensations resembling tic douloureux are produced. By this I would not say, that in this complaint [that is, we presume, tic douloureux,] there is an inflammation of the nerve, because I think other facts go to prove that there is not; but it shows that the increased heat and action have a decided effect in keeping up the complaint. The nerves may become enlarged from irritation, as in a case I shall relate, in the same way the muscles are from continued action: but when there has been inflammation of a nerve, though only of the chronic kind,—and to which sort that of tic douloureux must bear the greatest resemblance, if it were inflammation,—there would be the same change of structure that takes place in all continued inflammations of other parts of the body; viz. an enlargement from the deposit of coagulable lymph. This is shown when there has been a chronic inflammation of the extremities of the nerves in a stump, or when the nerves have been confined to a part that has been long subject to inflammation; and if this had been the case in tic douloureux, I think it would not have passed unnoticed."

These remarks are pretty good, as far as they extend, but they present only a faint and very partial glimpse of the admirable discussion on the same point, in the Dissertation which gained the prize in 1813. The author's remarks respecting the origin of this affection, are merely repetitions of common-place considerations. A case follows of *tic douloureux*, consequent on a blow over the right eye, which continued for ten weeks, and then suddenly disappeared on the occurrence of an eruption, like that of nettle-rash, all over the patient's body.

With respect to the treatment of *tic douloureux*, the author says, there appear to him to be two principal indications: "the first consists in strengthening the constitution, and thereby enabling it to counteract the habit which favours the continuance of the irritation; the second, in allaying the local irritation." The first is to be fulfilled by the exhibition of tonic remedies; the best of which, he thinks, is cinchona. This medicine has for some years been the favourite remedy of the French practitioners. Leeches, evaporating lotions, cold or warm fomentations, and an opiate liniment, are the measures for the second indication. A case is related, to prove the efficacy of these remedies. Another case is then adduced, that was also cured by the same means, where the disease ensued from a slight external injury. A third case, occurring subsequently to considerable disorder of the system, immediately consequent on the poisonous effects of verdigris, is afterwards related: it was treated, successfully, with cinchona. The author then alludes to the use of the other remedies which have been proposed, and amongst them the section of the affected nerves, without, however,—we regret to have such constant occasion for this repetition,—adding any original observations or inferences either of a general or particular kind; and we cannot say that what he does advance is characterized by any extraordinary degree of precision.

The fifth chapter treats of "painful Affections of various Nerves," in other parts of the body than the face: as instances of which, the following case is related; and two, of a similar kind, by Mr. Earle, (in the *Medico-Chirurgical Transactions*), and Mr. Abernethy, (in his *Surgical Observations*), are referred to.

"Mrs. W. had a pain in the left arm, which extended, in the course of the ulnar nerve, from the elbow to the little and ring fingers, both of which were weak and painful to the touch; the pain was not constant, but came on by fits. There was an evident disturbance of the digestive organs, with palpitations of the heart.

"She used a spirituous embrocation for the arm, and took five grains of the mercurial pill at bed-time, and a mixture with camphor and the volatile tincture of valerian; by which the pain was diminished. She was then attacked by a severe affection of the uterus; and after

some time, when she was recovering from this complaint, the pain in the nerve ceased entirely, and never returned."

When a section of the nerve is determined on, the author recommends that a portion of the nerve should be removed; a point of practice that was very amply and luminously discussed in the Dissertation which gained the prize in 1813. Mr. Swan, in relation to this subject, says, "I think it is a question whether the nerves have the power of communicating their influence to other nerves whose communications with the brain have been cut off, in the same manner the arteries have whose direct communication with the main trunk has been intercepted by a ligature; but I think we may safely say, that, at all events, it can only exist in a trifling degree, and in some particular cases."

Inferences, the reverse of those drawn in the passage just cited, are satisfactorily established in the Dissertation which gained the prize in 1813. This is a point of very great importance in regard to practice: it is, therefore, discussed in the Dissertation just cited, with a degree of care and profundity worthy of the object. We shall not express what our feelings are on finding it noticed in so superficial a manner as it is by Mr. Swan, seven years subsequently to the publication of the discussion just alluded to. We shall have more to say on this subject hereafter.

The sixth chapter is "on Inflammation of Nerves." This chapter occupies but three pages, and does not present any thing that is novel and original. This, too, is a subject which is treated on in a very interesting manner in the Dissertation which gained the prize in 1813: and, in the course of the long and profound, yet luminous, investigation devoted to it, the author, whose name we have not mentioned, has every where scattered an abundance of hints, for practical views, of the most important character.

"Ulceration of Nerves," is the subject of the seventh chapter. No example is adduced of the exclusive ulceration of nerves; the author's observations on this point relate merely to ulcerations of nerves involved in ulcers of the adjacent structures. In one of the two cases related in this chapter, the most remarkable circumstances were a thickening of some of the nerves of a lower extremity in which there was an old ulcer on the tibia, whilst others were emaciated and enveloped in a peculiar sort of fat. Several varicose veins were observed in different parts of the sciatic nerve, and some of the nerves were unusually soft. The other case presents nothing particularly remarkable. A case from MORGAGNI, and another by Sir EVERARD HOME, in which ulceration of nerves, of some considerable size, was supposed to exist, are referred to.

The eighth chapter is on "Tumors in the Nerves."—"When a tumor is forming in the substance of a nerve (the author says,) it causes very violent pain, which sometimes affects the whole nerve in which it is contained." That this statement is not correct as a general description, we have already shown, by quoting a case in which a tumor had formed in a nerve without the existence of pain. Mr. Swan relates a case, in which a tumor in a subcutaneous nerve formed about the middle of the leg, without any assignable external cause, and which was removed, with a favourable result, by excision. Cases by Portal, Dr. Denmark, Mr. Abernethy, and Sir Everard Home, are adduced in an abstract form; the whole of which, except that of Portal, (which is not an important one,) had been noticed in the Dissertation which gained the prize in 1813, and were there made the bases of numerous important inferences that have not been arrived at by Mr. Swan; whilst the same Dissertation presents all the views taken by this writer; differing from the latter only in being much more perspicuous and comprehensive, and infinitely more fertile in indications for the purposes of the practice of medicine.

After this, the author enters on a discussion of the question whether it is better merely to divide, or to remove a portion of the nerve, in certain cases, when its communication with its centre is to be interrupted. He says, "When a nerve has been divided, re-union in course of time generally becomes perfectly established, so that it performs its functions as well as if no division had ever taken place. When a portion of a nerve has been removed, and especially if it be a large portion, the breach is with the greatest difficulty, if ever, repaired, when it happens in the case of a nerve of the largest size."

The author of the Dissertation which gained the prize in 1813 had settled this point quite as well as the existing facts would permit it to be done; whilst he endeavoured to determine, precisely, the extent to which the reparation of nerves might be effected, and the time required for such a reparation. On this point Mr. Swan has added nothing to our knowledge; nor has he given any original hints for its improvement.

The ninth chapter is on "Injuries of the Nerves of Voluntary Motion, &c." It is so concise that we shall wholly transcribe it.

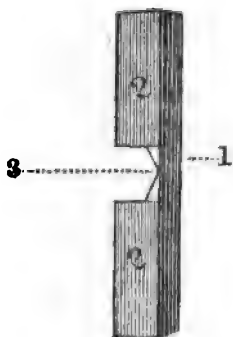
"The symptoms occasioned by injuries of the nerves are frequently very violent, but they are so various as to make it impossible to say what will be the result of an accident that has affected them; as sometimes an apparently trifling injury of them will bring on bad symptoms, whilst at another time a more violent one will not be attended by a single untoward symptom."

"The Treatment of Divided Nerves," is the subject of the tenth chapter. The author commences with stating, that "when a nerve has been divided, if the external wound is healed by the first intention, very little pain is felt in the nerve, in proof of which I shall relate the following case." The case shows that in one instance no pain ensued from division of a nerve of the thumb, when the union of the wound was effected by the first intention. The author has adduced no other than this single point of evidence, that is but *negative*, as the ground of a *general positive* inference. The rest of this chapter contains no pathological observations or therapeutical precepts—if we except the details of some not particularly remarkable cases; from which, however, the author has not drawn any original inference, excepting that above cited,—that are not presented, in a manner that precludes comparison, in the Dissertation which gained the prize in 1813. One of the cases is related, because it seems to prove that the sciatic nerve was wounded in a fracture of the neck of the femur below the capsular ligament, (an accident which the author thinks happens not unfrequently,) and because "the appearances of the limb in this case were different from what are usually presented in fractures of the neck of the thigh-bone; and were such that, without great care, might have been mistaken for a dislocation of the bone backwards."

The eleventh chapter is on "the Treatment of Punctures, or partial Divisions of Nerves." This chapter commences with a general account of the symptoms which occasionally ensue from the puncture or partial division of a nerve of any considerable size. The author then enters on an hypotheticalal discussion respecting the symptoms in question. This we shall wholly transcribe, not because it appears very luminous or satisfactory, but because we think it prudent to expose, in as complete a manner as possible, whatever is peculiar to the author in this Dissertation.

"When a nerve has been wholly divided, each portion of it immediately retracts, so that a considerable space is left between them. When only a partial division has taken place, the divided portions retract in the same manner, though not in so great a degree; leaving a space in the divided part of the nerve, whilst the undivided portion remains of the same length as before the division. Now each nerve, or at least the greatest part of them, is composed of different fasciculi, and these fasciculi, in most instances, communicate together; should one complete fasciculus be divided, that had not any communications with the others of which the nerve is composed, it would retract, and leave its fellows in the same state as before the division; and it is most probable that there would be no other difference, as from irritation, &c. than when the nerve is completely divided; but if a fasciculus is

partially divided, or if it is wholly divided, and at the point of division it was connected with the adjoining fasciculus, the retraction of the divided parts would stretch those that were joined to it, and thereby cause considerable pain; for we know that this stretching of a nerve produces violent symptoms, as in cases of tumor. I will endeavour to explain my meaning by the following:



1. Undivided portion of nerve.

2. Two divided portions.

3. Two fibrils by which the divided and undivided portions communicated, and which, by the retraction of the divided portions, must be kept on the stretch.

"But again, should a nerve be wholly divided, except one fasciculus, and at this place there were not many communications, the great retraction of the divided parts would very much keep on the stretch the undivided fasciculus. Any one may be satisfied of this, by taking an animal soon after it is killed, and laying bare a nerve, and almost entirely dividing it: the divided portions will be seen to have retracted in some degree; but, immediately on dividing the remaining part, each end of the nerve will retract in the quickest possible manner to a much greater distance than it did before the undivided part was cut through; thereby clearly proving that, as there was nothing but this small portion to prevent the retraction, it must have been kept very much on the stretch."

After this, the author remarks that, from his experiments in partially dividing the nerves of animals, it does not appear to him that more suffering ensues from such an injury, in the inferior animals, than from the entire division of a nerve: a fact which is somewhat adverse to his explanation; and he is, hence, induced to suppose that there are some peculiarities in the nervous system of man that give origin to such a diversity of results; or that there must be some peculiarities in the constitution of those persons in whom the severe morbid consequences have taken place; or that it is only when a punctured nerve becomes inflamed that those same effects happen. To all those suppositions, excepting the last one, there can be no objections; but, with respect to the last, it is satisfactorily shown, in the Dissertation which gained the prize in 1813, that this inflammation of the nerve that is the seat of the severe morbid symptoms, cannot reasonably be regarded as

the cause of the symptoms; and must itself be only a consequence of some other inappreciable condition.

This remark will apply to the evidence developed by anatomical researches, in a very extensive manner; it has recently been very forcibly presented to us in respect to the whooping-cough: we were beginning to hope, from some appearances witnessed on dissection, that we had discovered the cause of the phenomena peculiar to this disease, and especially of the obvious affection of the nervous system which so frequently arises in its progress, (and which seems to have happened with particular frequency during the epidemical prevalence of the disease in London, in the last winter;) but these hopes were unfortunately dissipated, by our finding precisely similar appearances (as far as our senses inform us,) in the body of a child who never was affected with the whooping-cough.

Mr. Swan adduces (from his own observations,) histories of some cases in which severe symptoms, attributable to wounds of nerves, ensued from the operation of venesection; but they present nothing extraordinary: some others, more or less interesting, are cited from the works of SABATIER and LARREY. His therapeutical precepts are only repetitions, not remarkable for any extraordinary degree of precision respecting their application, (which might render repetitions of general precepts of some value,) of what has been advanced by the ordinary writers on this subject; whilst several very interesting facts bearing on this point, contained in the Dissertation which gained the prize in 1813, and proper to that work, are not noticed by him. Mr. Swan then treats of Tetanus. He first describes its symptoms, and afterwards adduces the following remarks respecting its etiology.

“The cause of the disease is a violent irritation of the nerves, produced either by the suppression of perspiration, as when it comes on from cold, or from an irritation of the nerves of a wound, either where the large nerves have been injured, or where their more minute branches are irritated from the unhealthy action in the wound. Larrey* relates three cases where it was produced by an injury of the larger nerves. In the first, the anterior crural and sciatic nerves had been injured by a ball; in the second, the median nerve had been tied with the brachial artery; and in the third, the nerves had been tied in amputation of the leg.

“Some have supposed that this complaint proceeds from some disease in the parts about the medulla spinalis. The changes from the healthy appearance have been found in the membranes of the medulla spinalis in some cases of this kind, there can be no doubt; but whether they are the consequences of the violent contractions of the muscles or accidental occurrences, cannot, I think, be determined: at

* *Memoires de Chirurgie Militaire*, tom. iii. p. 290.

all events, I should hardly be inclined to think that the changes in these parts can have been the causes of the disease."

With respect to the doubts expressed in the last sentence: they had been previously stated in the Dissertation which gained the prize in 1813, and connected with a close investigation and comprehensive inferential disquisition on this subject. Dr. COPLAND has mentioned to us that he has, in one instance, (the only research of the kind he has yet made,) found the spinal marrow and its membranes highly injected with blood, in a hare which had been hunted just previously to its death; an observation which, though it be solitary, is qualified to throw much doubt on the propriety of regarding the inflammatory appearances found about the spine as the *cause* of tetanus. Dr. SAUNDERS, of Edinburgh, says,

"1. If any muscle, voluntary or involuntary, is affected with spasm, and during this affection the person dies, on examination it is found that the nerves which supply the spasmed muscle are covered with turgid red vessels at their visible origins, or where they appear to set off from the brain, medulla oblongata, or spinal marrow.

"This turgescence, and the effects of turgescence, are in the ratio of the degree and duration conjointly of the spasm or convulsion.

"The turgid vessels, in every obstinate and severe case, may be traced into the substance of the spinal marrow, by the sides of the strizæ, which seem to be the continuations of the nervous filaments; as also along the nervous cords, through their sheaths formed in the dura mater.

"The position of the body after death has no appreciable influence on these appearances: they are observed anteriorly or posteriorly, in the loins, thorax, cervix, or within the skull, bearing strict relation to the parts which have evinced spasmodic action.

"But the nerves serving the muscles which have not laboured under spasm or convulsion, are free from turgid vessels.

"2. If the tetanic affection is confined to the jaw, certain nerves arising from the tuber annulare and medulla oblongata, are found in the state above described.

"But if the tetanic affection involves the whole inferior extremities and the trunk of the body, as well as the jaws, then the origins of the nerves, from the tuber annulare to the cauda equina, are covered with turgid red vessels.

"In short, the nerves exhibiting such turgescence at their origins correspond, in number and situation, with the muscles which have exhibited inordinate contraction. I have conducted this investigation for about sixteen years, and have not met with one exception.

"Some are of opinion, that I maintain that the spinal marrow, its nerves and membranes, are always affected with turgid vessels in tetanus: this is incorrect. I have examined cases of trismus, in which the spinal marrow, its membranes and nerves, were almost entirely sound, from the atlas to the lumbar vertebrae. In these instances,

however, not only the origins of the nerves at the medulla oblongata, but the medulla itself, was inclosed with a close net-work of turgid red vessels.

“ There are many other appearances within the cranium and spinal canal, more or less connected with spasms and convulsions ; but those which I have here related are uniform, and accordingly constitute, we have reason to believe, an essential part of these diseases.

“ Morbid changes in the organization, as of substance of the brain and spinal marrow, or in their envelops, belong to another order of maladies, and, when present in those affected with spasms, the symptoms always indicate complication.”

The very interesting nature of this subject has led us to digress more from an analytic examination of the work of Mr. Swan than we intended : but our readers, we do not doubt, will wish that we had endeavoured, on other points, to render this article more interesting than it must be, if constituted solely of an abstract of the (almost without exception) very imperfect and common-place discussions of the author of the Dissertation before us. We proceed in our analysis according to the mode we have adopted in the former part of it ; and the next remark we have to make is, that Mr. Swan's observations respecting the treatment of tetanus present nothing that is original, and are of the most common-place character.

This brings us to the twelfth chapter, which treats of “ the Effects of Ligatures on Nerves.” It begins with the assertion, that “ many experiments have been made on animals, to show the effects which a ligature applied on a nerve has on the parts to which it is distributed ; but they do not show much respecting the changes the nerve itself undergoes, or the diseases the ligature might occasion.”

The Dissertation which gained the prize in 1813, presents the results of a series of experiments instituted for the express purpose of ascertaining the effects of ligatures on nerves : the object is a practical one, and, like all objects of this kind, it is discussed with a relative view in the Dissertation just cited. The common-place remarks of Mr. Swan, and the few observations he cites from other authors, will but ill supply the place of the discussion just referred to, for the purposes of the medical practitioner.

The thirteenth chapter is on “ the Compression of Nerves.” We shall give an abstract of the several cases which Mr. Swan introduces on this subject ; because it is true that they present some *new observations*, and our opinions respecting the value of these observations may differ from that of many other persons. The first two cases are cited from RICHERAND, to show that, when a certain degree of pressure is continued for a certain length of time on the trunk of a nerve, the parts to

which such a nerve is distributed may be deprived of their powers of sense and motion.

"A young man went to sleep with his head resting on his arm, the outside of which was placed on the edge of a table so as to compress the radial nerve; and the consequence was an insensibility of part of the integuments, and a paralysis of the muscles at the back part of the fore-arm. These symptoms were removed by irritating frictions in the course of the nerve.

"Compression of the median nerve during an operation that was performed on the fore-arm, produced a numbness of the limb; the sensibility was not restored before the end of forty-eight hours."

Mr. Swan then says, that, when the nerves have been injured from a continued pressure, "the best remedy will be frequent frictions of the hand, and the use of a stimulating embrocation;" for which he gives the following recipe:—"R. Linim. sapon. comp. 3x; Liquoris ammoniæ, 3ij. M." After citing a case from Portal, the author relates one to show that the bladder, when suffered to become distended in paraplegia, will tend, by its pressure on the nerves going to the lower extremities, "very materially to retard, if not to prevent, their restoration." Another case is then cited from Portal, to prove that "sometimes the nerves suffer so much from a sudden compression as to lose entirely their power, which they have the greatest difficulty in recovering." That "the same accident happens sometimes to the nerves of the axillary plexus, from an injury of the shoulder," is shown by a case narrated by the author. He then remarks that—

"A nerve may be extended some way without giving pain or uneasiness, as I have frequently observed in making experiments, when I have passed a probe under the sciatic nerve, and drawn it from its situation; and as is shown in cases of popliteal aneurism, when the swelling may get to some size before much pain is produced.

"But when a nerve is extended in any considerable degree, pain is excited; and, if the extension is increased, the pain is increased in proportion, till at length the nerve begins to ulcerate, and, if the pressure is not removed, is almost entirely destroyed."

After this Mr. Swan, in continuation, remarks, that "violent blows on the back sometimes cause bad symptoms, though they are unattended by much apparent external injury;" and a case is cited in proof. He then relates a case, "because it shows that, after an injury of the medulla spinalis, the nerves may be sufficiently restored to be capable of performing their functions so as to produce feeling, when they are not so, in the least degree, for the production of voluntary motion; and likewise because it shows that, when the medulla has not been too much injured, if every compressing power is removed, a very great

degree of restoration may be effected." With this case the thirteenth chapter terminates.

The fourteenth chapter presents the results of "an experimental inquiry into the process nature employs for repairing Wounds of Nerves." The author says,

"Many experiments have been made by physiologists, to prove that, when a nerve is divided, all sensation and motion are lost in the parts to which it was distributed; and that, after the re-union of the divided parts, it performs its functions as well as before the division. It had always understood that this was a point generally agreed upon by physiologists; and it has been so well illustrated, especially by the experiments of Dr. Haighton, that it is difficult to conceive how, after an elucidation so satisfactory, any doubt should remain on the question. But, when we find it contradicted by several eminent men, so much hesitation is produced in the minds of those who are unbiassed by any favourite hypothesis, as to lead them to make an experimental inquiry into the subject for themselves."

After noticing the impertinent expressions of doubt of Richerand on this subject, and the manifestations of ignorance respecting it, of Delpech, Mr. Swan says,

"Amidst these contradictions, as I was not aware that any experiments had been instituted to show the process nature adopts for the restoration of the parts, and as I could not obtain from books knowledge sufficiently satisfactory, I have made the following experiments, which I trust will account for many things respecting injured nerves, which surgeons do not at present seem clear about."

We can discern nothing of any value in the results of the experiments of Mr. Swan, that is not presented in the Dissertation which gained the prize in 1813. The general results of observations at several diverse periods, from various species of wounds of nerves, were stated in that Dissertation. Mr. Swan has shown the results of observations at a *greater number of periods*; but whether they are of importance, is a subject of mere opinion, and may be doubted until this importance is proved. The author of the earliest dissertation of the two on this subject, of course, thought that observations of the appearances at a certain number and diversity of periods, were sufficient for every useful purpose, or he would have extended his experiments; or else he must have considered that the results would not be likely to be sufficiently useful to warrant the additional torture they would cause to animals which, by their destinies, are submitted to our power. The observations of Mr. Swan do not appear to us to be calculated to make that author alter such an opinion, should he have entertained it: Mr. Swan does not, we repeat, prove the importance of his additional experiments, by any precise inferences, in the "conclusions" from them, with which his dissertation terminates.

Notwithstanding the weariness of the reader who has accompanied us so far in this article, we have yet a few remarks to offer before we part with him on this occasion. Mr. Swan, in the motto to his Dissertation, says, "*Non scribo hoc temere. Quo minus familiaris sum, hoc sum ad investigandum curiosior*:"—an assertion which but ill accords with his neglect of the Dissertation on the same subject that gained the Jacksonian prize in 1813.* We must suppose that he has not perused that Dissertation; for (without considering that several inferences are satisfactorily established in it that are adverse to the notions of Mr. Swan,) we cannot suppose that, had he been acquainted with the Dissertation alluded to, he would have ventured to publish what he must then have been conscious contains so little of valuable matter that is not to be found in the one which had preceded it; and what presents also, in every respect, so very inferior a view of the subject referred to. We repeat, in a general allusion, what we have said in relation to every separate chapter in the Dissertation of Mr. Swan, that, excepting a very few facts, which we consider to be of but trivial importance,—and which the author has not made to appear otherwise, by any original inferences from them,—that it contains no original information of at all considerable utility to medical practitioners. In making this assertion, we do not, with only two or three instances of exception, (as the notions about the connexion of the facial nerves with the auditory faculty; those respecting the reason why sensation is lost whilst the power of motion is preserved, and the converse [which are not novel]; and some observations on the structure of the pituitary membrane in the nostrils of the horse, which are original;) allude to what is to be found dispersed in numerous and foreign authors; we state it in reference to the Dissertation, on the same subject, that gained the Jacksonian prize in 1813. We are confident that every reader will be satisfied of the propriety of this assertion, on making a comparison of the two works. We have not cited the passages in proof of our statement from the earlier one, because this Dissertation is constituted of such an intimately-connected series of inferences and indications, or is, in other terms, such an admirable illustration of the motto to it,† that hardly any passages could be extracted in an insulated manner, without doing injustice to the author, by presenting a very imperfect view of his disquisitions on al-

* This Dissertation, it may be proper to remark, was printed and published, by Callow, in 1815.

† Namque aliud ex alio clarescet; nec tibi cæca
Nox iter eripiet, quin ultima naturæ
Pervideas; ita res accendant lumina rebus.

most any point considered in them. The reader will here, probably, be disposed to inquire how it has happened, then, that the prize has been assigned for this Dissertation of Mr. Swan: we are perplexed for a plausible conjecture in reply. The most probable one we can form is, that the members of the College who examined the memoirs (if there were more than one,) considered themselves obliged to award the prize to the *best* of them, whatever it might be. But this explanation almost precludes a supposition that more than one memoir was presented to them; for it is hardly possible to conceive that a dissertation so devoid of value as one from which it is impossible to select any thing of at all considerable interest or importance (with the two or three exceptions above stated,) that cannot be pointed out (expressed in at least as perspicuous and satisfactory a manner,) in *one book alone*, and that an English one, which was published five years since; it is not reasonable to suppose that such a dissertation could have been regarded as the *best*, had there been any contest for the decision. We must not express a doubt of the Court of Examiners being well acquainted with the Dissertation which gained the prize in 1819.

We have yet to observe, that there are three plates attached to this Dissertation: one, to show the figures and distribution of some regenerated nerves in a rabbit's leg; another, to represent the figures and distribution of the sinuses in the pituitary membrane of the nostrils of the horse; and the third, to show the situation and distribution of the more superficial nerves about the face and neck, especially those most frequently affected in tic douloureux. The preparation (made by Mr. Swan,) according to which the last plate was executed, is preserved in the museum of the College of Surgeons. So minute a representation of those nerves, is a thing of no small value; and we have regarded it with much gratification,—not but that we have seen as minute and intricate preparations, as well as figures, before,—but, unless one knows by whom they were executed, no confidence can be felt of their exactness. Such preparations, it is true, are preserved in museums at public schools, and sometimes with the names of eminent professors attached to them, though they have been the production of their pupils; and few young men can resist making attempts to improve, artificially, the appearance of their work, or to remedy, in the same way, the effects of an unlucky stroke of the scalpel, destroying the results of many days', or even weeks', labour. Hence it is that, in many such preparations, our admiration is conferred on the intricate distribution of varnished and painted strings of cotton and catgut.

CRITICAL ANALYSES

OF

RECENT PUBLICATIONS, IN THE DIFFERENT BRANCHES OF
MEDICINE AND SURGERY,

In the Literature of Foreign Nations.

Παρίδες ἄρα

Ἀνδράων, ἢ παύσαις ἄνδρες, ἀγαλλόμεθα.

Principes généraux de Physiologie-Pathologique, coordonnés d'après la Doctrine de M. Broussais. Par L. J. BÉGIN. 8vo. pp. 390. Mequignon-Marvis, à Paris, 1821.

IN the preceding article on the subject of this work, we took into consideration some of the principles most obviously connected with the development of morbid actions in the human body; we shall now pass in review, in a similar manner, several of the most important phenomena of disease, and endeavour to indicate how far they may be illustrated by means of the principles above alluded to, without entering more deeply into the history of their origin than is conformable with the researches of our author, and the ordinary habits of reflection of medical practitioners.

Mr. Bégin commences his disquisition on maladies in general, with the proposition that all diseases have their origin from more or less severe "lesion" of one or other of the "organs" of the body: by which statement, he means to indicate that there are no original diseases of the system generally, and that many of the symptoms in those appearing to be so extensively seated, are only effects or accidental contingencies of the essential organic lesion. This proposition will by many men be considered so obvious an inference from the best physiological, as well as etiological, views, as not to require an author now to produce any arguments in its support; and Mr. Bégin would probably have done no more than simply state it, had there not been recent instances of attempts to render prevalent some vague, or more properly, absurd, notions of certain diseases (idiopathic fever, as they say,) having their original seat in no one part especially, but in the whole system generally; a proposition which precludes all reasoning on the nature of the diseases said to be of this species; for it is impossible to draw any inferences respecting their etiology, when we have no ideas on which any reasonings can be founded.

All the local lesions which produce the apparent external phenomena, by the aid of which we recognize those diseases, do not leave traces, after death, in the part they have affected. MORGAGNI, Mr. Bégin remarks, and several other physicians whose accuracy of observation and due qualifications cannot be doubted, have stated that they have seen instances of patients dying after having experienced the symptoms of apoplexy or of pleurisy, without the body, after

death, presenting the slightest sign of lesion of the brain, or of the pleura. But these cases do not lead to the inference that diseases may exist without derangement in the vitality and texture of the organs: they only show that these derangements are sometimes so inconsiderable as to be dissipated, or not to be apparent, after death. The redness of the cheeks and tongue, which is sometimes produced by gastro-enteritis; peripneumonia; erysipelas, and other cutaneous inflammations, though very severe during life, often disappear instantly after death; and why may we not conclude that the same results may take place in inflammation of the internal organs?

After the establishment of the proposition above stated, a question, Mr. Bégin says, the solution of which is fundamental in pathological physiology, is this: "In how many different ways may the living tissues be morbidly altered?" Notwithstanding the objections which have been raised against it, and the partial modifications which it has suffered, the fundamental proposition on which *Brownian* rests, Mr. Bégin says, has been religiously preserved; and Professor Broussais himself, who is considered as the most strenuous adversary of that doctrine, in some degree conforms to it. "The *pathologic lesions** of the diverse tissues, says the author of the new medical doctrine, are characterized by the diminution or the augmentation in their vital phenomena. For us, the first of these states is an *ab-irritation*, the second a *super-irritation*, or *morbid irritation*." On this point Mr. Bégin very properly remarks, that a consideration which presents itself before the question can be discussed, is, that the tissues which BICHAT regarded as simple are very complicated, and should be analyzed by the pathologist. There enters, for example, in the composition of the mucous membranes, capillary blood-vessels, arterious and venous; cerebral nerves, and nerves of the ganglionic system; mucous follicles; exhalant and absorbent vessels; and it is obvious that especial lesion of each of those elementary textures will produce particular and peculiar effects in the organ in which it is seated, as well as in the remote parts with which it is related. With respect, too, to the therapeutical indications founded on the same principles, there are certain agents which seem to modify with more predilection one of these elementary textures than another. Thus, some purgatives often confine their agency to the production of a more abundant secretion of mucous fluid; certain stimulants excite more expressly than others the nervous sensibility, whilst many substances of the same kind act with more energy on the capillary blood-vessels. These distinctions may seem to be minute, but they are well-founded, and present some important therapeutical indications. They elucidate phenomena which are constantly witnessed in practice, and, amongst others, the effects, in a manner elective, of a given stimulant which cures, whilst another stimulant, not apparently more energetic, would have certainly been deleterious. Thus, for example, there is a certain degree of gastric inflammation which is removed by a dose of tartar-emetic, and which

* We cite here the terms of Mr. Bégin.

would have been aggravated by wine or ether: the tartar-emetie, wine, and ether, are all stimulants; and the first, if applied to the skin in a healthy state, would excite more inflammation in it than either of the latter. A multitude of examples of a similar kind will readily present themselves to the reflective observer.

Besides these considerations, Mr. Bégin adds, that we reasonably doubt if these notions of augmentation and diminution of action being the only modes of essential disease, "the simplicity of which is so seductive, and which the physician uses with so much assurance at the bed-side of the patient, are conformable with the course of nature;" and it is, he continues, "not without alarm that we can see all diseases reduced to two classes; the *materia medica* to two kinds of medicines; and general therapeutics to two curative indications."

"We cannot conceive, we must confess, what can be the nature of the vital actions which should not consist in depression or exaltation of the powers; but this motive is not sufficient for their rejection, if accurate observations attest their existence: this relation of more or less is not the only one which may exist, either between bodies, or their qualities, or the actions they execute. Does it not appear, for example, that syphilitic irritation, which has the same seat as scrofula, differs from the latter only in degree? The different species of herpes, although seated in the same order of vessels, present peculiar diversities which are not explicable by diversities in the violence of the irritation. Let us however observe, that, in proportion as physiological pathology is improved,—as we study with more attention the diseased tissues, and take into consideration the modifications which diversities of age, sex, and temperament, effect in the morbid phenomena,—we see diminish the number of diseases to be attributed to different species of augmentation or diminution of the vitality of the organs. The time will perhaps arrive at which the two principles of the Scotch physician will be demonstrated to be exact in all cases; but hitherto the evidence does not appear to be complete: new researches are necessary, in order to elucidate this point of doctrine."

We have cited thus copiously from the work before us, in order that we might enable the reader to form an idea of the scope of Mr. Bégin's reflections in this instance: the examination of the subject has been pursued to its utmost depth by only one author, whom we should incessantly quote, were we to do justice to his merits.

However it may be with regard to the subject of the foregoing discussion, Mr. Bégin says "it is indubitable that a very great majority of diseases consists in exaltation or diminution of vital action, and that one or other of these alterations always effects certain organs, which then produce in the economy very various sympathetic effects, which characterize, externally, the different diseases." The views here indicated are, perhaps, as penetrative as any that can be taken with our present knowledge of the human economy; and, what is more worthy of consideration, they are probably sufficiently so for the purposes of general use, where more profound information and nicer distinctions might not be often available.

"Are the diseases which consist in *irritation** more frequent than those of which debility is the first cause?" is a question next noticed by Mr. Bégin: it is one, he says, that merits all the attention of physicians, for it must lead to very different practical results, according to the manner in which it is solved. Broussais, though he has designated many of the diseases which he considers to consist in *irritation*, has not yet expressed his opinions on this point in a comprehensive manner: Mr. Bégin endeavours to supply the deficiency here alluded to.

"It would seem, on a first consideration," Mr. Bégin says, "that vital action is as susceptible of diminution as of exaltation. It would be possible even to sustain this proposition in a general manner; and many physicians would not only have faith in it, but would demonstrate that the former of these deviations is of more easy and frequent occurrence than the latter. However, when we study facts, when we observe a great number of patients, and we arrive thus to the source of our knowledge, we soon acquire the most complete conviction that irritations constitute alone almost the entire demesne of pathology. Look over the nosologic tables; study the phenomena of diseases; open dead bodies: every where, in books, at the bed-side of patients, and in the amphitheatres, you will find proofs of the correctness of this assertion." These three (or properly two) sources do not present such demonstrative proof of Mr. Bégin's assertion as he conceives; or they only show that certain phenomena, which we confound altogether under the name of irritations, most generally accompany the progress of diseases. The appearances found on the examination of dead bodies, are often only very remote effects of the first morbid change in the constitution of the parts affected, to which we must advert if we are to have correct notions of the nature of the disease; and with respect to the symptoms, during life, of *irritations*, it yet remains to be proved that they are indications of an exaltation of vital action. But let us admit, as a matter of convenience, that irritation is an exaltation of vital action, or is essentially allied with such an exaltation, and that the development of irritation is the first or most important morbid change in the diseases which are accompanied with, or ordinarily characterized by, it:—let us admit thus much, and see how Mr. Bégin supports his proposition, that such diseases constitute alone almost the entire demesne of pathology. "What has led systematic physicians to erroneous conclusions," Mr. Bégin says, "is their appreciating the force of the irritated organs by that which is possessed by the external parts; or, rather, they most commonly are ignorant of what organs are affected, and, seeing the limbs enfeebled, they infer that the disease is owing to debility, which has struck, as they say, the whole organization."—"These heresies," he

* The word *irritation*, employed in a general manner, according to Mr. Bégin, "signifies an augmentation of action of the capillary vessels or of the nerves which enter into the composition of parts. But, whenever it is necessary to give more precision to language, and to designate the texture especially affected, it becomes convenient to employ the following expressions: *sanguine irritation*, *lymphatic irritation*, *nervous irritation*, &c."

adds, are no longer admitted by enlightened practitioners; and their discredit is such, that criticism should disdain to combat them." This emphatic, rather than forcible, reasoning, leads him to assert, in continuation, that "observations, well directed, demonstrate *then* that the most part of affections consist in irritation of the organs; and my task would be confined to the announcement of this general result of the best-directed clinical investigations, and to cite *MOREAU, BICHAT, PNOET, DUFURMEN, LAENNEC, and BROUSSAIS*, who have rendered it incontestible, if it were not indispensable to theory to trace the series of physiological phenomena that explain the fact above stated." This is effected by Mr. Bégin in the following manner; omitting here only some details not of essential importance to the disquisition.

The nutritive actions of composition and decomposition preside over the existence of animals and vegetables. In order that a body having life may execute all the actions necessary for this function, it is requisite that it be excited; if it ceases to be thus affected, it languishes and perishes. Two classes of agents, indispensable for the maintenance of life, and which may become powerful causes of disease, are the excitants of the organic movements; one of which consists of matters that stimulate the economy, without presenting to it any thing which is capable of increasing the quantity of its liquid or solid materials;* the other of substances which prostrate the exercise of its functions, at the same time that they furnish it with the matters which repair the losses it suffers. A subdivision of the agents which excite the animal economy, is that into such as stimulate the whole system, and such as exert their influence only on certain parts of it. The general stimulants are light, caloric, electricity, oxygen, aliments of good quality, a due abundance of blood properly endowed with nutritive materials. The energetic, long-continued, and united, action of these different excitants, augments the force and vivacity of the vital actions, and thus disposes all the organs to excessive excitement; and the state of the economy which results from this violent stimulation (which is most commonly experienced in the age of adolescence, and most remarkably in the nations which inhabit the hotter regions of the globe,) is sometimes carried to such a degree that the subject of it, tormented by a superabundance of force, instinctively gives himself up to the most fatiguing exercises, and to the most destructive excesses, in order to dissipate this exuberance of vitality, which torments him beyond endurance.

The superabundance of very stimulating nutritive materials may arrive at such a state as will determine, in the most sensible parts, and especially in the mucous membranes and in the blood-vessels, an irritation which is followed by disturbance of the circulation and the other functions, and which constitutes that variety of inflammatory fever that Sauvages called plethoric.

Different effects are produced when the non-nutritive stimulants are

* This reasoning is shown to be incorrect in a work lately reviewed in this Journal. See vol. xliv. pp. 40—42.

the only species which act with an excess of power. The organs then are incessantly stimulated, without the losses produced by the vital movements being properly repaired; the nervous susceptibility then becomes extremely developed; the body becomes emaciated and enfeebled to the utmost degree. Such of the people of Asia and Africa as indulge in a continual abuse of opium, coffee, and in undue development of certain passions, as *venus nimia*, &c., and who feed on aliments having but little nutritive qualities, present examples of the effects of the inordinate use of non-reparatory excitants on the animal economy.

The privation of the two classes of general stimulants above mentioned, leads to a diminution of the vital powers in all the textures. The nutritive elaborations languish and become less complete; the organs, imperfectly nourished, are incapable of fulfilling their functions in the ordinary manner; the colourless fluids predominate over the red blood, and the latter itself is of a less intense colour, and contains less of nutritive matter than in the more healthy state. The results of the want of these general stimulants may be observed in cold, humid, and marshy, countries, where the inhabitants are nourished with a meagre and insalubrious diet. Persons in this condition are equally deprived of moral and physical energy,* and their vital actions preserve in their diseases a remarkable degree of sluggishness and debility, whilst the sympathetic movements in them are deprived of energy and vivacity. All the parts of the body are not, however, equally debilitated in these subjects. "The weakness resides almost exclusively in the exterior parts and in the least important organs: in proportion as the forces diminish, life becomes concentrated, in a manner, and retires towards the parts the most indispensable to the preservation of the individual; the principal focuses of the vital power absorb the last remains of the nutritive materials, and are the seat of the latest movements."

The superabundance or the penury of the general stimulants determine, in the whole economy, a relative excess of force or of more or less considerable debility. The too energetic action of these agents excites especially the nervous system; their privation debilitates the same parts, favours the elaboration of colourless fluids, and alters the composition of the blood. In the first case, there is an imminent disposition to irritation; but, all the organs possessing the degree of energy which is appropriate for them, and being habituated to a regularity and facility of action, there is established in the economy an "equilibrium," which the most immoderate abuse of excitants can hardly destroy. In the second case, the disposition to irritation is far from being abolished; it is perhaps more considerable† than in the

* We may refer the reader to the writings of MONTESQUIEU, KAIMS, CABANIS, and DE STAEL, for extensive disquisitions on this subject, and a due appreciation of the general statement of Mr. Bégis.

† This doctrine, of parts in a state of relative debility being therefore especially disposed to irritation, at the same time that irritation is regarded as an exaltation of vital action, is sadly in want of something like plausible arguments for its support: it is, however, a piece of doctrine that is now very popular.

other condition; the nervous mobility* favours "local concentrations," which take place with particular facility in the abdominal viscera: the "equilibrium" being but ill-established, or even broken in favour of those viscera, their lesion is the effect of the action of the most slightly energetic causes; though, if a person escape the influence of such causes, he may live in a languid state, and be even ready to perish, by the gradual extinction of his strength, without presenting any sign of local irritation, or any other "lesion" existing than his debility.

The most general, most active, and most efficacious, cause of excitation of the organs, is the exercise of the functions of those organs; an exercise which is provoked by the application of the stimulants which are proper to them: and, on the contrary, a want of exercise of them is the circumstance which most contributes to enfeeble them. Exercise determines in the vessels of the organs a more considerable afflux of fluids; their tissues, when excited by this cause, become more solid, more voluminous, and acquire an increased ability for action; but such effects take place only when the elements of nutrition are carried to the organs in a due abundance, and when they are of a good quality: without this last condition, the parts would only become enfeebled by their exertion.

It is to the alternative action of our organs that are to be attributed, almost exclusively, this "equilibrium" of the vital movements, and those regular and periodic returns of repose and excitement, that are observed during life. Those alternatives of action, their periodic returns, and the simultaneous excitement of two or more organs, rapidly become habitual when they have once been established, and constitute associations of functions and synergies, which play an important part in the development of the phenomena of diseases. Disease of one organ thus rapidly excites disease of other and remote organs, by an influence that is impalpable to the senses, but which we know, from its effects, to exist.

The inaction of one of our organs, by depriving it of the concentration of vitality which is the immediate effect of irritation, and by keeping absent from it the materials of nutrition, which are elsewhere directed, debilitates this organ, and insulates it, in a manner, from the rest of the economy; "but without any sympathetic effect in the other organs resulting from this inaction."† In order to recognize the correctness of this proposition, it is, however, necessary to distinguish, in diseases of debility, that which appertains to the local debility, from that which is produced by the interruption of the functions of the enfeebled organ. "All the sympathetic lesions are produced by the nervous system,"‡ and "it is not possible for them to be effected

* It should be remembered that Mr. Bégin has just said that the nervous system is especially enfeebled in this condition.

† This last proposition is very disputable, even with the modifications of it which immediately ensue. It is amply discussed in the Dissertation which gained the Jacksonian prize in 1813, and in several parts of the "General Indications which relate to the Laws of the Organic Life."

‡ This is a disputable proposition. If one branch of a polypus be rudely

otherwise than by means of the influence of an organ which is itself primarily irritated."* Thus, for example, the stomach may be in a state of debility, without any sympathetic effect in the system resulting from it.† But, in this case, the organ will effect an imperfect elaboration of the aliments it receives, and this imperfection will give rise to more or less secondary phenomena. "An organ simply enfeebled, is not painful; it cannot, consequently, transmit any excitation to other parts. The physiologist should regard it merely as a parasitic instrument, which lives at the expence of the economy, the mass of which is useless, but which exerts on the rest of the body no other deleterious influence than that which results from the cessation of its functions."

Such are the principles on which Mr. Bégin founds his conclusion that by far the greater part of diseases are constituted of irritations;‡ but there are, he says, other reasons from which it happens that, whatever may be the nature of diseases on their first development, physicians hardly ever have to treat any others than those of irritation. "Men are," Mr. Bégin says, "as it is well known, much more dis-

touched, another branch of it will, as an effect of this impression, be put into vital action: this effect cannot well be distinguished from those we term sympathies; and no nerves have been found in the polypus. A similar effect may also be witnessed in the sensitive plant. Some men, it is true—reasoning on the *petitio principii*, that no mode of sensibility can exist without nerves,—have inferred that even plants must possess nerves: but, if we are to condemn the evidence of our senses after this manner, we must resign all our pretensions to knowledge on any subject.

* This is a very interesting question in physiology: whether there be a communication of properties from one part of the economy—otherwise than from the blood and the nervous system, (which Mr. Bégin, from some inadvertency, we conjecture, neglects to notice, unless it may be considered that he has contemplated this in the effects of the functions of organs:)—to others, in the state of health, that is necessary to the preservation of this state; and, therefore, whether diseases may not arise in various parts, from an enfeebled organ not transmitting its ordinary influence, as well as from a communication of IRRITATION? We know, however, no positive arguments in favour of the affirmative; whilst the negative is supported by such as these,—that the rest of the economy will preserve its ordinary health after the loss of two or three limbs. Mr. Bégin's proposition must, therefore, be considered as highly probable.

† Mr. Bégin says, that the sense of weakness about the epigastrium, and the general languor which arises from it, (and which are removed by the smallest quantity of food or a single glass of wine,) from hunger, should not be considered as objectionable to his proposition: "it is incontestible," he says, "that there is then pain and irritation in the stomach, and that the general symptoms are produced by this pain, which is alleviated by food or drink;" but, if substances of too stimulant a kind, or such as are not proper for nutrition, are taken, this irritation is aggravated, and the general debility increased.

‡ This term is here used according to Mr. Bégin's application of it: that is, as designative of an exaltation of vital action. When we ourselves use this word, it should be understood that we employ it as a concise expression of our ignorance, when we suppose that some modification of vital action, of a morbid kind, exists, with the nature of which we do not pretend to be acquainted. We use the term excitement, to designate what we conceive to be a simple augmentation of vital action. We are not, however, satisfied that it is possible for such a mode of action to exist; yet it may be convenient to have a term for expressing an augmentation of action which is not accompanied with, or necessarily followed by, any sensible morbid effects.

posed to the abuse of stimulants than to endure their privation. *It is extremely rare that this privation is sufficiently complete to give rise to [literally, to determine,] diseases;** and when persons, even those the most exempt from prejudices, become sick, they at once resort to the use of the most active tonics or excitants. The labourer, oppressed by want, collects the last remains of his property, to procure himself wine, brandy, or other analogous liquors. Hot wine with sugar, punch, *vulneraries*, preparations of steel, elixirs of all sorts, sudorific infusions, compose the pharmacopœia of the more opulent persons. It is extremely rare, in the practice of medicine, either in hospitals or in the largest cities, to meet with diseases *free from all curative attempts*† almost always have the subjects endeavoured to remedy them by the aid of stimulants. There results from this general practice, that, if the disorder has depended on a penury of excitants, it is rapidly dissipated, and the physician is not consulted. Professional aid is sought only when the affection has resisted the common *touchstone*, and when the patient has already aggravated the disorder by the incendiary agency of the most powerful tonics; and, solely because his advice is required, the practitioner should expect to have to treat a disease which consists in irritation of some one of the organs: examination but rarely controverts this presumption."

Mr. Bégin then sums up his inferences respecting the nature of diseases in general, in the following manner:

"1°. The excessive action or the privation of general stimulants, never provokes, in the economy, any thing more than states of force and of debility, respectively, which predispose to local diseases. 2°. Even in the case of a penury of stimulants, the weakness is greater in the exterior than in the central parts; and this circumstance favours the development of irritation of the latter. 3°. Local *ab-irritations* cannot excite any sympathetic phenomenon of re-action in remote parts. 4°. The absolute privation of food, and the use of substances not proper for nutrition, which are considered as the general causes of debility, provoke irritation of the digestive organs, which complicate and augment the state of debility of the other parts of the body. 5°. Although diseases originating from debility were more frequent than we can suppose them to be from the foregoing reasons, the physician would yet have but a very small number of them to treat, because patients almost always, before they resort to professional aid, adopt the use of stimulants, which cure them in cases of debility, but which, most commonly, aggravate irritation."

Mr. Bégin says that he should here have terminated what he had to say about diseases in general, if, after Brown, the existence of passive inflammations and hemorrhages had not been a point of doctrine "generally professed." The objectionable notion here indicated is by

* Mr. Bégin should read Dr. HARTY's History of the late Epidemy of Ireland.

† Mr. Bégin will excuse our altering his mode of expression in our translation. "*Maladies vierges de toute tentative de traitement*," is too metaphoric, too delicate for English sensibility, to permit us to adopt the expression, however highly we may ourselves admire and approve such an ornament of style in medical literature.

no means generally adopted amongst us, and those who profess it, after having considered what has been said on the subject by Dr. PARRY and some other of our writers, would not, we conceive, have their faith destroyed by the arguments of Mr. Bégin. Professor Broussais' opinions on this subject, and his etiology of scurvy,—a disease which has been considered to present proof of the existence of passive hemorrhages,—have already been stated in this Journal. We shall, however, transcribe the paragraph which forms a conclusion to Mr. Bégin's discussion on these subjects: it presents in itself a summary of his opinions.

"Thus, on carrying our analysis to the utmost point, it is irritation that is the sole cause of the 'oscillation' and the 'equilibrium' of the vital actions; it is that which 'calls' the fluids, and which determines their afflux to this or that particular part; it is irritation which excites, or which exasperates, the motions of the capillary vessels, whether sanguineous, secretory, exhalant, or others, whatever may be the elaborations with which they are charged, and the composition of the fluids they prepare or contain. *Ubi stimulus, ibi affluxus, and Duo-bus laboribus simul obortis, vehementior obscurat alterum*: such are the bases of physiology and pathology. These axioms are, in medicine, of an importance equal to that of this proposition, that all bodies reciprocally attract each other, and that their attractive force is in a direct ratio of their masses, and in an inverse ratio of the square of their distances: a proposition which serves as the basis of all explanations in physics."*

* It would be very amusing, were not the objects of medicine of too serious a nature to allow of such a feeling being indulged when the best interests of that science are violated, to contemplate the diversities of opinion amongst men of eminent talents respecting even the very principles of pathology. Broussais and Mr. Bégin regard irritation as the foundation of almost, if not quite, all diseases; and irritation they consider to be an affection, originally, of the nervous system.* These are notions which have been designated by a modern English author, who was really a medical philosopher, as "either visionary or inapplicable, and which lead to practices tending equally to debase the moral character of mankind, to produce or perpetuate disease, and to discredit the medical profession."

* Mr. Bégin says, in conformity with Broussais, that "the agents of irritations commence, in all cases, with agitating the nervous system, which seems to be especially destined to collect all impressions; it is only secondarily that the stimulation is communicated to the capillary vessels, and that the fluids are directed towards the part."

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[HIGLEY AND SON, FLEET-STREET.]

Medical and Physical Intelligence.

A BOY born, at Morillon, October the 20th, 1817, and manifesting now the signs of puberty, was presented at a late sitting of the Society of the College of Physicians of Paris. He is fat and strong, and weighs about sixty-seven pounds; his height is three feet two inches. The circumference of his head is one foot seven inches; that of the thorax, immediately below the arm-pits, two feet; the diameter of the pelvis, from the pubis to the sacrum, five inches and nearly a half. The length of the penis, in a lax state, from the pubis to the end of the glans, is three inches and seven lines; its diameter at the base, three inches five lines; when in the state of erection, the penis is five inches in length: the testicles are not quite proportionally large. This boy, Jaques-Aimé Savin, has abundant, shining, chesnut-coloured, hair, which is somewhat stiff and curly. The body generally is slightly covered with hair, of a lighter colour than that on the head. There is a growth of downy hair, of the colour of that on the head, in the ordinary situation of the beard and whiskers; about the pubis, the hair is as abundant as it commonly is at the age of sixteen or eighteen years. The skin generally is rugous, roughish, and resistant to the touch. His *digestion* is very active, and he eats with voracity not the most delicate sorts of food. His muscles are well-marked, and his movements are firm. The strength of his loins was found to be equivalent to the weight of 90 pounds, and that of his wrist 27.* He can lift and carry from one place to another a weight of from forty to fifty pounds. He is bold and courageous; he wrestles with persons much older, and even bigger, than himself, and despises any trials of the kind with little boys. Fighting and wrestling are his favourite amusements. His voice is strong, and resembles in its timbre that of a lad of sixteen or eighteen. His external organs of sense present nothing particular. His intellectual faculties are not developed proportionately to his other functions, and especially not to that of his genital organs. His memory is particularly good; but his judgment is only conformable with his age and education,† (speaking in relation to persons in general;) and the same may be said in respect to his imagination. We shall transcribe what Dr. BRESCHET, whose description we are here abridging, says of his *passions*, in his own words, omitting only one short passage: "Les organes genitaux ne restent pas dans un état d'inertie. Souvent le penis entre en erection, et la presence de jeunes filles ou de femmes produit cet effet. Dans ces circonstances, toute la personne de Savin est animée et agitée; les yeux, la parole, et le geste sont en harmonie, et par un

* The strength of the loins of an adult man is said to be commonly about 290 pounds, and that of the wrist 110, when employed in the following way:—To try the former part, the *dynamometer* is held by the feet, whilst the hands pull at a bar placed horizontally; for the latter, a cushion, sustained by a bar which communicates with the spring and needle of the *dynamometer*, is struck by the fist.

† His father is a baker.

instinct particulier il cherche à porter ses mains vers les organes genitaux d'un sexe différent du sien, sans trop savoir les fonctions auxquelles sont appelés et les uns et les autres; il paraît certain que ni la copulation ne sont connues de cet enfant."

He presented nothing extraordinary, with respect to size or weight, at his birth: the midwife, however, told the parents that his bones seemed to be larger than ordinary; the sutures of the cranium appeared, also, to be firmer than they commonly are at the time of birth. He was suckled by his mother for eleven months, and was weaned without experiencing any indisposition. About this epoch it was remarked, accidentally, that he had a severe inflammation about the penis; and a medical practitioner, who was called to see him, found that he had severe paraphymosis. Emollient anodyne fomentations and poultices dissipated this inflammation nearly, but not entirely, in the space of two months. The cutting of his teeth was precocious: at the age of three months, the first incisors appeared in the upper jaw; at four months, seven teeth were to be seen; and at one year, there were twenty. Since this epoch he has not cut any.

His parents are healthy, and present nothing remarkable in their organization. His father, twenty-five years of age, is of a lymphatico-sanguineous temperament, five feet two inches in height, of a slender habit, and arrived at puberty at the age of fourteen. His mother is twenty-seven years of age, of a sanguineous temperament, of a weak habit, menstruated at fifteen, was married at twenty-four, and at the lapse of fourteen months from her marriage, the boy above described was born.

AN account has been lately read to the Society above named, of what the narrator—Dr. CHARPENTIER, physician to the royal forces of the marine, at Guerigny, near Nevers,—considers to have been instances of spontaneous combustion of the human body. We shall give the history in detail, because there are some circumstances in it that seem very forcibly to favour the inference above mentioned; whilst the occurrence of the phenomenon in two persons at the same time, on the contrary, furnishes grounds for a strong suspicion that the combustion arose from an external source.

"On the 15th of January, 1820, at ten o'clock in the evening, several neighbours of Mrs. P—, of Nevers, perceived a peculiar odour, which they thought similar to that of broiled animal matter and burning wool, only more disagreeable and nauseous. They saw neither smoke nor vapour issue from any of the adjacent houses; and at last, agreeing amongst themselves that this odour was produced by the burning of the remains of an old Carmelite nun, who had died in the neighbourhood that day, they retired to bed without making any further inquiries.

On the 13th, in the morning, a woman, living near the place, who had a second key to the door of the house, because she was in the habit of going there daily to assist the servant in attending on her mistress, opened the door to go and perform her ordinary duties. On

entering the room,* a dense vapour issued out, accompanied with an insupportable stench, that almost suffocated her. She retreated from the house, crying out in the most violent manner for help. The neighbours came about her; and, after waiting a few moments to let the vapour escape, they proceeded to examine the state of the room. They found neither Mrs. P— nor her servant. At first they saw no appearance of dead bodies, but they immediately recognized that Mrs. P.'s bed was entirely burned. Its different parts, however, preserved their form; but, on the slightest touch, it all sunk away, and the bedstead, pallasie, mattress, feather-bed, sheets, blankets, and curtains,† were reduced to a cinder. Before they stirred these cinders they examined the fire-place, in which they found no wood, nor any charcoal, in combustion: the fire had not been covered, and it had probably gone out for want of wood. A candlestick stood in the fire-place, and another, on the ground, in the middle of the room; there was no candle in either of them.‡

On proceeding to examine the ashes, or remains of the combustion, there was found, in front of the spot which had been occupied by the bed, the extremity of a leg covered by a stocking, with a shoe on the foot, and which was recognized to be part of the right leg of the servant. It was the only portion of the body of this woman that had not been reduced to ashes.§

The cranium of the mistress, devoid of the scalp, which had been burned, was found in a situation corresponding with that in which the head would be as the woman lay in bed. This was the only portion of her body that had not been utterly destroyed by combustion, excepting a small fragment of the neck, or rather the skin of the neck, that had been enveloped in a red kerchief, which had probably served as a cravat, and of which there were yet some remains immediately attached to the preserved portion of the neck.

The servant's bed, which was very near to that of the mistress, was untouched, as well as the table, chairs, and other furniture of the room, excepting a wooden clock, hung up against the wall beside the bed,|| that, having preserved its form, fell into ashes on the first movement.

Although the room had no ceiling, the beams and rafters, which

* From the tenor of this expression, and the want of notice of any other part of the house, there is reason for inferring that the house of Mrs. P. consisted of but a single room, the door of which opened into the street. It is not at all rare in France for persons having as much property as Mrs. P. had, to live in a house so constructed.—EDITOR.

† "The curtains were woollen."

‡ "A lighted candle had probably been placed in that situation in the middle of the room, and had been entirely consumed."

§ "From the position of this leg, it was considered that the servant died lying across her mistress, with the right foot resting on the ground, and the left leg, as well as the rest of the body, on the bed. It was presumed that the servant had fallen in that position as she went to help her mistress."

|| It is not possible to discern, from the original account, which bed is meant: we have translated literally, and have preserved the narrator's distinction of paragraphs. From the servant's bed having been just spoken of, in the same paragraph, we have some reason for suspecting that is signified.—EDITOR.

were very near to the top of the bed, were not burned, but they were black and felt very hot. All the things about the room, especially such as were close to the bed, were extremely humid; which was owing, without doubt, to condensation of the dense vapours with which the room was filled on being first entered.

As there were no other persons in the house than these two women, and the accident not having been discovered until the ensuing morning, no one can possibly know the cause of it.

During the night of the 12-13th of January, (that of the accident,) the weather was very serene, the air dry, and the cold very severe, since the thermometer marked ten degrees below zero.*

The mistress was ninety years old; the servant sixty-six; they were both of a weak constitution, thin, and meagre; their food was but bad, although the mistress had an income of 6000 francs. She, for some time past, had drunk *eau de Cologne* to great excess. It is said, however, that for two years past, from the remonstrance and advice of her physician, she had taken it in a less quantity, which was indispensable for the support of her declining strength, especially as she had eaten hardly any thing since this habitual abuse of the *eau de Cologne*. The servant also ate but little; she now and then took a little brandy; but her nourishment consisted chiefly in good old wine, hot and well sugared. She often took this in sufficient quantities to make her tipsy. It is believed that the excessive cold of the night in question had led her to drink to excess.†

The Société Médicale d'Emulation proposes the following as a prize question: the memoirs, written in French or Latin, are to be sent, before the 31st of August, 1822, to the Secrétaire-générale, Rue Bertin-Poirée, No. 10, at Paris. The value of the prize is 500 francs.

“What are the dispositions and structure of the system of organs called the *nervous ganglions of the organic life, sympathetic nerve, great intercostal, trisplanchnique, &c.*?”

“What are the functions of this system of organs?”

“And, as far as is known, what are the diseases in which it is essentially affected?”

The Society proposed this question for the concourse in 1819. Two memoirs arrived: one of them approached the desired object; but the author of this memoir having made himself known before a judgment could be pronounced on it, and having besides withdrawn his memoir, the Society, to which this question appears too important to be thus abandoned, proposes it for another concourse. It requires that the candidates direct their efforts to replies to the three points of the question, founded on dissections made of human subjects and diverse animals, and on experiments and clinical observations: it is a memoir constituted of positive facts that is desired.

* Equivalent to 14° Fahrenheit.

† Dr. C. produces a long discussion on the circumstances above narrated, with the view of supporting the inference already stated; but we have not thought it necessary to transcribe it.

WE promised, in the last Number, to give an analysis of the contents of Dr. MAGENDIE's Journal of Experimental Physiology: want of space for it, however, obliges us to postpone it. The paper next in regard to its interest, for the purposes of practice, to that on *rabies*, is one on a *new extract of opium*, by Mr. ROBIGNET. We have just space enough for an abstract of this, on the present occasion.

It has been ascertained that the most active properties of opium reside in two substances which may be obtained in distinct forms, and which produce very different effects: *narcotine*, and *morphine*, as they have been named. The former is a very powerful irritant to the nervous system, as it appears from the experiments of Dr. Magendie, and it seems to be this substance which produces the effects which we so much wish to avoid in the administration of this medicine, and which are obviated, to a certain extent, by giving it in the form of the *black-drop*, or other analogous acid preparations. *Morphine*, according to the evidence of the same experiments, and others made on human beings, produces sedative effects without symptoms of the slightest irritation. To separate the narcotine, then, from the other parts of opium, is a very desirable object of pharmacy, and Mr. Robiquet says it may be easily effected in the following way:

"I macerate," says Mr. Robiquet, "common opium, divided into small pieces, in cold water, as it were for the aqueous extract of opium; I filter the solution, evaporate it to the consistence of a thick syrup, and treat it, in convenient vessels, with rectified ether; the whole is then shaken a great many times, before the ethereous tincture is poured off; this, being separated, is then submitted to distillation, that the ether may be drawn from it. This process is repeated as often as crystals of *narcotine* are obtained as a residue of the distillation. When the ether is without action, I evaporate the solution of opium to a consistence proper for pills; and I obtain, by this means, an extract wholly exempt from narcotine."

Dr. Magendie says he has employed this extract in practice for some time, and that he has hitherto seen only advantageous results from it, even in persons who derived no benefit from the ordinary aqueous extract.

As this account may, we suspect, not be perfectly perspicuous to such of our readers as are not versed in chemistry, we shall add a few explanatory observations. Ether, when added to the *thick aqueous extract*, dissolves the *narcotine*, whilst it has no action on the *morphine*: the latter is thus left behind in the aqueous extract when the ethereous liquid is decanted; and fresh ether is added to the aqueous extract, and poured off, (after having been well shaken with it,) as the former, as long as it is found that ether, so mixed with the aqueous extract, leaves behind no crystals of narcotine on being submitted to distillation. The aqueous solution left after this treatment contains the morphine, and has only to be evaporated to a proper consistence to be fit for medical purposes.

REPORT OF DISEASES.

WE omitted, in the last two Numbers of this Journal, to make any report of the prevailing diseases, because no new circumstance worthy of remark had occurred, and we were unwilling to draw upon the patience of our readers by ringing perpetual changes upon rheumatism and winter-cough—winter-cough and rheumatism. From this dilemma we have been relieved by the March winds, and find, upon consulting our register, the more interesting names of gastritis, enteritis, and plenitis. Now, although we would not be understood as enumerating either of the former among *prevailing* diseases, still the occurrence of individual cases which we have seen, if viewed in relation to the inflammatory affections of the chest, tend to illustrate the nature of the change which has taken place in the prevalent form of disease. This, which for some months had been chronic in its nature, having generally the mucous structures for its seat, has now become acute, attacking principally the serous membranes. This last remark, which in a general point of view applies to adults, does not hold good with regard to children; among whom inflammation, though much more acute than before, has almost invariably been seated in the mucous membrane of the lungs: indeed, acute bronchitis has prevailed among children like an epidemic, frequently coming on with so much severity as to render the most active remedies indispensable, and in some cases unavailable.

We had formerly occasion to allude to the prevalence of erysipelas; a considerable number of instances have continued to present themselves, and the disease has in general been more than usually severe. Indeed, during the last few months it has committed considerable ravages, and the occurrence of severe, and even fatal, cases has not been confined to London; many having taken place in military hospitals, notwithstanding the advantages of those over our civil institutions in this town, from purer air and disciplined regulations. It is singular, too, that it has prevailed to some extent in one regiment of Guards stationed at the Tower; while those in the west end of the City have been comparatively free from it. This would prove, if additional proof were required, the strong disposition evinced by erysipelas to occur as an endemic in particular situations. But some recent observations have led us to suspect that it is really contagious: amongst other reasons for this inference, we have known the same individual attacked two several times with erysipelas, during attendance upon a person labouring under that disease;—a coincidence so remarkable, even though it were singular, would point out this subject as worthy of further investigation.

During the early part of the month, when the weather was comparatively mild, scarlet fever, in children especially, began to appear in an epidemic character: we saw, in the same range of practice, nearly twenty cases in about a week, at that time; but not a single instance within the last fortnight. Scarlet fever was then taking the lead of bronchitis, as regards children, and whooping-cough was becoming rare: the latter affections have again resumed their former prevalence. The history of whooping-cough, in a general point of view, has been this:—Symptoms of bronchitis, without whooping, for about eight or ten days; then whooping, with less of inflammation; and, in about another week, or somewhat less, signs of great disturbance of the nervous system; and, when these are particularly severe, the case will terminate in death, if not well treated. The constant, and most remarkable, appearances on dissection, in these cases, are signs of inflammation about the medulla oblongata and upper part of the spinal marrow; and, almost constantly, (evidence of some degree of inflammation having been seated here, has never been wanting, though the signs of it have been more or less dissipated,) signs

of inflammation, of no inconsiderable intensity, in the mucous membrane of the lower part of the trachea,—the situation corresponding with that of the *inferior glottis* of birds, (we mean to offer a hint here to comparative physiologists,)—and about the bifurcation of the bronchia. We shall say more on this point at a future time.

Small-pox exists in several parts of the northern and western parts of London; and the unprotected subjects for it are not few amongst the poorer classes of the people.

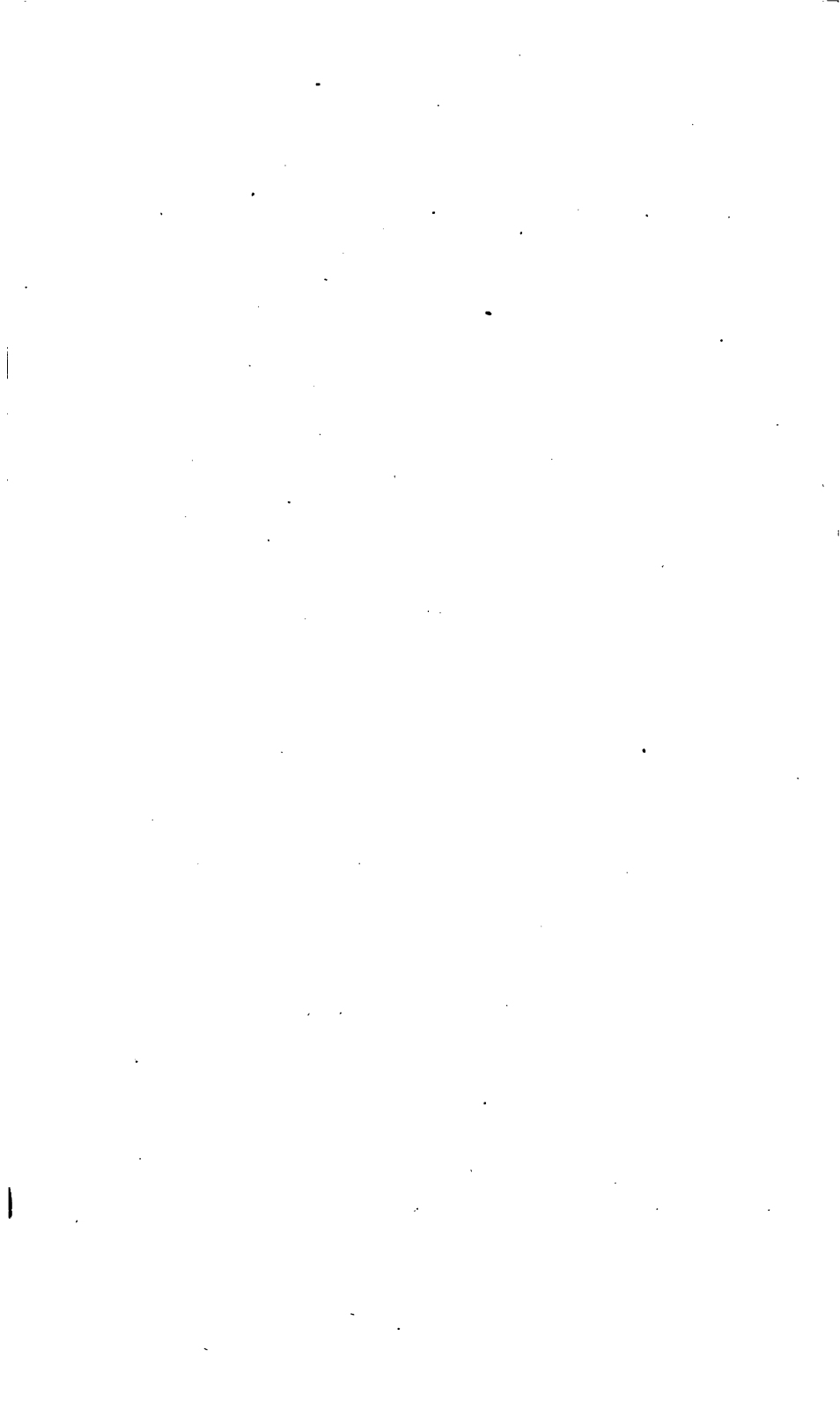
METEOROLOGICAL JOURNAL.

By Messrs. WILLIAM HARRIS and Co. 50, Holborn, London.

From February 20 to March 19, inclusive.

Day of Month.	Moon.	Rain gauge	THERM.			BAROM.		DeLac's Hygro.	WIND.		ATMOSPHERIC VARIATION.		
Feb.													
20			33 37 33			30.26 30.15		57 58	WNW	W	Cloudy	Fine	Rain
21		•03	36 40 33			30.20 30.24		60 60	NW	N	Cloudy	Fine	Cloud.
22			35 39 31			30.31 30 33		59 57	NNE	W	Cloudy	Fine	Cloud.
23			35 39 34			30.29 30.20		58 58	WSW	W	Cloudy		
24			36 40 33			30.15 30.12		60 59	E	ENE	Fog	Cloud.	
25	☾		35 42 34			30.09 30.07		59 59	NE	NE	Fog	Cloud.	
26			36 58 26			30.08 30.07		58 59	E	ENE	Cloudy		
27			29 31 28			29.87 29.64		57 59	S	E	Fine		
28		•10	29 31 31			30.32 30.30		58 58	E	ENE	Snow		
Mar													
1			33 40 38			29.37 29.64		60 64	SE	S	Rain	Cloud.	Cloud.
2			42 49 46			29.87 29.82		63 67	S	S	Fine	Rain	
3			49 55 48			29.77 29.67		69 71	SW	SW	Rain		Cloud.
4	●	•53	49 51 37			29.63 29.70		68 67	SW	W	Rain	Cloud.	
5			38 40 33			30.96 30.02		65 61	NNE	SW	Cloudy		
6			36 39 34			29.87 29.50		59 60	SSE	SSW	Cloudy	Rain	
7		•29	36 40 38			29.60 29.53		61 65	W	W	Cloudy	Rain	
8		•28	41 43 39			29.39 29.31		63 68	W	WSW	Fine		Rain
9		•05	45 56 48			29.40 29.63		60 62	SW	SW	Fine	Sho'ry	Rain
10	☾	•10	49 35 46			29.69 29.75		63 60	SW	SW	Fine	Hail	Fine
11			47 54 42			29.85 29.44		62 61	WSW	SSW	Fine		
12			43 51 44			29.96 30.01		60 59	SW	W	Cloudy	Fine	Rain
13			45 57 40			30.03 30.03		59 59	SW	W	Fine		
14		•05	43 53 37			30.08 30.32		60 55	NNE	NE	Fine		
15			40 51 37			30.36 30.36		51 52	SSW	S	Cloudy	Fine	
16			39 53 39			30.30 30.20		56 55	E	S	Fog	Fine	
17			41 55 39			29.91 29.72		51 55	SW	SW	Cloudy	Fine	
18	○		42 59 34			29.37 29.44		54 56	WNW	WNW	Fine	Stormy	Cloud.
19		•20	39 51 37			29.25 29.31		55 53	NW	NW	Fine		

The quantity of rain fallen in the month of February,
is 13-100ths of an inch.



Mr. Jukes's case of osseous tumors in the Uterus.



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NOTICE.

The Proprietors acquaint the MEDICAL PROFESSION, that the GENERAL INDEX to this Journal is published, comprising an Analytical Table of its Contents, from VOLUME ONE TO FORTY inclusive; carefully arranged in Alphabetical Order, with References to the whole of the cited Authorities, under their nominal Characters, &c. and forming an indispensable APPENDIX to the Journal; in one large 8vo. volume, price 21s.

Original Communications, Select Observations, etc.

An Account of some Osseous Tumors found in the Uterus of a Woman.
By MR. EDWARD JUKES, Surgeon, Accoucheur to the Westminster Medical Institution and Lying-in Charity.

[With an Engraving.]

ON examining, a few days since, the body of a woman, aged 67, who, as it appeared, died in consequence of chronic inflammation of the mucous membrane of the stomach and small intestines, I found the uterus present the following remarkable appearances. It was of the size of the head of a fetus of seven months, of a deep and dullish red colour externally; and the trunks of its principal blood-vessels were much increased in volume. The ovaria differed in colour from the uterus, having nearly the ordinary hue. The right one presented several points resembling the cicatrices formed after the escape of ova; in the left, only one mark of this sort was perceptible. Two vesicles projected from the surface of the right ovarium; one of them of the size of a lupin-seed, the other of that of a small pea: these vesicles, the coats of which were very thin and transparent, contained a yellowish fluid, which coagulated by heat. The cavity of these vesicles could be traced to only a small extent beneath the surface of the ovarium: their situation may be very well described, by stating that they appeared like vesicles which had been arrested just as they were on the point of total separation from the ovarium. Both ovaria contained several corpora lutea. The fallopian tubes were in the ordinary state, excepting that they partook of the inordinate vascular appearance of the uterus. The os uteri was much thickened, though without the least inordinate hardness: its

orifice was of about the size of a straw, and its margin very sharp. A canal, of nearly the same diameter as this orifice, extended through the neck of the uterus to a cavity of larger dimensions, which appeared to have been the proper cavity of the uterus. The structure of this organ was like a very dense cellular tissue, of a deep-red colour throughout, and about half an inch in thickness at the neck and the lower part of the body of the uterus; about the fundus it varied much, from the causes which will be presently described. Not the slightest semblance of muscular structure, or regular fibrous arrangement, could be discerned. Four distinct tumors projected from the exterior surface of the uterus: one, of about the bulk of a walnut, about the middle of the fundus; two, somewhat smaller, a little below it, posteriorly; and one at the anterior part of the uterus near the fundus. These tumors were seated in the midst of the parietes of the uterus; their surface was moderately smooth, and adherent to the surrounding soft parts: they appeared to have been enveloped in proper cysts, or a dense membrane had been formed around them by the adjacent cellular texture. One of them, on being divided by a saw, was found to be constituted of a very firm osseous mass, more compact towards the surface than in the centre, where it presented a cellular appearance, like that of the heads of the long bones in an adult. It had no distinct central cavity. The other tumors were not opened; but they seem to be of a similar kind.

On dividing what appeared, externally, to be the uterus, by a longitudinal incision from the fundus to the neck, the greater part of the mass was found to be formed by a large osseous tumor, which at first seemed to have been seated in the natural cavity of the uterus; but, on more minute examination, it was found that it was distinct from this cavity, and situate in the midst of the parietes at the fundus. This tumor was enveloped in a very dense cyst, by which it adhered to the surrounding soft parts: its surface was very irregular, being studded throughout, except at the somewhat distinct portion, A, fig. 1, in the annexed Plate, with small, rough, osseous protuberances; which were, however, covered by the cyst enveloping the tumor generally. The capsular membrane of the somewhat distinct portion, (thus A. referred to in the Plate, fig. 1,) was much firmer than that of the rest of the tumor, and presented a smooth polished surface. This was the most inferior part of the tumor, and was situate just over the cavity of the uterus.

The size of this tumor was that of a man's fist: its dimensions, as well as its shape, are accurately shown by the figures in the annexed Plates. Figure 1, represents the exterior appearance of the tumor; B, is the part of the surface studded with the bony protuberances; A, is a somewhat distinct portion

of the tumor, separate from the rest to the distance of three or four lines below the surface. On the tumor being sawed through the middle, and the two portions turned aside, the severed surface of one of which is shown in Figure 2, the greater portion of it, F, was found to be constituted of a dense cellular texture, interspersed with bony masses of various dimensions, from a portion of three inches in circumference, (A.) to others not larger than a pea. Some portions (as in B.) were constituted interiorly of a dense cellular substance; whilst others, and the greater number, were formed throughout of a mass of bone arranged in a cellular structure, somewhat like the heads of the long bones of an adult. c. fig. 2, show the interior distribution of the somewhat distinct portion.—A. fig. 1. This was constituted chiefly of a very dense cellular tissue, almost as firm as cartilage, having small osseous tubercles dispersed in it only here and there.—D. is a sort of subdivision of this distinct portion of the tumor, which was parted from the rest by a line of cellular substance: this subdivision was constituted of very dense cellular, or cartilaginous, substance, without ossification.—E. is another nearly distinct portion, formed of a more loose cellular texture.

The weight of the whole tumor was seventeen ounces.

There were no other remarkable morbid or preternatural appearances in the body than those just described, and such as were indicative of chronic inflammation of the mucous membrane of the stomach and intestines. This membrane was throughout studded with dark brownish-red patches, where it was much thickened, and in several parts disorganized: but, as these appearances do not appear to bear any particular relation to the tumor in question, I need not minutely detail them.

I did not see the subject of this disease until just the instant after her death, and I have not been able to collect any very particular information respecting her state of health and habits. There were no persons about her who knew much of her private life; nor have I been able to find any of her family relations. All I can learn, is, that she had been thrice married, —first, at some period before the twentieth year of her age; that she never had a child; and that her health had been tolerably good, until of late, when she experienced disorder of the stomach, which was attributed to the excessive quantity of gin she was in the daily habit of drinking. Her neighbours did not know that she had, for several years past, received any medical aid; and it does not appear that she ever complained, to persons about her, of any symptoms referrible to the disease of the uterus.

The recorded instances of osseous tumors seated in the cavity

or the parietes of the uterus, are by no means rare:* still I think the example of disease above related presents some circumstances which render it worthy of publication. It seemed to be particularly interesting to Mr. ASTLEY COOPER,† on my submitting it to his inspection; and this consideration alone would have been sufficient to induce me to offer the foregoing account for insertion in the Medical and Physical Journal.

London; Feb. 20, 1821.

Reply to Mr. Smerdon's Strictures on the Position of Fractured Limbs. By Dr. KINGLAKE.

THE few observations of mine which were published in the London Medical and Physical Journal for August last, have attracted the critical notice of Mr. Smerdon. The subject is certainly of sufficient importance to entitle it to all the consideration which my liberal commentator has thought proper to bestow on it. It was affirmed by me, that the question as to the preferable mode of treating fractures was still at issue; or, at least, that, in my judgment, the subject had not yet been brought to a satisfactory conclusion. It is therefore fairly open to discussion; and Mr. Smerdon has engaged in the undertaking with equal ability and candour.

Mr. Smerdon expresses much surprise at my regarding the management of fractured limbs, by placing them either in the straight or bent position, as still connected with theoretical considerations! He imagines that the luminous views taken of the subject by Pott and others, have removed all doubt on the propriety of the bent position, and have confirmed the improvement as a decided practical advantage; that theory has no concern with the obvious nature of the benefit: in fact, that it is an amendment too manifest to be reasonably denied. Much deference is due to the opinions of many of the advocates of the bent position in fractures, in the respectable list of whom Mr. Smerdon merits to be included; but yet an anxiety for the establishment of truth may be laudably indulged, in endeavouring to controvert what appears to be an unwarrantable assumption. When improvements are of such a nature as to accomplish all that could be desired, it would be equally

* Examples may be found in LIEUTAUD, (*Hist. Anat. Med.* tom. i.;) the *Memoires de l'Acad. de Chirurgie*, tom. ii.; the *Philosophical Transactions*, 1733; *London Med. and Phys. Journal*, vol. iii.; the *Ephemerides Natur. Curios.*; PARÉ, BARTHOLINUS, MORUS, and even in the writings attributed to HIPPOCRATES: though the precise nature of the whole of the cases related by the authors here referred to, is not clearly evident; as the older writers appear to have confounded osseous tumors with calculous formations, describing both under the latter appellation.

† It may not be improper for me to remark, that the preparation is now in the possession of Mr. Astley Cooper.

captious and useless to resist them; but, as long as events suggest a doubt of reputed correctness, whatever may be the questioned point, it is not only allowable, but necessary, that every just objection should be freely taken against it.

Mr. Smerdon quotes the following sentence from my communication, referring to the earliest periods of practical surgery: "To have insisted at that time that laying a broken bone in a curved position, would be favourable both to the osseous union and to the eventful [eventual] straightness of the limb, would have been thought unaccountably strange, and altogether untenable." Mr. Smerdon proceeds to observe, that "Dr. Kinglake has unintentionally written this sentence very obscure." It might here be justly retorted on Mr. Smerdon, that he himself has, unintentionally, it may be also presumed, cited this sentence not very "obscure," but very obscurely. "Eventful" and eventual, in reference to the subject of inquiry, sink the meaning below obscurity, by rendering it unintelligible. Mr. Smerdon pursues his criticism by observing, that, "in several places he (Dr. K.) has very improperly made use of curved instead of bent. Now, a curve, in the common acceptance of the word, signifies a part of the circumference of a circle; but, when a joint is bent, it forms an angle more or less acute, but certainly not a curve." This objection is raised on a verbal nicety,—on an attempt at accurate discrimination,—about which it is not worth while to dispute; but it would be inexcusably wrong to admit this gratuitous assertion, by conceding the obvious mathematical truth, that any deviation from the straight line, however small, is curvilinear; and, whether it be the minutest segment of a circle or comprising a large portion of it, still it is curved or bent from the straight line. The extremities of the human body, when bent or curved at the joints, are not in a straight direction, but describe portions of a circle with angular projections.

Mr. Smerdon is too sweeping, in assuming that the bent position in fractures is almost universally adopted. The exceptions are numerous and respectable. Instances sufficiently abound, both in the metropolis and in the provinces, in public establishments and in private practice, of the straight being preferred to the bent position. It must be owned that the modern management of fractures has been powerfully influenced, and in a great measure determined, by the doctrines and recommendations of public teachers, who have felt persuaded, from anatomical as well as physiological considerations, that the bent position ought to be adopted. With this strong bias, the young practitioner has resolutely pursued the admonition given, and could have had no motive for doubting its claim to preference, until undeceived and convinced to the

contrary by actual observation. With nosological references the most precise, with physiological estimations of the distinct and conjoint effects of muscular action the most profound, it has been confidently held that the straight position is fraught with constant irritation and eventual mischief; whilst bending the limb would anatomically keep at bay all obstacles, and mechanically insure the most quiescent and effectual union of the fractured bones. Notwithstanding the ample and consoling justification for the bent position, which may be derived from scientific speculations, from conditions that would appear to constitute and determine the physical necessity of the case, yet it has been often found that the straight position has been at once the refuge of the surgeon's skill and of the patient's suffering. The theoretic comforts of relaxed muscles and undisturbed bony union have been frequently tortured into the straight position, in defiance of muscular constraint, as the *solamen miseris*, as the only mode of avoiding the insufferable pains and evils of the bent posture. My own observation, and that of others, can bear testimony to the truth of this statement. A few years since, a message was referred to me to attend a consultation with two eminent surgeons, in a case of compound fracture of the tibia: the bone had been splintered, and a point, about an inch long, of the upper end protruded through the skin; the fracture had been ably reduced, the broken ends of the bone had been placed in exact co-aptation, and skilfully bent *secundum artem*. Involuntary muscular action repeatedly displaced the broken ends of the bone, and re-produced the original protrusion. This had occurred in several instances during two days after the fracture. At the period of my attendance, which was, if my recollection be correct, on the third day after the fracture had happened, the patient was in great torture at the broken part; had been sleepless; considerable tumefaction had overspread the limb, and the protruded bone was resting on the integuments, and lapping over the other fractured end. In this state, it was proposed to take off the protruded portion of bone, with a view at once to facilitate the necessary reduction, and to obviate the excitement which its irritating point was supposed to occasion. This proposition was resisted by my suggesting that a suitable extension of the limb should be made, for the purpose of reducing the fractured bone to its natural situation, and that the bent should be changed into the straight position, with proper bandaging to keep the fractured ends in due opposition. This altered treatment, with all the ideal difficulties of muscular constraint and tension, succeeded most perfectly. No further displacement occurred of the broken bone, nor any pain that prevented refreshing sleep; the existing tumefaction speedily subsided; and, after the lapse of about six weeks, the

most complete restoration of the limb was effected that could have been expected, or even desired. It may be proper to add, that the patient was a young man, of about twenty years of age, stout and muscular, and of a sanguineous temperament.

This is not a solitary instance of the beneficial effect of the practice proposed, but rather the common result of the straight position, when either originally adopted or subsequently resorted to under difficulties occurring in the bent posture.

Mr. Smerdon, with anatomical accuracy and precision, remembers and enumerates the names and actions of the muscles that are to be regulated and adjusted in the treatment of fractured limbs. He then imagines that the following sentence of mine, which he has quoted, contains a principle that is quite indefensible:—"The range of involuntary action given to the muscles connected with a fractured bone, when in a relaxed state, is such as operates most injuriously on the stationary position, in which it would be desirable constantly to confine the broken ends of the bone."—"Here," says Mr. Smerdon, "for reasons which I have before stated, I am entirely at issue with him, [Dr. K.] If distention," continues Mr. Smerdon, "be a powerful stimulus to muscular fibre, and if muscles are liable to involuntary action by being stretched, whether is it the more rational plan, in order to prevent that action, to relax them, or to place them in a state of distention?" Here appears to lie the gravamen of the question,—Whether a relaxed or tense state of the muscular structure immediately surrounding the broken bone, be the more favourable to a steady, undisturbed position of the fractured limb? The broken bone requires, for its speedy and uninterrupted union, the most undeviating state of mechanical rest. If this be at all disturbed, the eventual cure will be delayed, and probably rendered imperfect. Why were padding and bandaging always held to be indispensably necessary in fractures, but for the purpose of imposing on the part a constraint that should prevent any disuniting movement of the broken bone? Mr. Smerdon conceives that "distention," or rather extension, of a muscle, is a condition of high excitement; and that a relaxed state of that structure, is an abstraction of stimulus. This reasoning is applicable to muscular action, generally considered, but has little or no efficient relation to the muscular powers connected with the treatment of fractured bones. Nor even, generally speaking, with regard to muscular action, can it be conceded to Mr. Smerdon that extension, though a temporary, is a permanent stimulus to the muscular fibre. It is well known that it is the property of all stimuli to have their influence diminished by frequent repetition, and much more so by constant use. This is owing to the excitability of the part acted on being so lessened

as to lose its primary susceptibility for being affected. It is thus that the muscular structure connected with fractured bones soon parts with its first disposition to be inordinately excited, and at length becomes too torpidly passive to admit of being painfully stimulated by muscular extension and constraint. Muscular power exerted in a particular action for a long continuance, will be so exhausted as to incapacitate the structure furnishing it for strong contraction; so it is in the muscles that are bound up in a fracture, and laid in the straight position. Thus circumstanced, they soon become so worn and deprived of active energy, as not to occasion either pain or hurtful movement by the extended state to which they are subjected. Is it not constantly observed that the muscles of a fractured limb are, by disuse, much extenuated and enfeebled; and that they are only restored to healthy power and action by gradual and unremitted exertion? The topical constraint, therefore, that is imposed on the muscles of a fractured limb by being extended in the straight position, is highly conducive to preserving the broken ends of the bone in lineal connexion, and cannot be reasonably held to be a source of noxious irritation. The relaxed or bent position of a fractured limb certainly imparts to the contiguous muscles a sphere of motion in which involuntary and spasmodic contractions may take place, under the excitement of a broken bone, to an injurious extent; and which, in my judgment, is best obviated and provided against by the straight position.

The attempt steadily to fix the lower limb by comprehending the joints, as proposed, in the splints that may be employed, has more in it of form than reality. Unless the fixation that is applied to the joints be equally continued throughout the intermediate part of the limb, it can have no effect on the involuntary action of the muscles which are left at liberty, to be hurtfully excited by accidental occurrences. In fact, the main curative indication in the management of fractured limbs, is to avoid all motion of the broken bone, whether from adventitious causes or from muscular action, that might derange the lineal connexion of the severed ends, and induce unequal union and ultimate deformity. The constraint of mechanical confinement, carried no further than would be quite consistent with an unimpeded circulation through the bandaged part, and with the degree of pressure that would securely restrain, but in no measure paralyze, muscular power, is indispensably requisite; and most effectually insures early ease, progressive amendment, and, finally, the best attainable restoration of the fractured limb.

Although anatomical and physiological disquisitions may be plausibly indulged on the subject of the muscular structure;

instead of the broken bone, and the regulation of its power be regarded as the leading object, yet practical experience has sufficiently ascertained the visionary and fallacious nature of this scientific reasoning, and has satisfactorily proved that an extended limb may be more quietly and immoveably stationed for weeks together in that position, than could be effected by the bent posture. If this be the case, and of the truth of which there does not appear to be any doubt, the question may be tenaciously insisted on and ingeniously argued, but still its real merits will remain indisputable; and it is an object of great importance that its just claims should be recognized and allowed.

My observation may have been less extensive than that of Mr. Smerdon, but it has been fully sufficient to satisfy me that both the management and result of the straight position in fractures are far more advantageous than in the bent situation. Generally speaking, the attending inconvenience is less, and the restoration of the fractured limb more natural, in the former than in the latter mode of treatment.

It may be justly hoped, in the spirit of Mr. Smerdon's observation, that the equally cruel and reprehensible practice of periodically moving the broken ends of a fractured bone, to ascertain if the desired union had taken place, is by no means general, and that surgeons of intelligent reflection would deservedly reprobate it. But still it does occur; and, were occasions for noticing it so frequent with others as with myself, no question could be raised as to the existence of the mal-practice. It has even occurred to me to see it pursued in a public hospital, by surgeons of reputed skill and eminence, and that, too, with immediate and ultimate effects deplorably injurious. Binding up compound fractures after an exact reduction of the displaced bones, with suitable strictness, and laying the limb in the straight position, would appear to afford the best prospect of a speedy, an undisturbed, and an effectual cure. Effusions, suppurations, sloughings, and exfoliations, consequent on unnecessary exposure and the usual applications to the wounded part, are best avoided by effecting, under moderate pressure from bandage, an union of the soft parts by the first intention. This event, under the comparative quietude and stillness afforded by the straight position, may be generally accomplished in a manner equally gratifying to the feelings of the patient and to the reputation of the surgeon.

It is not to be inferred, because a preference would seem to be due to the straight position of fractured limbs, that, with vigilant and judicious management, the bent position may not also succeed very happily. The provisions of nature for re-uniting broken bones are so abundant and so efficiently opera-

tive, that mechanical obstacles only can impede the salutary progress of the natural work. The surgeon's province is to facilitate, by local arrangements, the process of osseous union; and adequate experience and dispassionate reflection will best enable him to make a correct election between the two positions offered to his preference. It is possible that the two postures may be a mutual resource for each other,—that changing from the one to the other, under circumstances of unappeasable irritation, might afford relief. It is not in my power to speak from observation of the benefit of the alternative from the straight to the bent position; but of that from the latter to the former, it may be truly asserted that immediate and progressive advantage will be found, if not uniformly, in most instances, to result from it. The evils of the bent position arise from the unconfined and almost incoercible motion of the broken bones and surrounding muscles. Insufficiently restricted, these parts are liable to hurtful movements. In the straight position, with a moderately strict bandage extended over the limb, somewhat concentrating its pressure on the fractured part, all disconcerting and painful movements of either bones or muscles will be effectually prevented; and, after the usual lapse of time, the limb will be restored to its natural shape and power of motion.

Tunton; Feb. 28, 1821.

History of a Case of Human Rumination; with an Inquiry into the Nature of the Process, and into some of the Phenomena of Digestion. With an Historical Relation of similar Affections. By JAMES COPLAND, M.D. Member of the Royal College of Physicians of London. (In a Letter to Dr. HUTCHINSON.)

PERCEIVING, in your Proëmium for the second semi-annual period of the last year, a case of human rumination referred to, and having lately had a similar affection brought to my notice, in an old and intimate friend, I am induced, from the great attention you have paid to physiological subjects in the direction of your Journal, to submit the particulars of this case, with some observations on the nature of the affection, to your disposal.

The novelty of such cases, and their physiological relations, led me to pay as much attention as was in my power to the particular phenomena constituting this affection; and the kindness and intelligence of my friend afforded me great facility in obtaining every information that I desired.

The subject of this case is a gentleman in the meridian of his age, of a strong but spare habit, and of the sanguineo-melancholic temperament. Owing to causes to which he was subjected through the very early period of life, he had been

obliged to take his meals in a very hasty manner. This was continued through the subsequent periods of early age, partly from the circumstances to which he was subjected, and in part from the activity of his mind in accomplishing whatever he was engaged in, whether serious employments or the pursuit of pleasure. The very few minutes allowed to his ordinary meals led to a hasty and imperfect mastication of the food; and, although his time was at his own disposal as he reached manhood, still the habit has been retained through the rest of the already passed portion of his life.

The greater part of its early period was spent in an active, varied, and pleasant employment, generally in the open air, and in the vicinity of the sea; and every moment that could be procured from his necessary avocations, was directed to study. This alternation of active exercise, in so healthy a situation, with mental exertion and periods of sedentary application, preserved the due equilibrium of the organic actions; the former neutralizing the effects produced upon the digestive functions by the co-operation of an hasty and imperfect mastication of the ingesta, and by sedulous study.

So long as this diversified mode of living was enjoyed, the regular operation of the digestive tube was continued, and no symptoms of dyspepsia appeared, until he took up his constant residence in the metropolis.

The dyspeptic symptoms appeared several years since, and at the commencement were only occasional. Flatulence, acidity, and cardialgia, chiefly prevailed; but at no time did he experience any loss of appetite, which through life has been uniformly good; nor at any period has he had to complain of any serious malady.

For a considerable time, the chief and almost only complaint was aepsia, or water-qualm: for two or three hours after every considerable meal, part of the more liquid contents was ejected from the stomach, in large mouthfuls, at intervals of from two to five or ten minutes, attended with a slight acidity; sometimes with a slight flatulence and sense of fullness at the stomach, but never with any cardialgia, nor with the slightest sensation of nausea. This affection was generally augmented by any of the usual articles of dessert, or by port-wine; while it was relieved, or entirely prevented, by a moderate quantity of white wine during and after dinner, and by avoiding every species of exertion that could tend to disturb the function of digestion. This affection, after continuing several years, with occasional interruptions, according to the care and means taken to prevent it, passed at last into complete rumination, which has been present after every considerable meal for some time. But, as it was attended with less inconvenience than the preceding

apepsia, and being unaccompanied with any disagreeable sensation, no great importance was attached to it, until it became complicated with a cutaneous eruption.

For that I was consulted; and, upon making inquiry into the state of the digestive organs, was readily informed of the ruminating affection. The professional intercourse that now took place furnished me with the particulars already related, which may serve as an introduction to a knowledge of the nature of this disease.

The following is a statement of the particulars of this case, when submitted to my care; for which I am considerably indebted to my intelligent friend.

The cutaneous eruption just alluded to, and to which my attention was first directed, owing to its being the chief object of concern with my patient, was the *lepra vulgare* of Willan; affecting the skin, to the extent of an hand-breadth, on the inner flexure of the knee-joint. It had been of two or three months' duration, and, with the exception of its apparent commencement in the same situation of the elbow-joint, it was present on no other part of the body. The ruminating affection was at that time generally present after all his meals, and constantly after breakfast and dinner.

The appetite was always good, and the food constantly taken in large mouthfuls, was masticated hastily and imperfectly, and swallowed eagerly. There was no thirst. Bowels were habitually costive. His sleep was sound.

His meals were taken more with a desire to satisfy an unpleasant sensation or a requisite desire, than to indulge the pleasures of the palate, and was performed hastily, in order that the studies and pursuits, to which he considered eating an interruption, might be immediately resumed.

Under the usual circumstances, rumination commenced from a quarter of an hour to an hour and a half after a meal. Immediately upon the commencement of this act, a slight sensation of fulness might be felt at the cardia, when the attention was particularly directed to it, that led to a deeper inspiration than usual. So soon as the act of inspiration was completed, and while the muscles of the glottis remained fixed, a bolus of the unchanged aliment rose rapidly from the stomach, with the first effort at respiration, at the moment when the diaphragm had just relaxed, and the re-action of the abdominal muscles commenced. But expiration did not take place until the alimentary ball had passed completely into the mouth, as the glottis remained closed until then: upon this having taken place, expiration was immediately effected; and so rapidly did expiration succeed to the regurgitation of the alimentary bolus, that the latter (unless when the attention was closely applied to the subject,) appeared as part of the expiratory act.

The ruminating process was never accompanied, at any time, with the smallest degree of nausea, nor any pain or disagreeable sensation. The returned alimentary bolus was attended with no unpleasant flavour, was in no degree acidulous, and was equally agreeable, and was masticated with additional pleasure, and with much greater deliberation than when first taken.

The whole of the aliments received at any one meal was not returned in order to undergo this process, only the part that had undergone an insufficient mastication; which indeed constituted the greater portion of solid aliment. That taken at the commencement of a meal was the first disgorged: this was ascertained by eating from a variety of solid dishes, or from partaking of different portions of the same. The more fluid portions were not always returned, unless along with the more solid or imperfectly masticated parts. When, however, the stomach was distended by a large meal, the fluid contents were frequently returned, and subjected to this process.

This affection may be considered as having been passively under the control of the will; and, although it sometimes took place when nearly unconscious of the process, yet it never occurred when the mind was incapable of being acted on by external impressions received by the senses. Thus, if at any time, from previous fatigue, and the concentration of the organic nervous energy towards the digestive organs, sleep was induced immediately after a full meal, this affection did not take place; but flatulence, acrid eructations, &c. afterwards supervened, and continued for some time, in consequence of the gastric juices being insufficient to the production of the requisite changes on the ingesta retained in a state of imperfect division. Very frequently, when the ruminating process was thus prevented, or voluntarily suppressed when circumstances required it, the ingesta, both fluid and solid, were returned at the end of several hours; but were then generally acid, frequently acrid and bitter, and sometimes in so large a quantity as to fill the mouth beyond its capacity of retention. But even then no cardialgia nor gastrodynia was experienced, nor the smallest degree of nausea; and even these disgorged matters were attempted to be masticated, although generally thrown out on account of the disagreeable taste.

In speculating upon the nature of this case, it appears evident that the energy of the digestive and assimilating organs were greatly diminished: consequently the stomach, deriving its influence, whether that presiding over the muscular action or vascular secretion, from the same source,—namely, the organic system of nerves,—experienced a proportionate diminution in its secreted juices. This was rendered apparent by the

changes which took place in the aliments, when taken even in very moderate quantity, and when retained without being submitted to remastication.

Connected with debility of this organ, an increase of its animal sensibility, which it derives from the distribution of the eighth pair of nerves, appears to have been present. Under these circumstances, the gastric juices (being, as inferred, in diminished quantity,) could be sufficient only for a small portion of aliment, which nevertheless had been taken in an abundant quantity; and, having combined with that part whose state is most favourable to such an admixture, and being, by the usual action of this organ, conveyed to the pylorus, the imperfectly masticated portions, and that part which remains unpenetrated by the gastric juices, must either continue at the cardiac extremity, or be propelled there by the action of the stomach. That the undigested portions of the food do not only remain in that situation, but may, by a peculiar and complicated action of this organ, be conveyed there, may be proved not only by reasoning upon the nature of its organic action, but has even been demonstrated by large fistulæ of this organ, situated at its anterior convexity, and opening externally at the epigastric region. In a case in the Hôpital de la Charité, under the care of Corvisart, the complicated action of this organ was witnessed, conveying the digested portions of the ingesta towards the pylorus, which passed only in very small quantity, while the bulk of the unchanged aliment was propelled, by a contrary action, to the opposite extremity of the organ. It cannot be supposed improbable that the irritation produced in this part of the stomach by the unchanged aliments in ruminating individuals, should excite the animal sensibility of this organ; and if the brain be in a state capable of receiving the sensation, it is propagated to the organs of respiration, and their action induced through the medium of the same set of nerves,—namely, the *par vagum*,—that forms not only the respiratory class, but also the connecting chain between the organic and animal orders of the grand nervous system; and, while it bestows an exquisite sensibility on the pulmonary system, it likewise gives a requisite, but sparing, share of its influence to this important organ.

In effecting the process of rumination, the organic contractility of the stomach can do no more than, by an elective process (soon to be explained), place the aliments about to be returned in a situation, in respect to the cardia, favourable to the excitation of the animal sensibility of this organ, and to its ready regurgitation and propulsion along the *cesophagus*. So soon as the demand is made upon the sensibility by the situation of the alimentary bolus, the *par vagal* class of nerves is excited

to action, and a full inspiration is effected, as has been described. The introduction of the bolus into the cardiac extremity of the œsophagus, may be considered as effected by the ordinary contractility of the stomach; perhaps sympathetically heightened at the moment by the re-action of the abdominal muscles; while, at the same time, the diaphragm has just undergone relaxation, in which the cardia may, from intimate nervous communication, suffer a similar participation, and thus give facility to the ascent of the alimentary ball in the œsophagus, which immediately contracts behind it from the irritation produced by its passage, and the bolus is thus conveyed to the mouth.

That relaxation of both the diaphragm and cardiac extremity of the œsophagus actually exists at the moment, although the glottis still remains closed, appears confirmed, both by the period of the respiratory act at which this process is produced, and from the circumstance that, when any restraint is exercised over this affection, it is principally by means of exciting the diaphragm to a frequent and continued action, when the premonitory sensation is felt at the cardia.

The influence of the will appears to be requisite, since the process is interrupted during sleep. But this influence is only passively engaged in the production of the ruminating act, by bringing about the co-operation of the respiratory organs.

The elective process exercised by the stomach in this affection, is similar to that which it employs in periods of health, and may be considered as relative to the degree of digestive energy, and to the comparative states in which the various ingesta may enter the stomach.

During the process of digestion, contraction takes place irregularly and in various situations in this organ, according as various portions of the longitudinal or circular fibres may act: this operates in producing a degree of arrangement in the aliments; and, as the gastric juices combine with the more soluble portion of the food, especially that situated towards the mucous surface of this organ, which, when duly effected, is conveyed, by the varying organic contractility of the muscular coat, towards the pylorus; while a successive and concentric stratum comes in contact with, and, if in a permeable state from its previous comminution and admixture with the salivary juices, is soon penetrated by, the secretions of this organ; and even the central mass not unfrequently is obliged to yield its more fluid parts to the exterior layer, when there is a deficiency of fluids in the alimentary contents. Hence the not unusual necessity for drink that takes place as digestion proceeds. In the course of this process, as it is the result of the healthy functions of the organ, the chyme in contact with its mucous surface

is conveyed in a direction from the cardia to the pylorus. But, if the propagation of the digested contents towards this extremity of the organ proceed faster than it can pass through into the duodenum, the accumulation of chyme that consequently takes place in that direction tends to propel, and even to return, the less soluble portions towards the cardia; where, according to the state of the organ, it may produce cardialgia, acrid eructations, or even rumination.

In the debilitated state of the stomach, and consequent deficiency of the secretions, digestion can be perfectly performed only when the aliments are presented in small quantity, and in a favourable state, from complete comminution and from intermixture with the salivary juices. If, however, in this state of the organ, the food is conveyed rapidly into it, possessed of neither of these requisites, so as to produce sudden distension, a re-action of this viscus upon its contents takes place; and, as the imperfectly masticated food constitutes the greater portion of the ingesta, there is abundance present to be returned into the cardia, while there is a deficiency of aliment in a fit state to combine with, or to be operated upon, by the gastric juices; which, when effected, is rapidly conveyed to the other extremity of this organ, by the re-action of the muscular coat, from the undue distension and the stimulus of solid contents. Thus a double effect is produced by the healthy organic contractility of this organ, when in a weakened state, and yielding a diminished quantity of the usual fluids; which state, indeed, may be considered as constituting this peculiar affection,—namely, that part of the aliment which is dissolved by the gastric juices is conveyed towards the pylorus, while, at the same time, the tonic action tending to diminish the capacity of the organ pushes the less comminuted and indigestible portions of food into the unresisting cardia; which is returned, as I have attempted to describe, in order to undergo a second comminution and intermixture with the salivary juices; after which it is in a fit state to be conveyed to its destination along the mucous surfaces, with the juices of which it combines, and thus permits a central portion of the mass to return and undergo a similar process.

Having occupied the attention of the reader so long, perhaps with a dry disquisition, on the succession of phenomena that appear to me to constitute this peculiar affection, I shall now very shortly direct his attention to the general plan of treatment which I pursued; after which, I shall exhibit a hasty sketch of similar cases that have attracted my notice.

In the curative plan pursued during the time this gentleman was under my care, the cutaneous eruption was viewed as originating in the long and progressive derangements of the diges-

tive canal; and the ruminating affection, from the highly intelligent history of its origin afforded me by the patient, as well as of the sensations and connexions of the phenomena so kindly accorded me during my attendance, was considered as the most advanced and peculiarly modified state of dyspepsia, or gastric debility.

With this impression, the indications that obviously presented themselves were correctly pursued. Having premised full evacuations of the intestinal and biliary organs, a course of tonics, combined with gentle and corroborative aperients, was entered upon, and pursued for some time, in conjunction with the warm bath and frictions of the whole surface twice or thrice a-week, and the usually-adopted stimulating ointments and washes to the diseased part of the cutis. Early in this tonic course, owing to the desire of my patient to have the cutaneous eruption as speedily removed as possible, I was induced to try the effect of the diaphoretics familiarly employed in similar affections. The various preparations of antimony were all found to increase the derangement of the stomach; guaiacum, either when combined with mezereon and sarsaparilla or with other medicines, produced the same effect; and the pilulæ submuriatæ hydrargyri compositæ, which I have frequently employed with advantage in cutaneous diseases, produced in this case great gastric irritability.

Owing to the peculiar state of the digestive organs, the diaphoretic plan of treatment, which was entered upon merely to satisfy the desire of my patient,* was entirely abandoned; and no medicines whose direct operation upon the cutaneous expansions could be looked for, were employed, unless a pill of four grains of myrrh and one of ipecacuanha, given night and morning, as an auxiliary to a tonic draught in the middle of the day, may be viewed as operating in that manner; as no doubt it indirectly might.

Under the use of infusions made from a combination of vegetable tonics, aperients, and aromatics, with the addition of an alkaline carbonate or a carminative tincture, and the frequent use of the warm bath, with subsequent friction; while, at the same time, a deliberate mastication of the aliments, and a moderate indulgence in light and digestible food, was enjoined; amendment soon became apparent. After a fortnight's continuance of this plan of treatment, the cutaneous disease had made considerable progress towards removal, and the ruminating affection, which till then had been present after every considerable meal, was now very seldom experienced; nor did

* It is not always a pleasant duty for a physician to treat the diseases of one who has no mean opinion of his own medical knowledge, although he may not possibly be a medical man.

any symptoms of dyspepsia take its place, unless when the injunctions regarding the mode of living and mastication of the food were not attended to, or when subjected to causes operating a diminution of the digestive energy; then, dyspeptic symptoms, or even slight rumination, occasionally presented themselves.

Within a few weeks, the eruption was entirely removed; but the ruminating affection returned whenever the proper precautions were not observed. Having impressed the mind of my patient with the necessity of pursuing correctly the plan I had prescribed to him, upon the grounds that such an affection, if indulged, would, by impeding the nutritive functions, gradually undermine the energy of his system, he became more attentive to the state of his digestive organs and to his mode of living: and now, for several months, he has enjoyed perfect health, and had no return of the ruminating act. Having transferred, as he says, the gratification formerly enjoyed in the second mastication to the first, this process is now performed more deliberately; a more complete admixture of the aliments with the salivary fluids and with the air, takes place; while the stomach is less suddenly, and much more moderately, distended: the action of the bowels has also become more regular.

In exhibiting the history of our knowledge of this disease, it is a matter of difficulty to determine whether it was known to the ancients, and, if known, in what light they viewed the affection; for I can scarcely call it with justice a disease, seeing that its possessors (from the cases I am about to exhibit,) did not consider it such, from its being, on the contrary, rather attended with considerable enjoyment. If we consider the habits and boundless luxury of the civilized among the ancients, the manner in which the stomach was unloaded of a previous meal, in order to re-enter upon a second gratification of the palate, among the Grecian and Roman gourmands, in their respective eras of luxury, it may be easily inferred that such an affection as that which I have now exhibited, would have been considered a most delightful source of animal gratification; and certainly would not have been the less indulged, nor would the enjoyment have been diminished, had a similar opinion been entertained by their physicians as was propagated by honest Fabricius ab Aquapendente, that the possessor was endowed with a double stomach, and that the other bestial concomitants might, in process of time, be expected, either in themselves or their more bestial descendants.

GALEN, who had ample opportunities of observation among the many instances of indigestion he must have met with in the luxurious, but peaceful, court of the Antonines, does not give

the history of a single case,—(so far as the *Index* affords me the knowledge, for, in truth, it would take too much time to get through the text when indices can be obtained; and we all know that Galen wrote as much in his long life-time as would require an equal period of existence in many of our modern physicians to read;—and, amid the various stomach-aches and affections of Marcus Aurelius, which, it would appear, both puzzled the brain and excited the anxiety of this prince of physicians and truly great man, so as to make him afraid that a glass of spiced wine might be too hazardous a remedy for the good emperor, the faculty of regurgitating his meals for a second mastication, appears not to have entered into the number. Unless we suppose that, not being considered a disease, the interference of Galen was not required, upon the ground that matters of taste, in the animal as well as in the mental application of the word, give a heightened enjoyment from their deliberate rumination.

FABRICIUS AB AQUAPENDENTE (*Op. Anat. Physiol.* p. 11; fol. 137,) furnishes two of the earliest instances of human rumination. The first is of a nobleman, in whom it generally took place an hour after his meals; which, whether solid or fluid, were always returned, in order to undergo a second mastication. Fabricius thought it proper to mention that the father of this individual had a horn growing from his forehead; and, with great good faith, adds, “ex quo forte datur nobis intelligi, parentis semen aliquam habuisse cum cornugenis animalibus, neque mirum fuisse genitum filium simile, quid a parente contraxisse:”—that, although the son did not inherit his father's horns, yet he possessed the accompanying faculty of rumination.

The second instance with which Fabricius has favoured us, was in a monk, who, although possessed of a most ravenous appetite, died of marasmus. This monk was possessed of still higher bestial attributes; for Fabricius describes him as having his forehead loaded with two horns; and Johannes Burgowerrus (*Dissert. de Rum. Hum.* Basil, 1626,) who visited this monk, in the company of Joh. Prevotius and Thos. Minadous, wrote a Dissertation on this interesting individual, and afforded Fabricius with the particulars which are inserted in his works. Burgower also adds, that the brother of this monk was also adorned with two budding horns, “Duorum cornuum vestigia gestasse,” as a *striking* feature of family likeness; or, as this author will have it, “Quod enim fratris erat, id monacho ruminanti simul gratis impertiunt.” But this illustrious individual did not ruminate, unhappily for the argument of Thos. Bartholinus, who, from these two individual instances, hastens to the conclusion (in his *Treatise de Unicornu*, cap. 2, and his

Anal. Historiæ, cent. 1, n. 39; et *V. Hist.* 16,) from the obvious analogy of the *cornugera pecudes*, that all human ruminants are possessed of horns; and also avers, that a double stomach will always be found on dissection. This piece of credulous doctrine cost the laborious Conrad Pyer, (in his *Merycologia*, p. 220,) no small trouble to refute; and he concludes in his turn, taking his honour to witness, (for he treats the subject with great gravity,) that this did not agree with his experience; seeing that many accounts have been given of horned individuals, none of whom ruminated.

DAN. SENNERTUS (*Med. Pract.* lib. iii. part i. sect. 11, cap. 8,) furnishes another history of a man of forty, who possessed this faculty from a child. He finds no difficulty in accounting for its occurrence, when he learnt that, when a child, this individual had lost his mother, and had been fed during his non-age with the milk warm from the cow. He accordingly soberly concludes, that he sucked it in with his nurse's milk: "*Quamobrem deficiente educatione, cum orbis infans, et institutionis humane inops nutricem vaccam observaverit tuereturque attentius, ispe ruminationi paulatim assuevit, sodalium familiaritate degemerans, &c.*"

PHILIP SALMUTH, (in the first Century of his *Observations*, p. 100,) furnishes us with another instance of human rumination. It always took place in this individual about a quarter of an hour after having left table. He always ate ravenously, and swallowed his food almost without any previous mastication.

JOH. FABER LYNCEUS, (in *Expositione Histor. Nardi Anthoni Recchi*, p. 630.) gives an instance of most obstinate rumination in a highly respectable German, who, even when seated over his cups with his friends, was always obliged to retire about half an hour after the meal into a remote corner of the apartment, and then ruminated the ingesta, undisturbedly and as quickly as possible; which having done, he enjoyed uninterruptedly the society of his friends. "Having been asked how he became obliged to indulge this propensity, he answered that from a boy he had been subject 'to acid eructations;' that, after having reached his thirtieth year, he found it impossible to resist admitting into his mouth the food that constantly regurgitated from his stomach. And, being farther interrogated whether the second mastication of his food afforded him any gratification, 'Indeed,' he replied, 'it is sweeter than honey, and accompanied by a more delightful relish.'"

This affection might be said to be in the family of this honest German; for he was blessed with two grown sons: the older, of twenty-four years, also possessed this delightful faculty, but had it more under control than the father, as he could prevent

it altogether when in company. The younger had not then come to its possession.

G. H. VELCHIUS adduces another example, (in his *Observ. Med. Episag.* xxxvi.) in an inhabitant of London; who, in the fortieth year of his age, and of sound health, always returned his food, in order to undergo a slower and more deliberate mastication. Rumination always took place in this individual from an hour to two hours after a meal, and even at the remoter period it still preserved a pleasant taste, and was without any degree of acidity.

In an instance adduced by DANIEL LUDOVICUS, (in the *Ephemerides Nat. Cur.* Decur. i. anno ix. and x. *Observ. clx.*) that occurred in a young woman, this act was not performed with the usual pleasure, and the returned food possessed a more disagreeable taste than that which accompanies perfect cases of this affection. Bitters and stomachic purgatives did not prevent its occurrence, which however was not always regular in its appearance; and, although cathartics and emetics prevented it for a short period, it soon returned. With all due respect for the memory of Dan. Ludovicus, I consider this to have been more allied to apepsia than to rumination.

JOH. CONRAD PYER, (*Mercurologia*, lib. i. cap. vi. p. 69,) mentions a case in a fatuitous* young man; also its occurrence in a rustic in Switzerland; and in a woman in the neighbouring town. He sagely endeavours to prove, from the circumstance of those individuals being rustics and cowherds, that the frequent sight of the ruminating process had impressed their brains with a similar propensity, which, although imperceptible in its progress, had nevertheless ripened into maturity.

SLARE, (in an early volume of the *Philosop. Trans.* the 193d article,) mentions the case of a Bristol man, who appears to have possessed this faculty in its perfection. This individual not only ruminated the solid ingesta, but also fluids, as milk and soups. There was, however, one imperfection connected with this case, as it relates to the state of this man's stomach during his meals; and which the very imperfect relation of the case (which was merely an answer to a number of queries transmitted from one of the Fellows of the Society in London, to an individual in the country, who merely contents himself with having answered the proposed questions,) furnishes this piece of information, that the victuals always seemed to descend imperfectly into the stomach, and to lie in the lower part of the throat. However, the portion of the meals first taken was first ruminated.

* I was lately informed, by a very intelligent individual, that a fatuitous boy resided in the village of Clapham a few years since, who constantly ruminated all his meals. This individual died at the age of fourteen.

In a case related in the 286th Number of the *Journal général de Médecine* by Mr. TARBES, as quoted by the Editor of the London Medical and Physical Journal, the prominent phenomena were nearly the same as the one I have just related.

Indeed, all the instances on record nearly agree in the individual phenomena constituting this affection, which, as it takes place by an unvarying mechanism, must, in its chief characters, remain unaltered, until it change into a different affection.

Dissections have not been able to throw any light upon this affection; nor can it be expected, in the present state of our medical knowledge, that, even in the event of a violent death taking place in a ruminating subject, that any visible alteration in structure could be detected. Fabricus ab Aquapendente and Thos. Bartholinus were confident of finding two stomachs, at least, in ruminating individuals, from the analogy of the cornuted tribes. Pyer, and Morgagni, (*de Sed. et Caus. Morb. Epist. xxix. art. 4.*) ridiculed the idea, and supported a contrary opinion, upon the ground that there were animals that ruminated without a double stomach.

The only instance upon record, to my knowledge, in which inspection after death took place, was in the instance of this affection occurring in a monk. This dissection is recorded both by Jo. Rhodius (*Cent. ii. obs. 59.*) and also by Bonetus, (in his *Sepulchretum*, l. iii. s. v. ob. 9 and 10 :) it was made by Franciscus Plazzonus, and is thus related by Jo. Rhodius:—"Monachus cum voluptate cibus ruminavit. Medici brutorum more gemino ventriculo præditum putabant. Ipso defuncto, F. Plazzonus œsophagum reperit undiquaque carnosum instar musculi, reliquis universi corporis partibus se recte habentibus." The physicians of the seventeenth century were not much enlightened by the opening of this monk, but their dreams respecting the existence of two stomachs were henceforth dissipated.

2, Walcott Terrace; March 13, 1821.

Thoughts on the Pathology and Treatment of Cyanche Trachealis, or Croup. By N. CHAPMAN, M.D. Professor of the Institutes and Practice of Physic and Clinical Practice, in the University of Pennsylvania.

TO this affection various other names have been applied, by the different writers who have treated of it. It is called suffocatio stridula, angina polyposa, asthma infantum, cyanache stridula, angina epidemica, morbus strangulatorius; and, in popular language, croup, or hives, the heaving of the lights or lungs, the choak or stuffing, &c. The best nosological title is *tracheitis*: it clearly designates the more ordinary

nature of the complaint, and at the same time gives uniformity to our medical nomenclature.

Croup has commonly been considered as a disease of modern date, and the credit of having originally noticed and described it is accorded to Professor Home, of Edinburgh, whose publication appeared about the middle of last century.* Turning over, however, one of the earliest volumes of the Transactions of the Royal Society of London, I find a very distinct account of the disease, illustrated by dissections. The writer, who was an obscure practitioner, describes it as an entirely new complaint, which had suddenly appeared among the children of Cornwall, committing very considerable ravages. It is also said to be particularly noticed by Martin Ghisi, an Italian writer, so early as 1749.†

Croup is, for the most part, confined to the early period of life, embracing the space between the first and fifth year, and affects chiefly children florid and robust. But I have known it to attack infants within the month, and also adult subjects. The illustrious Washington is said to have died of this disease. Two ladies of this city, who are now nearly in the meridian of life, I have attended in repeated attacks of croup: so strongly, indeed, are they predisposed to it, that they scarcely ever escape when exposed to the causes. The same liability has been transmitted to all their children, who are now numerous.

By some writers, however, it is asserted, that croup never occurs after the age of puberty. That it is a rare event, cannot be denied; nor, perhaps, is the fact without explanation. The parts constituting the seat of the disease undergo at this period a change, as is evinced by the new tone of voice acquired; which change enables them to resist those causes that, in the previous state of debility and relaxation of the larynx, more especially were invited to such morbid aggressions. Cases of this kind, however, are still to be considered as rare and anomalous deviations from the ordinary course and character of the disease.

Notwithstanding what has been so confidently alleged to the contrary, there is not the slightest reason to believe that croup is ever propagated by contagion. It would seem chiefly to arise from the influence of a moist and cold or austere atmosphere; and hence prevails more generally in the spring than at any other season, and near to the sea or other large collections of water, rather than in inland positions.

By some writers it is affirmed occasionally to occur as an

* 1765.

† There is, indeed, some reason to suspect that several of the much older authorities meant this disease, in the descriptions which they contain of a very fatal species of angina, without swelling of the throat.

epidemic, and perhaps this may be true. It is certain that the complaint is endemial to particular places, and within very narrow limits. Many situations on the sea-board are so particularly exposed to it, as almost to preclude the raising of children.

Croup has been divided into spasmodic and inflammatory, and not a little discussion has taken place on this subject. It would seem to me that, in all cases where it suddenly attacks, it must partake of the nature of spasm. Time is required to induce inflammation, which consists in an altered action of the vessels of a part, affected by comparatively a slow process. No cause, however, more rapidly promotes it than the disturbance occasioned by spasmodic constriction.

The early symptoms correspond with this view of the pathology of croup, and dissections fully confirm it; showing, where death promptly takes place, none of the phenomena of inflammation. But, under other circumstances, where the disease slowly approaches, or is the effect of inflammation of other parts, extending to the trachea, as sometimes happens in measles, scarlet fever, and most of the anginose affections, then it is of a contrary character; and *post mortem* inspections have revealed exactly such appearances as might have been anticipated.

Even, however, admitting the distinction contended for, I am not aware that it leads to any practical difference. Whether spasmodic or inflammatory, the directly depleting measures will be found equally effectual in the treatment. No remedy is so prompt in the reduction of spasm of high action as venesection; and none so unavailing or inappropriate as the anti-spasmodic substances. On this point I wish to speak emphatically, since some of the European as well as our own writers of high authority, entertaining other notions, have laboured to establish an opposite practice, consisting in the use of musk, assafoetida, and opium; than which nothing can be more false or prejudicial.

Croup variously makes its attacks. It commonly comes on at night, and sometimes without any premonition or exposure to its ordinary causes. The child wakes up with the hoarse, dry, stridulous cough, peculiar to the disease; which has been aptly compared to the sharp sound of the barking of a dog, and in other instances to the crowing of the cock. Concomitant with this, there is a distressing difficulty of respiration, menacing, in some instances, suffocation; with a flushed face, a quick irritated pulse, an unusual degree of restlessness and anxiety, with a sort of indescribable wretchedness. The child will not remain long in one position, nor can its complaints be in any way appeased. It whines, and cries, and frets, and seems

to be excessively uneasy, without suffering any very positive pain.

Cases of this nature are probably dependent on spasm, and terminate fatally in a very short time, where relief is not afforded. But, on many occasions, the disease advances gradually, with the ordinary catarrhal symptoms,—such as heaviness, suffusion of countenance, defluxions from the eyes and nose, a harder and more shrill cough than usual; and with various degrees of fever, which, with the cough, is always exacerbated at night, and especially after the child has slept. Completely formed, there is no material difference between the two species of croup; and henceforward their progress is nearly, or perhaps exactly, similar.

My mode of managing this disease is exceedingly simple, and has hitherto proved so successful, that I always approach it, in the early stages, with a greater certainty of curing it than any of the other complaints of infancy or childhood.

Called in the commencement of the attack, I endeavour at once to puke the child very freely; and for this purpose prefer the tartarized antimony, given at short intervals, as being one of the most certain and powerful of the emetics. At the same time I direct the child to be put into a warm bath for ten or fifteen minutes. This is a useful remedy: it rarely fails to promote the operation of the emetic, and will, indeed, alone sometimes cure the disease. The emetic, however, not operating,—or, if after its operation, the desired effect be not realized,—I then bleed copiously, and repeat it and the bath. An attack must be extremely obstinate if it do not now yield. Nevertheless, it will occasionally continue with little or no abatement; and, under these circumstances, I resort to topical depletion, by leeches or by cups. The cups should be applied to the sides or back of the neck; as, when placed anteriorly, they will, by pressure and suction, greatly impede respiration, and sometimes endanger suffocation. Twice I have seen the distress from this mistake so violent, that I believe death would have taken place had not the cups been removed. As means of local bleeding, leeches are very much to be preferred in such cases. Next I put a sinapism or blister over the throat; or, in some instances, these may be made to precede the former applications.

The foregoing remedies failing, or where the symptoms become so alarmingly violent as to demand immediate relief, I bleed *ad deliquium animi*. When pushed to this extent, I may almost say that venesection is invariably successful; as yet, I have never known one instance in which it failed. The moment that syncope takes place, the coarseness, cough, impeded respiration, and fever, disappear.

This valuable suggestion I derived from Dr. Dick, of Alexandria, one of the most original, bold, and successful practitioners of our country. It has been claimed, I understand, elsewhere, with what justice I pretend not to determine. That, however, the practice was adopted at least thirty years ago, by this distinguished physician, is unquestionable.

To prefer small and repeated bleedings at this period of the disease; as is advised by one of the most authoritative of our own writers, is a pernicious abuse of an important remedy. It may be laid down as a rule to which there are few exceptions, that, in acute diseases, where venesection is at all demanded, it should in the commencement be so copious as to produce decisive effects. The rationale of the measure seems not to be well understood. Detractions of blood, in a small or large quantity, operate, as remedial processes, very differently: the former *abates* action only; while the latter *alters* it, or so far reduces it as to enable the natural energies of the system to subvert or overcome it, and to re-establish health. Of this principle we have illustrations in pleurisy, in fevers, and many other affections, where a single profuse bleeding, timely recurred to, arrests the progress of the case.

Conceding that the loss of blood is necessary to a cure, it will be proper, under the circumstances stated, to pursue this course, even where we have grounds to apprehend debility. As small bleedings require to be often repeated, the aggregate of blood lost becomes ultimately greater, and more exhausting in its effects. Besides which, as there is less structural or functional derangement, the convalescence is more rapid and complete. Whether, therefore, with a view to a prompt cure, or to economize the resources of the constitution; or as a security against relapses or imperfect recoveries, this practice claims a preference.

The disease being broken, which is shown by the removal of the preceding symptoms, and even still more by the restoration of the natural susceptibility of the system to the action of medicine, I administer calomel, not in small and repeated doses, as is more generally advised, but in the largest possible dose, in order that it may speedily and most actively purge. In this particular stage of the disease, a thorough opening of the bowels carries off the lingering symptoms, obviates a relapse, and confirms the convalescence. But, should cough or hoarseness, with tightness of the chest and deficient expectoration, remain, I employ the polygala senega as an expectorant. It is in extinguishing the remains of croup, that it displays, I think, not the least of its valuable properties. Doubtless, however, it may be used at an earlier period of the disease with advantage as an emetic; though still I prefer the tartarized antimony.

The practice, as here detailed, is applicable chiefly to croup in its forming and early stages. At this period the disease is restricted pretty much to the upper portion of the trachea, and consists either in a spasmodic constriction of the glottis or inflammation of the membranous lining of the larynx: but, permitted to continue for ten or fifteen hours, and sometimes even in a shorter interval, it extends itself to the bronchiæ and into the substance of the lungs, producing, sooner or later, vast collections of mucus and phlegm, or exudations of coagulable lymph, or an engorged state of the pulmonary organs with blood.

The symptoms at this critical conjuncture are materially different. Now we have all the manifestations of an interrupted and defective circulation. The lungs, loaded and oppressed, very imperfectly execute their functions. The complexion is mottled, and the cheeks have a circumscribed flush, with some mixture of lividness. The eyes are prominent and inflamed. The pupil is often widely dilated; attended by an expression of countenance wild, haggard, and ghastly. The respiration is exceedingly laborious, with a full and disturbed pulse; or the child, sinking under the disease, has its breathing rather more tranquil, with a weak and irregular circulation.

The symptoms in these different states of the lungs are so analogous, that it is not easy to establish, in all instances, satisfactorily, a diagnosis: but, though difficult, it is a point of some consequence to be determined, as the treatment in every respect is not precisely the same. To arrive at a just conclusion, we must take into view all the circumstances appertaining to the case in its several stages, as well as the existing appearances.

Of the nature of bronchitis, and especially of that form of it which resembles catarrhus suffocativus,—or, in other words, when it proceeds from collections of phlegm, or mucus, or lymph, in the bronchiæ or pulmonary cells,—I have mostly found that the case has had its origin in catarrh, and which has run a course more than ordinarily protracted. There is also at the time greater or less discharge from the lungs, or at least evidence of heavy accumulations of matter, with an inability to throw it up; and to which may be added, that the pulse is languid, and the surface cold and clammy. But, occasioned by sanguineous congestion, however oppressive the dyspnoea may be, there is little or no cough or pituitous discharge; and, what is very distinctive, an entire absence of the wheezing, so general a symptom in the first case. The respiration, however, is singularly hurried, panting, and laborious. The pulse, too, is full, though irregular and disturbed, and very readily compressible. Cases of this sort, moreover, are apt chiefly to occur in florid and

plethoric children ; or, as I have seen, in directly the reverse, the weak and valetudinary ; and generally this condition is disclosed at an earlier period in the disease.

The indication now, in each shape of croup, is to relieve the lungs of oppression, and to re-establish a free and equable circulation. To effect these purposes, the child should be placed in a warm bath, and, while there, copiously vomited by an active and stimulating emetic. The sulphate of zinc has been recommended, and is useful, though the tartarized antimony, with calomel and ipecacuanha, or the juice of garlic or onion, is preferable.*

But, in the second case, having pursued the same measures, we are also very cautiously to draw blood ; taking away a little at once, suppress the flow, and watch the effect on the system. Being beneficial, we may renew the bleeding from time to time, till our views in this respect are attained.

The necessity of such extreme circumspection in the use of the remedy in this case, is readily explained. Engorgement of the great viscera, and especially the lungs, takes out of the general circulation such a large portion of blood, and confines it so closely in the part, that any considerable loss by venesection is very sensibly felt, creating, in some instances, prompt and irreparable exhaustion.

Where the lancet is altogether forbidden, cups or leeches may be substituted, and will be most serviceable on the back.

In each species of the disease, the vesicating applications are highly important remedies. The blister should be put over the breast ; or, if the case be so urgent as not to admit of delay, some means of more prompt vesication may be resorted to,—as cloths wrung out of hot water, or, what perhaps is better, pledges of lint dipped in a decoction of cantharides, made with the spirit of turpentine.

The subsequent treatment consists principally of the pretty constant use of expectorants ; and for this purpose the antimonial wine, the oxymel or vinegar of squills, the decoction of Seneka-snake root, either alone or in combination with the carbonate of ammonia, will answer exceedingly well. The hive syrup is here a very useful preparation.†

* These latter are very certain and active emetics, and will frequently succeed in exciting vomiting when the officinal articles have failed.

† This is a prescription of Professor Coxe, which I have reason to believe has justly acquired great popularity in the treatment of croup. It is prepared agreeably to the following formula ; and the dose is about a tea-spoonful for a child of one or two years old.

Syrupus Scilla Compositus.—R. Seneka snake-root, bruised ; Squills, dried and bruised, ʒ iiss. ; Water, ℥ ss. Boil together over a slow fire, until the water is half consumed ; strain off the liquor, and add, Strained honey, ℥ 4. Boil them together to six pounds, or to the consistence of a syrup ; add to every pound of this syrup, sixteen grains of tartar emetic,—that is, one grain to the ounce.

See *American Dispensatory*, 4th edit. p. 343.

Much may also be expected, in some instances, from the liberal exhibition of calomel. At all times an exceedingly active expectorant,—by which I mean whatever enables the bronchial structure to disengage and expel an oppressive load of matter,—it seems, under these circumstances, occasionally to operate with really a specific efficacy. There are some, indeed, of the respectable practitioners, both of this country and of Europe, who trust almost exclusively to it.

Calomel was originally employed in croup by the late Dr. Kuhn, of this city, who prescribed it so early as the year 1770. The Scotch physicians are devoted to the remedy, and consider it almost infallible; or such rather seems to be the opinion of some of the most distinguished of their writers. By one of them it is said, “that, in every case where it was employed previous to the occurrence of the lividness of the lips and other mortal symptoms, it has completely succeeded, both in curing the disease and in preventing any shock to the child’s constitution.” His manner of exhibiting calomel would appear daring, even to rashness, were we not acquainted with the insensibility of the system in this disease to remedial impressions of every description. To a child of two years old, he has given upwards of one hundred grains in twenty-four hours.

With Dr. Hamilton, to whom I have alluded, the professor of midwifery at Edinburgh, I am acquainted, and, from his high standing and character, I entertain not the slightest doubt, with some allowance for an undue enthusiasm of expression, of the veracity of these representations. Nevertheless, I will not take upon myself to support or recommend his practice. The mode which I have suggested of managing this disease, (at least as it appears in this country,) I must think decidedly more effectual, and certainly less hazardous, as well as repugnant, to popular prejudices.

In the preceding history, I have delivered, very concisely, some account of the pathology and treatment of croup. It results from what has been said, that I consider it at first as a spasmodic or inflammatory affection of the larynx; and in its subsequent stages as one or the other of the forms of peripneumonia notha,—either a congestion of the lungs with mucus or lymph, or with blood. The former I believe to be by far the most common occurrence, or usual shape, of the disease.

The practice appropriate to the several circumstances of croup, I have also endeavoured to point out with some degree of precision.

It will be perceived that, in relation to the latter stages of the disease, while I maintain that the lungs are affected differently in some cases, the only distinction in the treatment suggested, is the limitation of bleeding to the apoplectic condition of these

organs. Though I hold the other state to be essentially bronchitis, and hence originally of an inflammatory character, still, from the early depletory measures generally pursued, such no longer exists. We have, on the contrary, at this time, as its product, effusions or exudations, obstructing respiration. Yet, wherever there is reason to suppose a remnant of inflammation, topical bleeding, at all events, may be, and ought to be, practised.

What, on the whole, I wish especially to call attention to, is the view which has been presented of the nature of croup at an advanced period. It is interesting, not as mere theory, but as leading to the practical improvement on which I have dwelt. Though not generally entertained or adopted, it is most fully established, as well by the phenomena of the disease already noticed, as by dissections. To this point we have, to a certain extent, the testimony of Cheyne, who has written with ability on the disease, and the still higher authority of Baillie,—not to mention other names of less distinction,—all which has been confirmed by dissections conducted in this country.*

Not a little is said of the existence of a membrane in the larynx, and to which so much is ascribed in occasioning death, that an operation has been proposed, and even practised, for its removal. That it does occasionally exist, cannot be denied, though I suspect rarely, as I never met with it in my repeated examinations for this purpose.

The appearances I have observed in dissections relating to the larynx, were slight marks of inflammation, with more or less of mucus, such as is formed by all the secreting surfaces. Why I have not seen the membranous production, is perhaps susceptible of explanation. To throw out coagulable lymph of which it is composed, requires the vessels to be highly excited; a state which, by the copious depletion adopted in the cases that came under my notice, was probably prevented.

* Neither of these writers, however, have noticed the apoplectic state of the lungs. The venerable Dr. Bard, of New-York, who was among the very first to adopt a correct pathology of croup, says, in his Essay on the subject, published in the year 1771, that he has found the pulmonary organs so dense and solid, from sanguineous congestion, that they exhibited the appearance of the structure of the liver.

Baillie, in his *Morbid Anatomy*, tells us, "that, when the inner membrane of the trachea is inflamed, it is sometimes lined with a layer of a yellowish pulpy matter. This does not adhere very firmly to the inner membrane, but may be easily separated. It extends from the upper part of the cavity of the larynx into the small branches of the trachea, which are distributed through the substance of the lungs. There is, at the same time, a good deal of mucus in the trachea and its branches, together with a mixture of pus. This is the appearance of the inside of the trachea, in patients who have died from the croup." Cheyne's account of the *post mortem* appearances in the disease so closely resembles the preceding, that I consider it unnecessary to recite it.—*See his Essay on Diseases of Children*, Edinburgh, 1801.

Even, however, were we assured of its existence, I do not know that, in ordinary cases, the operation would do more than protract life. The disease at this time has reached the lungs; and hence no relief in this way could be expected. Yet it does sometimes happen, though seldom, that it is restricted to the larynx, and that respiration may be so obstructed by the membrane alluded to, or from an accumulation of mucus, as to threaten the immediate extinction of life. By the removal of the mechanical impediment, an operation might be useful, and has actually proved so in two cases recorded in the foreign Journals, in which relief was instantly afforded, and ultimately recoveries took place. I have now in my possession a drawing executed by my friend the late Professor Dorsey, representing the membrane in a position completely to intercept the passage of air to the lungs; and which, had it been displaced by an operation, as he strenuously proposed, the child would probably have been preserved.

In the estimate of this resource of our art, we ought, moreover, not to overlook the fact of the striking effect, in many cases, from the expulsion of the membrane by vomiting or coughing, and sometimes in a state of things, too, the most critical and alarming. Yet it seems that the operation, on the whole, is deemed a very desperate and precarious expedient, to be held in reserve only for the extremest emergencies, and where common measures have altogether failed.

Two causes have conspired to render croup, which is not necessarily a fatal disease, so much so, that it is placed by some writers even among the *approbria medicorum*, and by most practitioners is considered a highly obstinate and dangerous affection. The first is an erroneous notion regarding its pathology; and the second, the careless and feeble mode in which it is commonly managed.

An impression almost universally prevails, that children, owing to an extreme delicacy and frailty of constitution, cannot bear any very vigorous impression from remedies. As a natural consequence of such an opinion, the general practice in their complaints is extremely inert; exactly, indeed, of that kind which has been facetiously described as observing a strict neutrality between the patient and the disease, neither declaring for the one nor the other.

By no narrow or partial observation, I am thoroughly persuaded that the very contrary of this opinion is true. Children, I have remarked, display an uncommon tenacity of life and strength of constitution. They often survive under circumstances which destroy adults. They have been found living at the breasts of their mothers who had perished by exposure to cold, as is recorded by travellers and other writers. They

confessedly resist contagion better than grown people; and, when attacked, more certainly recover, not only from diseases of this description, but from all others, when properly treated.

Nor is this all. They sustain better the operation of the most active remedies, as vomiting, purging, sweating, blistering, and, I add without hesitation, bleeding.

During the growth of the body, the fluids, in relation to the solids, are larger in proportion, as is distinctly proved. This fulness of their vessels and greater excitability of system, render children peculiarly liable to inflammatory attacks. Nearly all their complaints partake of this character.

It follows, therefore, that they require oftener to be bled; and my own experience convinces me that venesection may be resorted to in their cases with more safety, and decidedly greater advantage. No one who is conversant with their diseases, and has practised venesection much in them, can withhold his assent from the accuracy of this statement.

Endowed with superior vital energies, children have, moreover, very extraordinary recuperative powers. They notoriously recover more speedily from wounds and injuries, and surgical operations; and recruit with greater rapidity, after being reduced either by disease or by remedies of any description.

It is on this account that, while there is any indication of life, however discouraging the appearances may be, we ought never to view the case of a child in an acute disease as altogether desperate; but, still retaining some hope, to continue to minister to the restorative principle of the constitution: and, were this course generally pursued, I am persuaded that we should not unfrequently be rewarded by such cures as reflect lustre on the art, and give to our skill a glorious triumph.

To do this, however, in the disease before us, the practice must be prompt and energetic, and our attendance unremitted till relief is afforded. It is a rule with me never to leave a child, in croup, till the alarming symptoms are over. This great degree of vigilance and attention are necessary, from the rapid career of the disease, and not less from the extreme and peculiar uncertainty of the operation of our remedies in it.

As a most formidable enemy in all its presentations, it should be attacked early, vigorously, and on the very outworks. Delay never fails to invigorate its force; and, when permitted to get possession of the citadel, or, in other words, a firm hold of the system, we shall find it always difficult, and often utterly impracticable, to dislodge it.

COLLECTANEA MEDICA:

CONSISTING OF

ANECDOTES, FACTS, EXTRACTS, ILLUSTRATIONS, &c.

*Relating to the History or the Art of Medicine, and the
Auxiliary Sciences.*

[This department is vacant, in the present Number, in consequence of the necessary length of the articles of Review which are inserted.]

CRITICAL ANALYSES

OF

RECENT PUBLICATIONS, IN THE DIFFERENT BRANCHES
OF MEDICINE AND SURGERY.

"I would have men know, that, though I reprehend the easie passing over of the causes of things
"by ascribing them to secret and hidden vertues and properties; (for this hath arrested and laid
"asleepe all true enquiry and indications;) yet I doe not understand but that, in the practical
"part of knowledge, much will be left to experience and probation, whereunto indication cannot
"so fully reach: and this not only in *specie*, but in *individuo*. Yet it was well said, *Vere scire
"esse per causas scire.*"—BACON.

*An Inquiry into the Nature and Treatment of Gravel, Calculus, and
other Diseases connected with a deranged Operation of the Urinary
Organs.* By WILLIAM PROUT, M.D. F.R.S. 8vo. pp. 227.—
London: Baldwin, Cradock, and Joy. 1821:

THE author of the present volume, it is said in the Preface, has been in the habit, for many years, of closely attending to the diseases of the urine; and this work exhibits an outline of his observations on the subject. "With his own observations," the author adds, "the principal facts and opinions of others have been likewise incorporated; while, on the other hand, to avoid controversy, whatever appeared doubtful has been in general omitted without remark. It was his original intention to prefix an historical introduction respecting the urine, with a detailed account of the chemical experiments on which many of his peculiar views are founded; but, upon reflection, he was induced to relinquish both these objects for the present, and to confine his attention chiefly to practical points. Chemical details could not, indeed, be altogether avoided, because chemistry constitutes the very basis on which the whole superstructure is founded: care, however, has been taken to render them as plain and concise as possible, and thus to present such a view of this part of the inquiry as may be intelligible to the general reader. To establish new views on medical subjects, is almost too much for an individual to hope. The author,

therefore, has chiefly confined himself to *illustration*; and, leaving it to the profession at large to establish his conclusions, (if they are capable of being established,) rests in the mean time perfectly satisfied that justice will be done to his attempts."

The task of the reviewer on this occasion is a very simple and easy one: he has but little to do with criticism; he will best perform his duty by giving as perspicuous an account of the author's *illustrations*, or observations of facts, as his abilities will permit; but, as the space consigned to him will not allow of a comprehensive exposition of those observations, he must endeavour to present such a view of them as will show the principal points of the evidence on which the author's inferences are established: so that, when these are brought forward as the most probable inferences,—as they appear to be,—the reader of this abstract may recognize them to be such. The facts related are to a considerable extent original: their great importance will be best proved by showing how fertile they have already been rendered, by the reflections of the author, in apparently useful pathological inferences and indications for the practice of medicine.

The work commences with an "Introduction;" in which the author presents a contrasted view of the elementary principles of the human urine and blood. After giving a description of the properties of these fluids, the author cites Berzelius's account of the relative proportion of the several *elements* (as far as properly regards the fluid in question,) in the urine; which is as follows:

	Water	933·00
Animal and destructible principles.	Urea	30·10
	Lithic acid	1·00
	Pure lactic acid, lactate of ammonia, and animal matters not separable from these	17·14
	Mucus of the bladder	·32
	Sulphate of potash	3·71
Alkaline and earthy salts.	— of soda	3·16
	Phosphate of soda	2·94
	— of ammonia	1·65
	Muriate of soda	4·45
	— of ammonia	1·50
	Earthy phosphate, with a trace of fluete of lime	1·00
	Silex	·03

1000·00

Healthy urine, when first voided, reddens litmus paper; and has, therefore, been generally considered as containing a free acid. Its specific gravity has been estimated, at a mean, to be about 1·0125.

Besides the principles just designated as the constituents of healthy urine, this fluid contains others, some of which, as well as some of those of healthy urine, do not manifest themselves in the blood. These relations are shown at one view in the following tabular arrangement of the constituents of the several fluids.

BLOOD contains		URINE contains,	
		Healthy,	Diseased,
Water		Water	
Albumen, fibrin, red particles		_____	Albumen. Fibrin.
_____		_____	Red particles.
_____		Urea	_____
_____		Lithic acid	_____
_____		_____	Nitric acid. Erythric acid. Purpuric acid.
_____		_____	Oxalic acid. Benzoic acid. Carbonic acid. Also xanthic oxide. Cystic oxide. Sugar.
_____		_____	Bile.
Lactic acid, and its accompanying animal matters		Lactic acid, and its accompanying animal matters	_____
_____		Mucus of the bladder	_____
_____		_____	Pus.
Sulphur. Phosphorus.		Sulphuric acid	_____
Muriatic acid.		Phosphoric acid	_____
Fluorine?		Muriatic acid	_____
_____		Fluoric acid?	_____
Potash. Soda. Lime.		Potash. Soda. Ammonia. Lime. Magnesia. Silex?	_____
_____		_____	_____

Having adduced those preliminary general observations, the author considers, in a particular manner, the properties of the several constituents of the urine, and the various conditions under which they present themselves. The *water* may exist in an increased or a lessened quantity in regard to the whole or part of the rest of the constituents, or its quantity may vary from the ordinary state with a correspondent variation of that of the whole or part of the other elements. Urine containing an increased proportion of water, is usually limpid and colourless, and may be easily known by its low specific gravity.

Albumen, fibrin, and the red particles, as already remarked, only appear in certain diseases. The albuminous matter is usually more analogous to that of the chyle than that of the blood: this curious fact will be noticed more particularly hereafter. "Urine containing chylous albumen is generally pale,

coloured, and, on being exposed to a temperature of about 150° , becomes opaque, and deposits this principle in a coagulated state. The effect is increased by the addition of an acid, especially the nitric acid; but the most delicate test of albumen is dilute acetic acid, and the prussiate of potash. *Bloody urine* is, of course, likewise albuminous; but is always more or less of a dark colour."

Urea is formed in the kidney, of some of the constituents of the blood, Dr. Prout says, "perhaps the albumen." The colour, and other sensible qualities of the urine, were formerly ascribed to this principle; but Berzelius proved that it is colourless, and Dr. Prout has shown* that it has no remarkable smell or taste. It commonly assumes the form of a four-sided prism. Its crystals are pellucid and colourless, and have a slight pearly lustre. The specific gravity of its crystals is about 1.350. Water at 60° dissolves more than its own weight of urea.

"An excess of urea in the urine seems to be characteristic of a peculiar form of disease, to be described hereafter. The mode which I commonly use to detect an excess, is to put a little of the urine into a watch-glass, and add to it carefully nearly an equal quantity of pure nitric acid, in such a manner that the acid shall subside to the lower part of the glass, from its greater specific gravity, and allow the urine to float above it. If spontaneous crystallization takes place, an excess of urea is indicated; and the difference of excess can be inferred, near enough for practical purposes, by the greater or less time which elapses before the crystallization takes place, which time may vary from a few minutes to two or three hours. Such urine is commonly, but not always, of a pale colour."

Dr. Prout says, he knows of no disease characterized by a diminished proportion of urea. In diabetes very little is present, but the other saline principles are also relatively deficient.

Lithic, or *uric*, acid, though not found in the blood, appears to be always present in healthy urine; and, in a pathological point of view, it seems to be the most important of its principles. Dr. Prout has some peculiar opinions respecting the mode of its existence in healthy urine, and the modifications which it is capable of undergoing, the arguments for which he exposes in detail. Berzelius appears to consider that it exists, at least in part, in the urine in a free state, and, consequently, that it is held in solution merely in virtue of its solubility in water; and this seems to be the general opinion. But, from its existing in the urine in a greater proportion than that in which it is soluble in water in a free state,—1000 parts of urine containing one part of lithic acid, according to Berzelius; and

* *Medico-Chirurgical Transactions*, viii. p. 529.

one part of this acid requiring at least 1720 parts of water at 60° (at which temperature uric acid is not precipitated from healthy urine,) to dissolve it, according to Dr. HENRY, and at least 10,000 times, according to Dr. Prout,—and from several other reasons, some of which are the results of his proper experiments, Dr. Prout infers, as a probability, that the lithic acid in healthy urine exists in a state of combination with ammonia, (which combination is much more soluble than free lithic acid,) and that in reality urine contains no uncombined acid at all.

Lithic acid combined with diluted nitric acid forms what Brugnatelli described as a peculiar acid, under the term *erythric acid*: this compound may be obtained in the form of pellucid colourless crystals. The addition of ammonia to a solution of these crystals, produces a fine purple colour; the same solution treated with potash and sulphuric acid, in a way formerly described by Dr. Prout, (*Philos. Transac.* 1818,) produces *purpuric acid*, which assumes the form of a yellowish or cream-coloured powder. Such changes in the urine sometimes happen, in certain diseases, “either by the action of the kidney, or the natural operation of the various principles existing in the urine upon one another;” the mode of which operation Dr. Prout proceeds to attempt to explain. The amorphous, or uncrystalized, sediments, usually denominated *pink* and *latteritious* sediments, and supposed by Mr. Proust to constitute a peculiar acid, which he named *rosacic*, have been found by Dr. Prout to consist essentially of the *lithate of ammonia*, and sometimes of the *lithate of soda*; and he considers that they owe their colour partly to the colouring matter of the urine, (to be described hereafter,) and partly (in some instances almost entirely,) to the *purpurates of the same bases*. The reasons on which those inferences are founded are given in detail, and they are such as renders the truth of the author’s inferences in the highest degree probable; but it cannot be absolutely proved, because the proportion in which the alkaline purpurates exist in the amorphous sediments—admitting their existence—is too small to permit them to be separated so that their presence may be demonstrated.

In some forms of disease, Dr. Prout says, the urine does really contain an uncombined acid, (which will be pointed out hereafter;) and in this case the lithate of ammonia is decomposed, and the lithic acid deposited in a crystalline form, and nearly pure; thus constituting the disease called *gravel*. When the compounds of the lithic acid exist abundantly in the urine, this fluid is almost invariably of a deep colour; and, if at the same time a free acid be present, the urine is for the most part unusually pellucid, and free from mucus.

"In a pathological point of view, phosphorus and its compounds particularly claim our attention. I am not acquainted with any disease connected with the simple absence of phosphorus and its compounds from the urine; though the existence of such a disease is not improbable, when we consider that health is always accompanied by the due separation of a certain proportion of these principles from the economy. On the contrary, cases where this acid exists in the urine in a free state, and apparently acts as a precipitant of the lithic acid, are by no means uncommon. Phosphoric acid, however, becomes most formidable when the earthy bases, lime and magnesia, are secreted in greater abundance than natural; which, by combining with the acid, form insoluble phosphates, and thus constitute by far the most distressing species of gravel and calculus. In healthy urine, this acid, like the sulphuric, appears to exist principally in union with potash, soda, and ammonia, and partly, perhaps, with lime and magnesia; the different salts being, from the excess of acid, in the state of superphosphates. Phosphoric acid is shown to exist in the urine, by its yielding, with the nitrate of barytes, a precipitate soluble in nitric acid, and again precipitable from that acid, by ammonia, without decomposition."

Muriatic acid. Muriates.—Muriatic acid exists in the urine, as well as in the blood, in combination with soda and potash: "thus appearing," Dr. Prout says, "to pass through the kidneys unchanged. This acid and its compounds, in a pathological point of view, are, perhaps, to be considered as the least important existing in the urine; no disease arising from their excess or defect being at present known."

Fluoric acid has been said by Berzelius to exist in the urine, in small quantity, combined with lime: but, Dr. Prout remarks that, as far as he knows, this observation has not been verified by any other chemist.

Soda. Potash. Ammonia.—The two fixed alkalies exist in the urine, as well as in the blood, in combination with the sulphuric, phosphoric, muriatic, and, according to Berzelius, the lactic, acids. "Ammonia exists only in the urine, apparently in combination with the muriatic, phosphoric, and lithic acids. No disease is known to arise from the excess or defect of the fixed alkalies; but the deposition of the earthy phosphates in the urine is almost always accompanied, if not immediately produced, by an excess of ammonia. Hence, in a pathological point of view, this is a principle of the greatest importance. The apparent source of the excess of ammonia is the urea."

Lime. Magnesia. Silica.—The author's observations respecting these substances cannot be given otherwise than in detail.

"Lime and magnesia exist both in the blood and the urine; but in very different states. In the blood, they appear to enter, perhaps as elements, into the composition of the albuminous principles; and hence cannot be obtained without combustion: in the urine, they occur chiefly in the saline state, apparently, as before observed, in union

with the phosphoric acid. I am not acquainted with any disease characterized by a deficiency of these earths in the urine; but the most distressing and dangerous form of calculous complaints is connected with, and indeed immediately arises from, their *excess*,—namely, the deposition of the earthy phosphates. In this form of the disease, the earthy bases seem to be separated in a much greater proportion than usual; while the quantity of phosphoric acid is relatively diminished. This deficiency of phosphoric acid, however, does not seem to arise from a deficiency of phosphorus, but from some defect in the oxygenating operation of the kidneys, by which that principle is permitted to pass through them unchanged: for the urine, under these circumstances, often seems to contain, in some unknown state of combination, even more phosphorus than natural. The urea, also, in this form of disease, exists in great abundance; but in some peculiar and apparently imperfect state, by which it is rendered extremely prone to decomposition, and liable to be converted into the carbonate of ammonia. Hence, the urine in this disease is either naturally alkaline, or speedily becomes so; and this excess of alkali contributes to the rapid union of the earthy bases with the phosphoric acid present, and their consequent deposition in the form of phosphates. Urine containing an excess of the phosphates is generally of a pale colour.”

“Silic,” Dr. Prout says, “has been stated to constitute urinary sediments, and even to form a part of urinary calculi in some instances: but this assertion requires to be better authenticated than it is at present, before it can deserve credit. This earth, however, ordinarily exists in the urine in minute quantity, according to Berzelius; but he supposes it to be derived from the water which we drink, which is not improbable.”

As this part of the work comprises so many principles which are constantly referred to in the subsequent divisions of it, we have thought it necessary to give a comprehensive abstract,—the means for testing the presence of the principles of the urine being alone wholly unnoticed, in the greater number of instances, because they are, in these instances, simply conformable with the general principles of chemical analysis;—and we shall, for the same reason, transcribe the whole of the author’s reflections on the facts which have been passed in review.

“From the preceding sketch, we find that the most striking differences between the blood and the urine, is the complicated nature of the latter. The astonishing variety of substances formed from such a paucity of materials, naturally leads us to reflect upon the vast extent of the operation of the kidneys. On considering, however, a little more attentively the nature of the operations of these organs, we shall find, as Berzelius has justly remarked, that *acidification* constitutes the chief feature in them. Thus, the sulphur and phosphorus of the blood are converted by the kidneys into sulphuric and phosphoric acids: a new acid, the lithic, is generated altogether, &c. Such, then, evidently is the natural and healthy operation of these glands. We find, however, that, in certain forms of disease, this acidifying tendency

is carried to excess, and nitric acid, oxalic acid, &c. are produced. On the other hand, it is occasionally suspended, diminished, or altogether subverted; and unchanged blood, or albuminous matter,—neutral substances, as urea or sugar,—or even alkaline substances, as ammonia, lime, and magnesia, are separated in abundance,—and the phosphorus and sulphur at the same time pass through the kidneys without being acidified. With respect to the character of the diseases attending these states of the urine, it will be generally found that, when acids are generated in excess, the urine is commonly small in quantity and high coloured, and the disease inflammatory: when neutral or alkaline substances, the urine, on the contrary, is generally pale coloured and larger in quantity, and the diseases are those of irritation and debility.

“With respect to the mode in which all the different substances existing in the urine are naturally combined, it is impossible to state any thing with certainty, except generally that the several acids divide the alkaline bases among themselves in the order of their respective affinities and quantities. The greatest difficulty which occurs among the salts, is with respect to the phosphoric and lithic acids and their compounds. There can be no doubt, however, as formerly stated, that the whole or both of these acids are in combination with some base or bases; otherwise the lithic acid could not be retained in solution. Yet the solution of these compounds reddens litmus paper very strongly; showing that the acids, though in a state of *combination*, are not in a state of *neutralization*, (two very different things, though frequently confounded with each other;) and we can only explain this by supposing that the affinity of the elements of the different salts are so balanced, that the ammonia of the super-lithate of ammonia, for example, is held too firmly in combination by its acid to be separated by the phosphoric acid of the super-phosphates.”

The “*diseases of the urine*,” the author remarks, are so extensively related with morbid conditions of the animal economy that there is some difficulty, at least in the present state of our knowledge, in devising an unexceptionable mode of arranging them. Perhaps, he adds, the most simple and obvious principle of arrangement is that founded on the *solubility* or *insolubility* of the elements met with in the urine. This he adopts, and, in conformity with it, he divides the subject into two sections:

“I. Diseases in which principles soluble in the urine are morbidly deranged in quantity or quality.

“II. Diseases in which principles insoluble in that secretion are similarly deranged.

“By this arrangement, we shall, indeed, as will be found hereafter, separate one or two diseases which appear to be closely connected with one another; but this defect, the consequences of which can be easily obviated, appears to me more than counterbalanced by the general conveniency of the arrangement in other respects.

“Under the first of the above general divisions will be considered,

“1. Various forms of albuminous urine.

" 2. Anonymous diseases, in which an excess of urea is the characteristic symptom.

" 3. Diabetes.

" Under the second,— all the various forms of gravel and calculus."

Dr. Prout does not, however, consider that the above list comprises all the morbid conditions of the urine; but they are, all those of which his knowledge is sufficiently precise to be laid before the public. An important circumstance in the history of morbid states of the urine, is a diminution or an increase of it, or more expressly, a diminished or increased separation of *water* by the kidney, without regard to the quantity of the other constituents. These diversities of proportion are connected with two very different states of the system. "A diminished flow of urine," the author observes, "accompanies active inflammation, and an inflammatory state of the system in general. The urine is invariably of a deep colour. An increased flow of urine, or *diuresis*, very constantly accompanies those diseases connected with a peculiar state of nervous irritability, as hysteria. It may be also produced by certain passions of the mind, as fear. Lastly, it may be induced by local irritants acting on the urinary organs themselves. In those cases the urine is always of a pale colour."

Under the title of *diseases in which the presence of an albuminous principle is the characteristic symptom*, the author does not comprise urine rendered albuminous by the presence of blood, but "a peculiar condition of that secretion, in which it is found to contain one or more principles, usually more resembling those met with in the chyle than in the blood." These principles sometimes exist in very large proportions, in which case the urine undergoes a kind of spontaneous coagulation; but it is more frequently smaller, when the urine is almost invariably pale coloured, and of a moderate or low specific gravity. Dr. Prout thinks that mucus derived from the prostate gland, which may be coagulated by heat, has sometimes been mistaken for albumen. Mucus is coagulated by dilute acetic acid, which is not the case with albumen.

"The symptoms usually attending this albuminous condition of the urine, are those of irritability. In slighter cases, there is generally a frequent desire to pass water, and for the most part decided diuresis. I have never known albuminous urine attended by positive pain; though the patient, for the most part, complains of certain indescribable sensations, which render him conscious that all is not right. In severe cases, where the drainage from the system is greater than natural, there are, as might be expected, an inordinate craving for food, and other symptoms somewhat resembling diabetes."

The history of a case, presenting several very interesting circumstances, in which this state of the urine existed in an

extreme degree, is then given in detail. When this case occurred to the knowledge of the author, he had already formed the notion that *chyle*, and not blood, is occasionally the source of the albuminous principle; he was therefore well prepared to recognize all the phenomena which seem to point out such an origin. Another case is then given, in which the albumen was less abundant, and which he observed before the notion just mentioned occurred to him; the history of which, nevertheless, presents similar indications. The author concludes his observations and reasonings on this point with remarking that, "however well-founded or important the fact of the presence of chyle in the urine may be, I cannot venture at present to draw any conclusion from it."

We may be permitted to request the reader to bear this circumstance in mind during the perusal of the review of the work of Dr. ALARD, in a subsequent part of this Journal, and to connect it with the fact that the colour, odour, &c. of certain substances taken into the stomach are found in the urine and not in the blood,—with the apparently well-authenticated cases of vomiting of urine,—and with some observations respecting the udder of the cow; which will appear in the next number of this Journal. Here will, we think, be presented what may warrant some new researches for communications between different parts, by means of absorbent vessels, that probably exist, and have not yet been discovered.

The slighter degrees of albuminous urine," Dr. Prout remarks, "in which the kidney may be considered as simply *passive*," may exist for years, without apparently producing any serious effects in the constitution.

Dr. Blackall attempted to show that, when the urine is albuminous in dropsy, the use of blood-letting in general is indicated; and Dr. Wells had already remarked that albuminous urine is "connected with too great action in some part of the system." Dr. Prout does not deny that blood-letting may here be proper; but, unless there are other symptoms present which seem to warrant that measure, he thinks that it should not be resorted to; and he seems to consider that, speaking generally, opium will prove more beneficial in this disease. He believes that the presence of albumen in the urine is, in some instances, the first step towards the conditions in which there is *excess of urea* and *diabetes*.

Whenever the specific gravity of the urine is high,—for example, above 1.025 or 1.030,—the proportion of urea, in common with the other principles, is necessarily larger than natural; and in this case crystallization will frequently take place, on the addition of nitric acid. "This concentrated state of the urine not unfrequently takes place in febrile and other diseases, and is quite unconnected with any disease of the urinary

organs, and appears to depend upon a diminished secretion of water only:" and, perhaps, we may add, from the water of the urine being partly removed after it has arrived in the bladder. In other cases, "an excess of urea, as compared with the other ingredients of the urine, is actually present."

"Those diseases in which an excess of urea may be considered as in some degree characteristic, do not appear to have been hitherto distinguished, but have been probably confounded with other diseases, and particularly with that form of diabetes which has been sometimes denominated diabetes *insipidus*. These diseases, however, differ considerably from diabetes, as the following observations will show.

"The average specific gravity of the urine in these complaints seems to be a little above 1.020, and occasionally to vary from 1.015 to nearly 1.030. Most generally it is pale, but occasionally it is high coloured, and exhibits somewhat the appearance of porter, more or less diluted with water; and this variety in appearance not unfrequently takes place in the urine of the same person. When first voided, it reddens litmus paper. For the most part, it is entirely free from sediment, except the mucous cloud of healthy urine; and the only remarkable property which it appears to possess, is that of containing abundance of urea; so that, on the addition of nitric acid, crystallization speedily takes place. From the quantity of urea present, it is very prone to decomposition, and soon becomes alkaline, especially in warm weather."

This state of the urine appears, as far as is at present known, to be referrible to no peculiar symptoms, either constitutional or local, excepting, of course, in regard to the latter, the frequent evacuation of the urine; which is, in some instances, effected more frequently than is explicable by the distension of the bladder. "In the few cases of this disease which have hitherto fallen under my own immediate observation," says Dr. Prout, "the subjects have been middle-aged men, of thin and spare habit, with a sort of hollow-eyed anxiety of expression in their countenance; free from gout and constitutional diseases in general; and, as far as could be ascertained, free from any organic defect in the urinary organs." The author has had no opportunities of ascertaining the progress of these diseases; but he thinks it extremely probable that, if permitted to proceed, some of them will terminate in diabetes, or in a deposition of the earthy phosphates. Their causes being so little understood, and their nature and origin being probably various, the proper mode of treatment for them cannot be determined, and must be founded on remote and more or less vague analogies. "In every case, however," Dr. Prout says, "which has hitherto fallen under my own observation or knowledge, sedatives, and particularly opium, have been the only efficient remedies." Two selected cases are related, which show that much alleviation, at least, of the disease may be obtained by

such medicines, combined with others appropriate for the removal of particular symptoms which may be present. Dr. Prout was induced to have recourse to opium in this complaint, from knowing the good effects of this medicine in diabetes; with which, as already remarked, he considers it to bear a close analogy.

The term *diabetes* is applied by Dr. Prout, after some other authors, exclusively "to those affections in which the urine is *saccharine*." The urine is here almost always of a pale straw-colour; its specific gravity has been stated to vary from 1.020 to 1.050. Dr. Prout says he has seen it higher than this, but never so low. We select the following from amongst the author's proper observations on this subject, as the most interesting of them.

"The quantity of urea is almost always very much diminished, though I have never met with a specimen in which it was entirely absent. It contains, for the most part, little or no lithic acid. The usual saline matters existing in healthy urine, are met with in diabetic urine in nearly the same relative proportions, but their absolute quantity is very much diminished. Sometimes diabetic urine contains a little blood,* and not unfrequently albuminous matter analogous to that of the chyle. I have seen it also contain a white milky-like fluid precisely similar to chyle, which slowly subsided to the bottom of the vessel. In this case the vinous fermentative process was indeed very rapidly in the urine, the chylous matter apparently acting like yeast."

It has not yet been proved that a saccharine condition of the urine may exist without an increase of the *quantity* of this fluid; but Dr. Prout thinks it probable that it does exist, in its incipient stages, for example, when the symptoms may be overlooked. He thinks it doubtful whether the increased quantity of the urine is a consequence of the simple saccharine condition, or whether it depends on other causes; but he remarks, in a note, that "the most probable cause of the increased flow of urine, is that irritable state of the system which forms a part of the disease, and which appears to resemble nearly that peculiar condition sometimes present in hysteria and other nervous affections, in which a large flow of limpid urine frequently takes place."

Two cases in which amaurosis accompanied diabetes from the first, and which apparently originated from the same conditions of the system as the latter affection, were related in a late Number of this Journal. We have seen a case where it was apparently related, in a similar manner, with a considerable degree of deafness.

Whether or not the quantity of the urine is the consequence

* See WATT'S Cases of Diabetes, pp. 47, 74.

of its saccharine condition, it seems, Dr. Prout says, to be in some degree a measure of the severity of the disease; for, the greater the flow of urine, the greater, for the most part, is its specific gravity and the proportion of sugar which it contains. Dr. Prout has suggested nothing original respecting the etiology of this affection. Nothing satisfactory has yet been advanced on this point. The cases adduced by Dr. Prout concur, with others which have been treated by the same medicine, to show the utility of opium, in large doses, which seems to be the most effectual measure hitherto employed. The advantages from animal diet and blood-letting, are not so well established: these means have lessened the quantity of the urine, but it is not certain that they have improved its quality. "I think, however," Dr. Prout remarks, "that there are stronger grounds for presuming that blood-letting has improved the quality of the urine, than that animal diet has produced this change."

The author commences his considerations on "*the diseases of the urine in which principles insoluble in that secretion are morbidly deranged in quantity or quality*," with a description of urinary gravel and calculi, and a summary account of their chemical composition, &c.

Substances deposited from the urine, though composed of the same general ingredients, may, the author says, in a pathological point of view, be conveniently divided into three classes: "1. Pulverulent, or amorphous sediments; 2. Crystalline sediments, usually denominated gravel; and, 3. Solid concretions, or calculi formed by the aggregation of these sediments." Amorphous sediments almost universally exist in a state of solution in the urine before it is discharged, and even afterwards until it begins to cool. Their colour is, for the most part, red, diluted with more or less of brown or yellow: they contain, at different times, almost every principle capable of becoming solid itself, or of forming a solid compound with any other principle of the urine.

"Generally speaking, however, they may be stated to consist of two species of neutral saline compounds,—viz. the lithates of ammonia, soda, and lime, tinged more or less with the colouring principle of the urine, and with the purpurates of the same bases, and constituting what are usually denominated *pink* and *lucuritious* sediments; and, secondly, the earthy phosphates, namely, the phosphate of lime and the triple phosphate of magnesia and ammonia, constituting for the most parts sediments nearly white. These two species of sediments very frequently occur mixed together, though the lithates generally prevail: and it is to this circumstance, and to the little tendency that the salts of which they are composed have to assume the crystalline form, that their heterogeneous and amorphous nature is to be referred."

Crystalline sediments are composed of—1. Lithic acid nearly pure, (always more or less of a red colour;) 2. The triple phosphate of magnesia and ammonia, (always white;) and, 3. Oxalate of lime, (of a dark blackish-green colour.) These different varieties of crystalline deposits are never voided together, though they not unfrequently occur with amorphous sediments.

With respect to the appearances and chemical properties of urinary calculi, the author adds nothing of much importance to what had been said by Dr. Marcet.

The author next treats of the comparative prevalency of the different forms of urinary deposits, and the order of their succession, accompanying his relation of facts with remarks illustrative of the etiology of those affections. He, in the first place, adduces particular accounts of the results of examinations of the largest collections of calculi in England, which he afterwards exhibits in a general view in the following table. The names connected with the specimens of calculi examined, are those of the chemists who examined them.

General character.	Particular Species.	Hunterian Museum, Mr. Brande.	Norwich, Dr. Marcet.	Guy's Hospital, Dr. Marcet.	Manchester, Dr. Henry.	Bristol, Mr. Smith.	Particular Totals.	General Totals.
Lithic acid	Nearly pure	16	66	16			98	294
	Mixed with a little oxalate of lime	6	71	74	6	
	Mixed with a little of the phosphates	45	45	
Mulberry	or oxalate of lime	6	41	22	11	33	113	113
Cystic oxide	1	2	3	3
Phosphates	Nearly pure	12	4	16	202
	Mixed with a small proportion of the lithic acid ..	66	18	84	
	Phosphate of lime, nearly pure	4	3	1	8	
Alternating Calculi	Triple phosphate, nearly pure	2	1	3	186
	Fusible, or mixed calculi	49	24	18	91	
	Lithic and mulberry	15	15	
	Mulberry and lithic	11	29	40	
	Lithic and phosphates	39	12	51	
	Mulberry and phosphates	1	16	32	49	
	Lithic, mulberry, and phosphates	
	Mulberry, lithic, and phosphates	5	7	12	
	Fusible and lithic	1	1	
	Fusible and mulberry	2	2	
Comp. Calculi	Composition not mentioned	6	10	16	25
	Mixture not mentioned	2	7	3	8	25	
		150	181	87	187	218		823

Our extracts from the author's remarks on the above data must be more partial than those we have given in respect to former subjects: those remarks will not admit of a perspicuous abstract; we shall, however, select a few of such as appear of most importance to medical practitioners in general.

It appears, from the foregoing table, that somewhat more than one-third of the whole number of calculi, in the collections referred to, belongs to the class in which lithic acid predominates: but, when it is considered that the whole of the calculi examined by Dr. Marcet were not sawed through,—that others were examined in a way not calculated to lead to precise results in this point of view,—and that it is universally admitted, by all authors on this subject, that lithic acid constitutes by far the most common nucleus round which other calculous matter is deposited,—Dr. Prout thinks that it may be safely asserted that “at least *two-thirds* of the whole number of calculi originate from lithic acid: that is to say, if a lithic acid nucleus had not been formed and detained in the bladder, two persons at least out of three who suffer from calculus would never have been troubled with that affection.” Dr. Prout points out the diversity of the proportion of *mulberry* calculi in the different collections, as a curious subject that cannot at present be accounted for. The proportion of calculi consisting chiefly of phosphates, appears, from the table, to be about one-fourth of the whole; but, from the mode in which some of them were examined,—some not being sawed through, and Mr. Brande, after having cut through them, having removed a portion of the whole cut surface by a file, in which way “all the different ingredients of the calculi were obtained” for examination,—it cannot be known whether several of those consisting chiefly of the phosphates had not a lithic acid nucleus. In the Manchester collection, where this point was regarded in the examination, Dr. Henry says that, “in four instances only out of 187, the calculus has been composed throughout of the earthy phosphates.”

The causes above noticed have also rendered the general results respecting the proportions of *alternating calculi* equivocal. The stated proportion of calculi of this species is hence found to vary more than what is probably really the case: thus, it is said to be only about one-fourteenth in the collection at Guy's Hospital, whilst it is one-half in that at the Bristol Infirmary. This, Dr. Prout shows, in another part of the work, to be a very interesting subject in a pathological point of view. He arranges the alternations of the layers of different calculous matters in the following manner:—1. Lithic and mulberry; 2. Mulberry and lithic; 3. Lithic and phosphates; 4. Mulberry

and phosphates; 5. Lithic, mulberry, and phosphates; 6. Mulberry, lithic, and phosphates; 7. Fusible and lithic; 8. Fusible and mulberry. The specimens of *compound calculi*,—that is, those “composed of different ingredients mixed up together,” are comparatively rare.

Notwithstanding the diversities in their composition and appearance, above enumerated, urinary calculi may, Dr. Prout remarks, “be considered as made up of four elementary substances only, viz.—1. The lithic acid and its compounds; 2. The oxalate of lime; 3. The cystic oxide; and, 4. The earthy phosphates; two or more of which principles are seldom or never found in excess in the urine at the same time. Hence they may be supposed to represent so many distinct diatheses, or conditions of the system requiring to be separately considered; and this accordingly is the principle on which the future arrangement of my subject will be founded.”

Having explained more particularly his reasons for adopting this arrangement, the author proceeds to treat “of the lithic acid diathesis in general, and on the best means of counteracting it, so as to prevent the original formation of calculus, or its recurrence after an operation.” The perusal of this disquisition must, very generally, be productive of extreme gratification, for many reasons, amongst which, not the least interesting one is this, that the subject is discussed by a thorough chemist, who has not permitted his knowledge of this science, and the peculiar zeal he entertains for it, to give an undue bias to his views in physiology.

When, from particular causes affecting the health, the quantity of lithate of ammonia in the urine is increased above the natural standard, the excess is precipitated as the urine cools, and thus constitutes one (the *amorphous*) of the two forms of depositions of compounds in which lithic acid predominates. When such a pulverulent sediment appears, then, the obvious inference is that there is an excess of lithic acid in the urine; and such is most generally the case, though not universally so, for the compounds of lithic acid appear to be sometimes deposited in consequence of a very slight excess of some other acid in the urine. The causes of this excess of lithic acid are stated by the author to be of three kinds:—“*a.* Simple errors in diet; *b.* Unusual or unnatural exercise, either bodily or mental, particularly after eating, and the want of proper exercise at all other times; and, *c.* Debilitating circumstances.” We must refer the reader to the work itself for details on these points; we can only adduce some hints of the general inferences of the author respecting their mode of agency. He considers that they, in the first place, derange, in some unknown way, the digestive functions, by means of which either chyle too

imperfect for the purposes of the economy, or some new and unnatural principle is generated, which, requiring to be removed from the system, is eliminated by the kidney, and by that organ converted into lithate of ammonia; and thus an *excess* of this substance is formed.

Several useful inferences and indications are derived by the author from the *colour* of the amorphous sediments, which he distinguishes into three principal varieties:—1. Yellowish or nut-brown; 2. Reddish-brown or lateritious; and, 3. Pink, sediments.* Those characterized by the nut-brown colour, (that of ripe hazle-nuts,) consist essentially of the lithate of ammonia, but usually contain also more or less of the phosphates; and generally, but not at all constantly, the nearer they approach to white, the greater is the proportion of the latter substances. “This class of sediments,” the author remarks, “may be termed the *sediments of health*, if the term may be allowed,—being such as are produced in the urine of healthy or slightly-dyspeptic individuals by errors of diet, and all the other circumstances before mentioned, which seem, independently of actual fever, to produce turbid urine.” When there is, however, an extraordinary liability to this affection, a tendency to excess of lithic acid is denoted that may lead to gravel and its consequences, of which it is almost constantly the forerunner.

The sediments of the second class vary in tint from nearly white (when they are with difficulty distinguished from the former,) to a deep brick-red or brown. They consist essentially of the lithate of ammonia, or lithate of soda, tinged with a large proportion of the colouring principles of the urine, and more or less of the purpurates of ammonia and soda. When the purpurates exist in the urine (indicating the secretion of nitric acid by the kidney,) “feverish or inflammatory action is almost constantly indicated;” and this law is so constant, the author adds, that he has never seen a decided exception to it. Amongst the diseases in which the colouring matter of the urine is secreted in a greater abundance than ordinary, the author mentions gout, rheumatism, and “hepatic affections.”

The pink sediments consist essentially of the lithate of ammonia, but they differ from those of the two former classes in being almost entirely devoid of the yellow tint derived from the colouring matter of the urine; and, consequently, in owing their colour chiefly to the purpurate of ammonia. “This class of sediments, therefore,” the author says, “appears to indicate the absence of the large proportion of the colouring principle

* A plate is attached to the work, in which the tints of the principal varieties of those several classes of sediments, as they generally appear, are represented.

of the urine, so constantly present in inflammatory fever; and to denote the secretion of a greater quantity of nitric acid, and the consequent formation of more of the purpurate of ammonia: and this view of the subject actually coincides with my observations. The most perfect specimens of this kind of sediment which I have ever seen, were obtained from the urine of dropsical individuals: they occur also occasionally in the urine of the hectic, and of those obviously labouring under certain chronic visceral affections, especially of the liver."

The presence of these sediments in the urine, the author considers, appears to show that fever has existed and is going off, rather than that it exists at present. They appear almost constantly in continued fevers; but this, he presumes, can be explained "upon the supposition that the sediments, for example, generated by the fever of yesterday, appear in the urine secreted during the remission of to-day; and those generated to-day, in the urine of to-morrow, &c." The length of time which the urine is retained by feverish patients must also be taken into consideration.

When discussing in a particular manner, in this part of the work, the causes of crystallized sediments or gravel, the author illustrates the operation of a free acid in the urine, by remarking that the addition of a few drops of any acid to healthy urine will produce a deposit of crystallized lithic acid: so that an excess of lithic acid is not always, though it is generally, present when this acid is precipitated from the urine. "I have frequently," says Dr. Prout, "seen the urine so completely divested of lithic acid in this form of disease, that, upon adding to it even an excess of a mineral acid, not another particle of lithic acid has been deposited." The free acid productive of this phenomenon is not always the same: "most generally," Dr. Prout says, "it appears to be the phosphoric acid, sometimes the sulphuric. I think I have also seen it take place from the nitric and erythric acids, and occasionally from some other acid of a destructible nature, which I was unable to make out. Even the carbonic acid may be occasionally the cause of this precipitation."

On treating of the conditions of the system under which gravel is formed, the author remarks that the symptoms in most instances are such as indicate some error in the digestive functions,—as acidity of the stomach, flatulence, &c.

Having discussed the history of this affection, Dr. Prout proceeds to consider the means by which urinary sediments may be obviated. In respect to the first class of amorphous sediments, he remarks that, when they occur only occasionally and not frequently, the affection "scarcely requires a formal treat-

ment with medicine, but a careful attention on the part of the patient to avoid all the circumstances (errors of regimen and the like,) which have been before stated to frequently give origin to this deposit in the predisposed." The treatment of the second and third classes must be absolutely regulated by, and adapted to, the nature of the disease of which they are a consequence. The treatment of gravel must be chiefly directed by the same principles. The use of alkalies is, however, for the most part, particularly indicated; "but they are seldom or never to be given alone, but, to be really useful, must be conjoined with alteratives and purgatives. The pil. submur. hydrarg. comp. or a pill composed of the pil. hydrarg. and antimonial powder, taken at night, and followed up the next morning by a solution of Rochelle salts and carb. of soda in a bitter infusion, may be had recourse to. A little of the same mixture may be taken two or three times a-day, so as to keep the bowels fairly open; or, instead of this, a little magnesia may be taken in a glass of soda-water, as often as it may be found necessary."

In the treatment of the most severe stage of these affections, or what is usually denominated a *fit of the gravel*,—in which, there is a union of the second class of amorphous with crystalline sediments, which, according to Dr. Prout, indicate fever, and inflammation, as well as the secretion of a great excess of lithic acid,—the principles of the treatment are similar to those proper for the milder stages; only that blood-letting, general, or local, or both; antimony, opium, or hyosciamus, in larger quantities than in the latter, are here also indicated; and Dr. Prout thinks it a matter of much importance that such measures should "precede the use of diuretic medicines."

With respect to the causes of the deposition of oxalate of lime, Dr. Prout remarks, we know but very little, the data are so imperfect: "I shall, however," he adds, "venture to make a few remarks upon the subject, which I leave my readers, if they choose, to consider as *conjectural*."

"1. We have seen that the oxalate of lime frequently forms renal calculi, which often increase to a considerable size in the bladder. From this I think we may infer, that the formation of this concretion is connected with a distinct diathesis excluding the existence of other diatheses; and that it is not an accidental occurrence happening in common with many others to the urine."

"2. From the dissection of calculi, it appears that the oxalate of lime diathesis is both preceded and followed by the lithic acid diathesis; a circumstance which is peculiar to these two forms of deposit, and which, when taken in conjunction with the phenomena and circumstances already related, appears to show that they are of the same general nature: consequently it is probable that,—

"3. In this diathesis, instead of lithic acid, the oxalic acid is generated; which, by combining with the lime naturally existing in the urine, forms the concretions in question.

"4. Is this oxalic acid actually secreted by the kidney, or is it formed afterwards by the action of the nitric acid upon some of the other constituents of the urine in the same manner that the purpurates appear to be formed? From various reasons which might be mentioned, the former opinion seems more probable.

"From all these observations taken together, I am induced to conclude that the oxalate of lime diathesis, though consisting in a manifestly deranged action of the kidney, and therefore distinct from the lithic acid diathesis, is nevertheless of the same general nature; and consequently that it requires a mode of treatment founded upon the same general principles."

After having given a summary of the cases of cystic-oxide calculi related by Dr. Marcet and Mr. Brande, the author adduces the following remarks:

"We know nothing of the state of the urine in this diathesis, or whether the calculous matter is ever deposited in the form of gravel or other sediment; but the probability seems against this supposition. We know very little, also, respecting the state of the general health; though I think we may conclude that it is not much affected, and that the cause of the disorder is rather a diseased or depraved action of the kidney.

"With respect to the medical treatment of this diathesis, we are equally ignorant. If the above view be adopted, that it is of the same general nature as the lithic acid diathesis, the nature of the mode of treatment will be obvious from what has already been said: but this must be decided by future observation."

The author's remarks on the "phosphatic diathesis," will not admit of a perspicuous abridgment: we shall extract from them some particulars which appear of very remarkable importance. After a general account of the constitutional symptoms, which "consist in great irritability of the system, and derangement of the chylopoietic viscera in general," and a description of the appearances of the urine, in cases where amorphous sediments of the phosphates are deposited as the urine cools and has stood for a little time, the author remarks that "in all cases the urine is extremely prone to decomposition, becomes alkaline by the evolution of ammonia, and emits a most disgusting smell." The greater proportion of the cases which have come under his own observation, have been "distinctly traced to *some injury of the back*, (as a fall from a horse or the like.)" It is an old observation, he remarks, that injuries of the back produce alkaline urine. Dr. Prout says he has not had an opportunity of inspecting a body after death under these circumstances. We have ourselves seen alkaline urine, from abundance of ammonia, in three cases where inflammation (shown by dissection,) of the

spinal marrow occurred from other causes than mechanical violence. In one case,—the subject of which was a player on the clarionet, engaged at Vauxhall, and who had sweat much, from playing, during a cold and windy night, previously to the attack of the disease,—the sweat, which was copious, had also a very strong ammoniacal odour. This patient had paraplegia. The sweat and urine often smell strongly of ammonia in tetanus, which appears to be always accompanied with inflammation of the spinal marrow.

Dr. Prout is of opinion that excess and deposition of the phosphates will also take place from local causes, which may be “some irritation about the bladder or urethra, especially when operating constantly for a considerable length of time;” but, in the case of the origin of this species of sediment from foreign bodies being present in the bladder, round which they are collected, he does not admit the explanation usually given of this circumstance to be generally true;—that is to say, that the urine in contact with the foreign substances always *undergoes an incipient process of decomposition*. The fact is, he says, “that the foreign substance sympathetically affects the kidney, and produces a change in the urine, causing it to abound in the phosphates, which are deposited on the foreign substance.” Dr. Prout does not notice the experiments of exposing foreign substances to urine without the bladder,—exposed to air and undergoing decomposition,—the results of which have been supposed to favour the prevalent opinion. We presume he has not thought such inapt experiments worthy of his attention. “With respect to the proximate cause of this form of disease,” he says, “we may suppose it to consist in a diminished or suspended action of the usual acidifying powers of the kidneys, and the formation, instead of lithic acid, of a greater quantity of alkaline matter than natural,—as urea (equivalent to ammonia), lime, and magnesia: but this being little more than a simple expression of obvious facts, of course throws no light upon the immediate cause of these depraved actions.” He alludes to the notion that the mucous membrane of the bladder is the source of the phosphatic sediments, and says he does not deny this altogether; and even thinks it possible that, in some instances, the earthy matter may be partly derived from this source.

The *crystallized* sediments composed of the phosphates, are constituted almost invariably of the triple phosphate of magnesia and ammonia, and exist in the form of perfectly-white shining crystals. This form may exist alone, or accompany the amorphous sediments of the phosphates: it is a disease of a milder character than the latter, and often precedes it. The urine is pale coloured, and, upon standing for some time, an

iridescent pellicle is frequently formed upon its surface, which is composed chiefly of the salt in question. Minute crystals of the same salt are frequently attached to the sides of the vessel in which the urine has stood for some time. When the urine much abounds with it, the crystalline deposit is formed before the urine is discharged from the bladder.

The causes of this affection "resemble, or may perhaps be identical in all respects, with those occasioning the preceding affection;" they may, however, be much slighter in degree. Mental anxiety will frequently produce, in many people, an excess of this salt in the urine. Several medicines that act as diuretics will cause it; "as the neutral salts in some cases, and particularly the Rochelle salts; and other saline compounds in which the acid is of vegetable origin." The author thinks it likely that a long-continued use of alkaline remedies will produce a tendency to an excess of the phosphates in general. This affection is more frequent in children than in adults. "The indications of cure to be attended to in these forms of disease, appear to be two: to diminish the unnatural irritability of the system; and to restore the state of the general health, and particularly of the urinary organs, by tonics and other appropriate remedies." In severe cases, especially of the amorphous species of sediment, "opium," says the author, "as far as my experience has hitherto extended, is the only remedy that can be employed with any advantage to fulfil the first indication." It must be given in doses of from one to five grains three times a-day. In conjunction with the opium, when the more distressing symptoms have been alleviated, the mineral acids, or citric acid, cinchona, uva ursi, and iron, may be had recourse to. When distressing pain is felt in the loins, a large pitch or galbanum plaster may be applied. When purgatives are requisite, they should be of the milder kind, as small doses of castor-oil. The author is doubtful whether mercury should be at all employed in the more severe cases; if it is used, it should be merely as "an alterative." All diuretics, as well as alkaline remedies, should be avoided. "The diet, in severe cases, should be of the mildest and most nutritious kind, and taken in very moderate quantities at a time." The author thinks an animal diet preferable, in general, to one of acescent vegetables, commonly recommended. Every thing that can be done for the patient is "of very little use, if the mind cannot be set at rest."

This chapter terminates with the following observations respecting those intermediate or *transition* states which usually exist for some time during the change from other diatheses to the phosphatic, or which sometimes precede the phosphatic diathesis.

Transition from the lithic to the phosphatic diathesis.—The first circumstances in the condition of the urine which generally denote a change from the lithic acid to the phosphatic diathesis, are the general paleness of its colour, and sometimes its increased quantity. There is also, for the most part, a great tendency in the urine, from the slightest causes, to deposit the amorphous sediments, which are always of a pale colour, and generally contain more or less of the phosphates intermixed with them. As the tendency to change proceeds, the urine may be frequently observed, after standing a few hours, to be covered with an iridescent pellicle on its surface, which, on examination, is found to consist principally of the triple phosphate of magnesia and ammonia; and, if at this time it be suffered to remain at rest for a short time, especially in warm weather, it becomes putrid, assumes a yellowish opaque appearance, and will be frequently found to contain large apicular crystals of the triple phosphate above mentioned. This constitutes what may be considered as the first stage of the series of changes in question. I have once or twice known a calculus extracted from the bladder during this stage, which I have had an opportunity of examining; and in every instance found it externally composed of pale-coloured lithate of ammonia nearly pure.

“The above state of the urine frequently occurs in sickly children, in whom the functions of the digestive organs are much deranged. It is liable also to occur from all the causes formerly enumerated, and particularly in those of an irritable habit, and who are subject to lithic deposits in general: also from any cause deranging the general health; or producing local irritation in the urinary organs. As to the constitutional affections, they are always more or less of the irritable kind, and generally accompanied by derangements of the digestive organs. In adults, also, there is not unfrequently some uneasiness felt in the region of the kidney. With respect to the tendency and danger of this sort of change, it may be generally removed, or at least prevented from getting worse, by a judicious use of the means formerly mentioned, provided its exciting causes can be removed. But, if these are permitted to operate, or are of such a nature that their operation cannot be prevented, medicines are of very little use; and the phosphatic diathesis will certainly sooner or later be induced, particularly if there be already calculus in the bladder.

In the second stage of the change in question, the urine commonly assumes a more decidedly pale whey-like colour, and is either alkaline when voided, or very soon becomes so. The lithate of ammonia also diminishes in quantity, or entirely disappears; while that of the phosphates, and particularly the triple phosphate of magnesia and ammonia, is increased. In short, this stage runs into the confirmed phosphatic diathesis by such imperceptible grades, that it is frequently difficult or unnecessary to draw the line of distinction; the symptoms and treatment being the same in most instances, only differing, perhaps, a little in degree. It may, however, be proper to observe, that where the lithate of ammonia is deposited in large quantity, mixed with the phosphates, hyosciamus rather than opium is to be preferred, as opium seems frequently to increase the formation of lithic acid.

"Transition from the oxalate of lime to the phosphatic diathesis.—

In the second chapter of this section I have given a summary description of a calculus composed of a nucleus of oxalate of lime surrounded by the phosphates, with an account of the series of intermediate changes which took place. From this description it appears that the first step towards the change in question, was a secretion of an excess of lime; and that, as this proceeded, the proportion of oxalic acid decreased, while that of the phosphoric acid increased, until at length phosphate of lime, in nearly a pure state, was secreted, which constituted the external crust of the calculus. I had no opportunity of examining the urine in this case in the earlier stages of the affection; but in the latter stages it had all the properties, as might be expected, of that secretion when the phosphatic diathesis is present.

"With respect to the treatment, &c. to be adopted in this form of the disease, I have nothing to add to what has been already advanced.

"No instance in which a calculus of cystic oxide has been surrounded by the phosphates, has come to my knowledge."

The sixth chapter treats "on the modes of formation and future increase of calculi; on the symptoms produced by the different varieties in different situations; and on the medical treatment to be adopted when they are lodged in different situations."

On the first of these subjects there is but little that is peculiar to the author, that had not been indicated in the previous parts of the work, excepting some conjectures respecting the causes by which the bases of calculi are collected in a crystalline form in the kidney. The points of most practical importance in the other subjects of this chapter, were also pointed out in the discussions on the several calculous diatheses, or are familiar to medical practitioners.

With respect to the operation of *solvents* of calculi in the urinary passages, the author says he knows nothing; and, from what little he has seen, he is very much disposed to doubt if they can be ever so administered as to produce the desired effect.

The work concludes with "general observations on the periods of life, sex, climate, &c. most subject to calculous affections; on the mortality attending the operation of lithotomy; with observations on the circumstances in which it ought or ought not to be recommended:" the data for which, excepting those relating to the circumstances under which lithotomy is advisable, are collected from the writings of Dr. MARCET and Mr. SMITH.

CRITICAL ANALYSES

OF

RECENT PUBLICATIONS, IN THE DIFFERENT BRANCHES OF
MEDICINE AND SURGERY,

In the Literature of Foreign Nations.

Παλριδὶς ἄρα

Ἀνδράσιν, ἢ παλαιαῖς ἀνδράσι, ἀγαλλόμεθα.

Du Siège et de la Nature des Maladies ; ou, nouvelles Considérations touchant la véritable Action du Système Absorbant dans les Phénomènes de l'Economie Animale. Par M. ALARD, D.M.P.; Chevalier de la Legion d'Honneur; Médecin en chef-adjoint de la Maison Royale de Saint-Denis; Médecin Consultant des Succursales de cette Maison; Médecin Honoraire des Dispensaires; et Membre de plusieurs Sociétés de Médecine nationales et étrangères. 2 tomes, 8vo.; de 367 et 577 pp. Baillière, Paris, 1821.

Morborum omnium unus et idem modus est: locus
vèro ipse eorum differentiam facit.

HIPPOCRATES, *de Flatibus*, § iv.

THIS work owes its origin to the views which broke upon the mind of the author, after he had, in the year 1800, been engaged in translating into French Dr. HENRY's "Observations on the Glandular Diseases of Barbadoes." He was astonished, he says, when he saw, in the description of the English author, the history of a disease whose seat was evidently in the absorbents, whilst its symptoms and progress were analogous with those of several affections which had been attributed to various organs and another order of vessels. At first, his notions of this disease were only those at that time generally prevalent; one of which was, that the peculiarities in its phenomena were the proper effects of the particular climate and soil of the island in which it was endemical; but, on regarding it more attentively, he inferred that an affection which prevailed especially in seasons productive of inflammatory diseases,—which gradually became endemic as that atmospheric constitution became established,—and which alternated sometimes with fevers, and at others with inflammation of the viscera,—required for its production only a concurrence of circumstances which every climate might present. This notion led to a series of researches, the results of which were published, in 1806, in a work of which the title was "A History of a Disease peculiar to the Lymphatic System," but which was afterwards changed to that of "A History of the Elephantiasis of the Arabs;" and at the same time that the author's inquiries enabled him to recognize the existence of the same malady in Asia, Africa, America, and even in Europe, he was again struck by the great number of relations and points of analogy which it presented with several other affections; and these relations and analogies induced him to consider the absorbent system as a structure

having an importance in the animal economy very different from that which had hitherto been imagined. From this time the horizon of his views extended before his eyes, and he soon perceived that no precise knowledge of the extent of the disorders dependant on this system was yet possessed; and that he was in the way for attaining some discoveries that would reflect a new light on the proximate causes of diseases in general. Since that time, he has done what his clinical experience, literary researches, and powers of reflection, would enable him to effect towards the attainment of the object here alluded to; and the work we are about to take into consideration presents the general results of his labours.

The author, in his Preface, endeavours to obviate some prejudices which he supposes to exist against such a work, and to conciliate the minds of his readers to its principal object. "A book of theory," he says, "a book on the proximate causes of diseases, is brought forth very incongruously in an age when the disposition given to the minds of medical students leads them only to empiricism. I venture to believe, however, that a little reflection will lead to notions more conformable with the true interests of science. The illustrious Professor of the School of Paris who, one of the first, turned the minds of his contemporaries from theoretical speculations, reprobated only vain and futile theories, composed, almost solely, of vague and insignificant terms which passed current in the ancient schools. It appears to me that he has been but ill understood, when it has been supposed that he intended that we should constantly observe diseases without ever occupying ourselves with considerations respecting their origin." It is, he adds, "undoubtedly necessary to observe, and to observe well, facts; but these facts would remain barren, if we did not trace their relations, and thence form some general inferences respecting them, without which there can be no science."

Much of the declamation against theory with which they are pestered in France, as well as we are in England, has, it appears, then, arisen from a vague employment of terms: some men of good judgment have used the word *theory* when reprobating, not a general expression of inferences immediately drawn from facts, but conclusions founded, more or less extensively, on inferences raised on supposititious bases;* and then certain persons who are very apt at echoing,—and who are qualified to do nothing else than echo,—the expressions of men of place or merit, apply it to all sorts of reasoning; whilst their own conduct, to complete the absurdity of the matter, is such as is described so exactly and laconically in the cited passage introduced at the commencement of the review of the work of Mr. Bégin.

The work of Dr. Alard is constituted of two distinct parts: the one treating of the distribution of the absorbents, and the part those vessels perform in the vital functions in the state of health; the other, of their action in the phenomena of diseases. The anatomical and physiological disquisition occupies the whole of the first of the two volumes

* The word *hypo*-thesis expresses very well (in conformity with a general admission in modern languages,) this sort of reasoning; and it is a pity that all writers will not employ it where it is necessary.

into which the work is divided ; and it has been rendered thus extensive, from the author, as it appears, having thought it prudent to display the bases of his inferences, and discuss the obvious objections to his conclusions, in such a manner as to relieve any of his readers from the necessity of seeking elsewhere for the most important of the facts which appear to support, or to oppose, the system of pathology which he has here developed. In the analysis we are about to give of this work, we shall suppose the reader to be informed of many things of which the author has considered it proper to produce detailed observations, but which are familiar to all physiologists ; as well as of several inductions from facts, equally well understood, that he has not advanced without a narration of the evidence on which they are established.

Dr. Alard commences his work with the following proposition :—
“ That which constitutes the basis of the body of man and of the most part of animals, is a tissue of vessels of diverse nature. The hardest parts themselves are but a vascular assemblage : their hardness depends only on the matter contained in the vessels, as the fleshy appearance has no other cause than the presence of blood, or of a sanguineous fluid, in the interior of those canals.”

In support of this proposition, the author first notices the observations of HALLER, KING, RUYSCH, and several other and later anatomists, which tend to show that the smallest perceptible portion of the structures of the animal body, even though, using the expression of Haller, it be no bigger than a grain of sand, is found, on examination by the microscope, to present a multitude of small vessels, different orders of which are filled with their proper fluids. The failure of injections to demonstrate this mode of structure in the bones, the brain, and some other parts, in first trials, was considered to present an objection to the generality of the inference above stated : but, as early as 1739, a French academician, on examining, with the aid of a good microscope, bones tinged with madder, found them to present, first a tissue of white fibres, then one of a reddish colour, under this a third, and then a fourth, of deeper-coloured fibres ; so that the bone was, throughout the coloured part, wholly penetrated as it were by a natural injection. Some preparations of the bones, made by SCARPA, and preserved in the museum at the University of Pavia, seem also to show that the basis of the osseous structure is nothing but an aggregate of vessels. The brain was found by LEUWENHOEK to be constituted, as far as the powers of the microscope could inform his sense of vision, wholly of a congeries of vessels filled with various fluids. These observations of Leuwenhoek were soon afterwards supported by others of a similar kind, made by VIEUSSENS. We noticed the principal arguments for the application of the same general inference to the structure of the eye and several other organs, as well as the membranes formed in inflammations, when we gave an account of the Dissertation of Dr. FELICI on this subject, in a late Number of this Journal. The author's researches on this point have been very extensive : he has, indeed, made himself acquainted with every thing of importance that has been advanced in support of the proposition with which the

chapter commences. It does not, however, come within our views to notice the observations in detail; as they are amongst those which may be supposed to be generally understood. Some facts may, however, be adduced respecting the tubular structure of the muscular fibres,—which was first asserted by KING, (in a paper published in the *Philosophical Transactions*,)—that will, perhaps, be novel to some of our readers.

After BLANCARDI had, as he asserted, injected the muscular fibres by means of the arteries distributed to them, MUYS, on throwing warm water into the crural artery of a lamb, perceived the muscular fibres lose their colour and become entirely white: on passing, then, a coloured fluid into the artery, he found the muscles assume the colour of the liquid, and, on examining them attentively with the microscope, he was satisfied that the smallest fibres of the muscles were filled with the liquid; whilst no vestige of it appeared in their interstices. This is conformable with what is stated by HALLER, in his commentary on the *Prelections* of BOERHAAVE; when he says, that “water injected into the coronary artery returned by the vein, at first coloured with blood; but it was gradually seen to become paler and paler, until it was quite colourless. The heart, preserving its form, had become as white as the stomach or the bladder.” We shall conclude our observations on this subject, with remarking that, notwithstanding the evidence presented by the facts above detailed or alluded to, many eminent anatomists of the present day believe in the existence of a simple laminated texture, forming the base of the cellular and other membranes; and that, whilst such a texture is intimately interwoven with vessels, which, especially when distended by injections, may appear to occupy it wholly, there yet remains a membranous expansion, not of a tubular nature. With respect to the tubular structure of the muscular fibres, we have, also, to remark, that Mr. CARLISLE has stated (in his *Croonian Lecture* for 1805,) that he can distinctly see an ultimate muscular fibre, which appears “as a solid cylinder, the covering of which is reticular membrane, and the contained part a pulpy substance irregularly granulated.” We know not, however, what to believe on this subject,—the statements of men of eminent talents are so incongruous: for *Leuwenhoek* and *Hook* compare the ultimate muscular fibre to a string of pearls; *Prochaska* says it is as fine as a gossamer film, and extends throughout the whole length of a muscle; whilst *Haller*, admitting its filamentous character, says it extends only to a short distance along a muscle; *Muys* asserts that it has a knotted appearance; *Blancardi* describes it as being contracted here and there; whilst *Lecat* says that it looks somewhat like a knotted reed: the greater number of anatomists, and amongst them *Cuvier*, agree, however, in stating, that the utmost powers of the microscope present it to us in the form of a solid cylinder, of a filamentous character, similar to the fibres which are cognizable by the unassisted visual powers.

With respect to the vascular nature of the ultimate muscular fibres, we may add the remark, that some physiologists have regarded the fact that the muscles, in cases of death from asphyxia, have been found

of a brownish-red colour, whilst the blood in the arteries was of a deep-purple hue, as a proof of the combination of colouring matter with the muscular fibre itself; as, did the colour of the muscles depend on blood circulating through them, they must assume the purple hue of the blood. But this inference is not irrefragable, even were it not opposed by the fact that muscles may be washed white,* by injecting water into their arteries. The ultimate fibres of a muscle may derive their colour from blood circulating through them, and yet preserve their ordinary hue when the larger vessels are filled with purple blood, in virtue of that law which causes certain vessels to admit only certain kinds of fluids to enter them under natural circumstances. This law was well appreciated by Bichat, in his reasonings on the functions of his capillary system, when he endeavoured to explain why those vessels refused to admit the red blood unless their vitality had undergone some modification; and it is somewhat extraordinary that he did not apply it to the condition of the muscular fibres in the case of asphyxia.

The second chapter commences with the proposition that "*the vessels which compose the basis of the bodies of men and animals communicate throughout with each other.*" MALPIGHI, the author remarks, appears to have first perceived the communication which exists between the arteries and the veins, by the aid of the microscope. His observations were afterwards supported by those of Leuwenhoek, who observed, besides, that it is impossible to determine where the arteries end and the veins begin. This intimate connexion throughout the body, appeared to him to be so well proved that he could conceive no other way of accounting for the secretions than the admission of a transudation of the fluids through the parietes of the small arteries. The observations of Leuwenhoek were made on fishes or amphibious and cold-blooded animals; and, therefore, some doubts were entertained respecting the mechanism of this part of the economy in man, or in hot-blooded animals. The fact of such a mode of communication of the arteries and veins in animals of the latter kind, was, however, soon established by the observations of COWPER; and subsequently by those of CHESLDEN, HALLER, SPALLANZANI, and numerous later inquirers. The passage of injections from the arteries into the veins, in almost every part of the body, was shown by RUYSCH, DE GRAAF, VIEUSSENS, HALLER, MASCAGNI, and has become a familiar experiment in every dissecting-room.

The communication of the larger blood-vessels with the lymphatics, has not been proved by an equal extent of evidence. The fact of such a communication has, however, been well established by CRUIKSHANKS and several other anatomists.

But, besides the vascular lymphatics capable of attaining a size which renders them perceptible to our eyes and permits us to trace them with our instruments, there is another series of vessels, extremely small in diameter, which anatomists have been accustomed to call by

* Other physiologists than those above named have stated that this may be effected; but GORTER seems to stand alone, when he asserts that a muscle thus washed white—pale as a membrane, he says,—will preserve the faculty of contracting on being excited.

different names, in conformity with their supposed uses; and which were regarded as arterious by Ruysch, because he injected them by the arteries; as lymphatics, by Cruikshanks and Mascagni, because they injected them by the larger lymphatic vessels; and as venous by Dr. Ribes, because he has injected them by the veins. These circumstances, and the results of experiments on the absorption of fluids placed in contact with various surfaces, seem to warrant the inference that the sanguineous vessels are connected, by their parietes, with vessels which are distributed about the body in the utmost abundance, and developed on all the surfaces, exterior and interior; and which, though they generally carry only colourless fluids, transmit red blood under certain conditions.

The influence of the vital powers in preserving the vessels last alluded to from the passage of red blood, is well shown by some experiments made by Professor BUNIVA.* He had obtained some curious results on injecting the arteries of dead animals with blood diluted with water; and, wishing to try the effect of a similar injection in a living animal, he tied the axillary artery of a calf, opened the vessel below the ligature, and urged the injection into it with considerable force. The liquid entered with difficulty, and presented no sign of its presence on the exterior of the limb. The spinal marrow was then divided near to the head: the heart and arteries almost instantly ceased to pulsate; the animal ceased to live; and, as suddenly, the diluted blood which remained in the syringe passed with rapidity into the limb, and showed itself on the surface, as it had done in his former experiments with injection of the same fluid. Some of the experiments here alluded to were made on the human body, where the injection, too, was thrown into the aorta; and, as these presented the greatest variety of interesting phenomena, we shall transcribe an account of their results on subjects of this kind.

The skin generally assumed a blood-red colour, whilst the palms of the hands had a fine rose tint; a blistered surface became tumid, reddened, and covered with drops of blood; in certain parts,—the cheeks, for instance,—a bloody sweat broke out; the liver and the spleen swelled, and became so reddened as to present the appearance of violent inflammation; the stomach was so red as to have the semblance of a muscle; the intestines showed a similar aspect, whilst bloody fluid oozed from their parietes; the conjunctiva was as red as it is in ophthalmia; the iris, the retina, the sclerotica, the choroides, and the capsules of the aqueous and vitreous humours, partook of the same colour; as did also the bones and periosteum. The phenomena just described, and especially the diversities in the results of the experiments when living and dead animals were the subjects of them, prove, in the most decisive manner, that during life the red part of the blood is retained in its proper vessels by the laws of vitality, rather than by a want of capacity in the lymphatics to admit of its reception into these vessels; whilst they demonstrate the continuity of the vascular system in all its parts.

* Published in a memoir read to the Institute in 1799.

The author next discusses the question of the existence of any organ or order of vessels continuous with the extremities of the arteries and the origin of the veins, and in which the circulation is independent of the heart. Although he considers that this question had already been satisfactorily replied to, in the negative, by the facts already cited from Malpighi, Leuwenhoek, Cowper, Vieussens, and others, of the continuity of the arteries and veins, he thinks it proper to notice the results of several experiments which serve to establish the propriety of this decision. He remarks, that Leuwenhoek saw signs of the impulse of the heart communicated even to the largest veins; that Hales observed the motion of the blood accelerated, by each systole of the heart, in both the arteries and the veins of the lungs of frogs; that Spallanzani has proved, by experiments, that the rapidity of the movement of the venous fluid is increased during the systole of the heart, and lessened in the diastole, in several species of lizards, and that, not only in a few small veins, but in an innumerable quantity, whether the circulation was effected with the greatest rapidity or in a languid manner; and he concludes with relating the following experiment of Magendie. This physiologist laid bare the crural artery and vein of a dog, and put a ligature round the whole of the limb excepting these two vessels; he then tied the crural vein, and opened it below the ligature: the blood sprang from it in a continuous jet, of considerable volume. He then compressed the artery: the jet from the vein continued from a few instants, but it diminished as the artery became devoid of blood, and ceased entirely when it was completely empty: although the vein was "gorged" throughout its whole length, blood no longer escaped from the opening in it. The compression of the artery was removed at this instant; the blood entered the vessel, and the jet of blood from the vein was immediately renewed. Similar results occurred in the several repetitions of this experiment that were instituted.

Dr. Magendie, wishing to show the supposed fact in question in another manner, introduced the extremity of a syringe filled with warm water into the crural artery, and gently propelled the piston: the blood in the vein instantly escaped, at first alone, and then mingled with water, in a jet which was more or less considerable according as more or less force was exerted on the piston of the syringe.

To this evidence of the existence of a continuous volume of blood from the arteries to the veins, Dr. Alard joins some physiological considerations, tending to show, as he infers, the danger that must result to the economy were the motion of the blood influenced, to any considerable degree, by the capillary vessels independently of the heart. "The movements of this 'circulatory circle,' (he says,) must be very regular, in order that health, and even life, may be sustained; for it is absolutely necessary that the blood which, in the time of one pulsation, has passed from the arteries to the veins, should re-pass in the same quantity, and in an equal period, from the veins to the arteries. If the transmission were not to take place in this due proportion, it is obvious that engorgement of the veins would ensue; and, on the contrary, the same condition of the arteries would happen

if, in the time of a second pulsation, more blood passed from the veins to the arteries than had passed from the arteries to the veins. Were this relation of the course of the blood to exist for any considerable length of time,—though the blood may accumulate in the veins to a certain point without much alteration of the health,—the circulation would certainly be disturbed: and how could this danger be absent for an instant in a system of which the parts were unconnected, or of which the two portions, although united by a common centre, should yet be regulated by different laws, and which should propel their fluids by two impulsions which could not agree? Such, indeed, would be the sanguineous circulation, if there really existed a system of vessels intermediate to the arteries and veins, which was free from the influence of the action of the heart, and in which the blood would enter in a multitude of diverse ratios, according to the casual modifications of sensibility." This conclusion of Dr. Alard, it cannot be concealed, is founded on an assumption that is merely gratuitous, and which may not appear to be so plausible to others as it is to himself. Many physiologists will think that it does not appear so necessary a result that the circulation must be commonly disturbed, were it influenced by an order of vessels intermediate to the larger arteries and the veins, and independent of the action of the heart. The preservation of a due relation between the two motive powers, would not be incongruous with several other relations in the economy, as that of the iris with the retina, that of the respiratory actions with those of the heart, by which the former are executed quicker or slower, in conformity with similar changes of the motions of the heart.

Dr. Alard adduces several other considerations in support of his opinion, though he notices but few important facts that had not been brought forward, with similar views, by Dr. CHARLES PARRY. Dr. Alard admits, however, with the generality of physiologists who regard the heart as the predominant agent of the circulation throughout its whole extent, that the arteries and the veins possess "a moderate degree of sensibility; such as is qualified, at the utmost, to second the impulsion given by the heart." The question here noticed is one that is not likely to be soon, or even ever, settled. The cases in which satisfactory evidence of a distinct propulsive motion of the capillary vessels is manifested in warm-blooded animals, are instances of deviation from the ordinary state of health; and, considering the diversities of judgment of different men, even those of the best disciplined minds, on all inferences from merely analogical reasoning, it can hardly be expected that diversities of opinion will not be entertained respecting the propriety of regarding the manifestation of effects only in states of disorder as evidence of their existence in that of health. Perhaps the case the least remote from health in which motion of a small artery, distinct from that of the heart, has been observed, is that related in the Dissertation which gained the Jacksonian prize in the year 1813. The author of this Dissertation says, that the dorsalis pollicis in his right hand is large and conspicuous, so that the action of it may be seen as well as felt. On producing an inflammation of the skin covering this artery, by friction, the number of its pulsa-

tions, in a given time, has been greater than those of the radial artery of the same side: thus, he says, "the pulsations of the arteries generally were at seventy; the *dorsalis pollicis* has been irritated, it has beat eighty-four times in a minute; the radial artery was immediately examined, and was found to beat seventy-two strokes in a minute; the examination was again transferred to the *dorsalis pollicis*, the pulsations of which still continued at the rate of eighty-four in a minute, the vessel being visibly enlarged by the friction."

We advert to the fourth chapter of the work before us, which is thus entitled: "The humours are elaborated, without the sanguineous *circulatory circle*, by the absorbent vessels. Properties of these vessels." After remarking, in conformity with the inferences drawn in the preceding chapter, that the circulation of the blood should be regarded as "a physical rather than a medical phenomenon," the author observes, that "this circulation of the blood in the arteries and veins would be entirely useless for the maintenance of life, if there did not pass from those vessels "*rivulets*" which irrigate and nourish the invisible tissues of the parts adjacent. These rivulets are not formed by simple jets of extravasated fluids: pellucid canals, charged with the irrigation of these parts, every where spring from the arterial parietes. The existence of these canals is doubted at the present day, after having, for a long time, been admitted as a demonstrated truth. An opposite of this sort in the opinions of men depends on the extreme tenuity which these tubes always preserve, and especially to a degree of pellucidness which renders them invisible when they are empty or contain only fluids diaphanous as themselves, and which, also, permits them to assume the red colour whenever they are penetrated by blood; thus causing them to be often confounded with proper sanguiferous vessels. This double source of illusion renders the study of them one of the most difficult points of anatomy, and must throw in the way of those who devote themselves to it inevitable subjects for contradiction. We may, however, assemble, a series of facts dispersed in a great number of works,—facts too much neglected by the moderns, and which, when united, form an imposing collection of proofs in favour of the existence of these supposed tubes, parting from the parietes of the blood-vessels; which it would be difficult to avoid admitting, even though we could not materially demonstrate them, if we would explain, in a plausible manner, either the phenomena manifested by the nutritive functions, or the symptoms of the greater part of diseases." The propriety of these inferences will not be denied, we think, by any physiologist; the only point for disputation being, whether these pellucid vessels,—lymphatics, or exhalants, as they are most commonly termed,—spring from the sides of the arteries, or are expansions of the extremities of some of them which are not continuous with veins: for, whilst the fact of this communication of the arteries and veins has been admitted, as applied to the vessels, to a certain extent, it has been conjectured that there are other arteries which finally terminate in an expansion of exhalant vessels, admitting only colourless fluids, whilst the red parts of the blood are transported by the nearest proper arterial ramifications.

This last question is the subject of discussion in the fifth chapter of the work before us, which commences with the proposition that "the capillary system, such as it was conceived to be by Bichat, has no real existence." We shall pass over the evidence in the last chapter, tending to establish the inference of the existence of the pellucid vessels above alluded to: our limits will not permit us to give a comprehensive account of it; and it is, besides, detailed in works familiar to those versed in the study of anatomy. We could not do justice to the reasonings of the author on this evidence in an abstract, but we may remark that he has supported his inference in a very forcible manner. His arguments against the views of Bichat on the point just alluded to, serve to show that the functions attributed by this physiologist to his imaginary particular system of vessels, and the apparent laws by which those functions are regulated, are conformable with those of the absorbent system generally, and incongruous with those of the proper sanguiferous system.

This discussion is continued through the subsequent chapter; in which he endeavours, more especially, to prove that "the exhalant and cellular systems are only dependences of the absorbent system."

The subject is discussed, in a more general manner, in the seventh and eighth chapters; the objects of which are to show, that "the active part of the parenchymatous substance of the organs is composed only of absorbent vessels;" and that "the absorbents are the sole agents of nutrition and growth, as well as of decrease and decriscence." We shall endeavour to give a view of the author's opinions on these subjects in a concise general abstract, without following him through his discussions; for, by proceeding, as he has done, from proposition to proposition, discussing each singly, he has been obliged to enter into many repetitions, which it is not requisite that we should detail. One series of repetitions in which he indulges, and which we may just notice here, is that in which he endeavours to show that the nerves, the muscles, the cellular texture, and, in a word, all the soft parts of the body, are constituted of a congeries of minute vessels, variously arranged, containing fluids of diverse kinds, in virtue of the different vital properties, or modes of sensibility, which those vessels possess in the several organs or peculiar structures. These vessels, Dr. Alard believes, possess every where the power of contraction and dilatation, and receive diversities of fluids as they are differently excited by external agents; as we see, very well, in those of the conjunctiva of the eye, which, when stimulated, admit, or, using the author's expression, *pump* the red blood from the arteries with which they communicate. The only difference between the author's view and that of Bichat on this point, seems to be that the former considers the functions of these vessels to be similar to those of the larger series which have ordinarily been termed absorbents,—a notion which has led him to regard the whole as one system, to be designated by one term; whilst Bichat considered them to be expressly analogous to the arteries, and indeed as the final expansions of these vessels. Bichat, however, it must be remarked, whilst he contemplated the abundant distribution of capillary vessels in every structure, did not suppose

but that there existed also a sort of simple membranous expansion or laminated texture, which formed the bases of the parenchymatous structures in which those vessels are so copiously distributed, as well as of various other modes of simple fibrous arrangement.

The view of Dr. Alard,—that of minute pellucid vessels, springing from the parietes of the small arteries; distributed in every point of the body; conveying different fluids and performing different organic functions, according as their vital properties are modified; having corresponding vessels, which may be said to spring from the most intimate texture of the organs, uniting into larger tubes, forming in some instances long continuous canals, (generally denominated absorbents,) in others running to be inserted in the parietes of veins;—is one that is calculated to explain, more plausibly than any other, the mechanism of the distribution of the fluids for the purposes of the organic functions, and several of the most remarkable phenomena manifested in disordered states of those functions; whilst it does not want plausible arguments in its support, (as Dr. Alard has well proved,) and is, besides, calculated to obviate the difficulties which have been presented by the results of the experiments of Hunter, Magendie, Brodie, and other eminent physiologists, on the mechanism of absorption. Still, it must be acknowledged, the author's inferences are founded, principally, on merely plausible suppositions; but these suppositions have lately received a very important degree of support in the discovery of Dr. FOHMANN, of Heidelberg, of a communication of the lymphatics of the intestines with the mesenteric veins. It requires not a great stretch of analogy to infer—considering the evidence in favour of the existence of the pellucid vessels in question, presented by the observations and experiments of Ruysch, Cruickshanks, Ribes, (see page 415,) and several other anatomists,—that a communication which exists between a series of those vessels so large as to be palpable to the senses, exists also in such as are too minute to be thus recognized.

It may be here asked: if the basis of all the organs is but a congeries of vessels, allied to absorbents by their nature, how is it that certain organs execute functions so different from those of other parts; and, as an example, how is it that absorbents, arranged as a muscle, possess the power of contracting with such force as is witnessed in this species of organ? To this the author would reply, that the same order of vessels possesses peculiar properties superadded to the general characteristics of those vessels, in consequence of diversities in their alliances, arrangements, and the fluids they contain; and, in respect to the contractility of the muscles, he observes, that this faculty is the proper quality of absorbents; so that, instead of saying that absorbents possess muscular properties, it would be better to say that muscles possess, in an eminent degree, the inherent property of those vessels,—that of contracting whenever they are submitted to exciting agents; and if, using his own expressions, “muscles are provided with the contractile power in so high a degree, it is not to the peculiar nature of their fibres that they owe this prerogative, but to the manner in which those fibres are disposed in the intimate structure of those organs; to the union of

those innumerable bundles of parallel fibres, the contraction and agency of which all the surrounding parts concur in favouring."

By admitting this hypothesis, we readily dispose of the difficulties which beset the questions of the structure of the urethra, uterus, iris, and choroid processes; and it is, in the structure of some polypi, &c. of course, applied to such a purpose by the author. The uterus, in his views, becomes muscular during pregnancy, because its vessels assume then that arrangement which constitutes a muscle; and it ceases to be muscular after parturition, because it then loses this particular arrangement. It is unfortunate, however, for this view of the subject, that no such arrangement as that on which the author's inference is founded is recognizable by our senses, in any stage of utero-gestation. The absorbent vessel, then, is, according to Dr. Alard, the seat of irritability, whilst it is the most simple, and only essential, part of animal organization. The seat of irritability is a point that has so perplexed the most eminent physiologists, that one could almost wish that he could persuade himself,—as, in the want of sensitive evidence in this case, we must incline our belief, more or less, to some hypothesis or other,—to admit the notions of Dr. Alard. Finding that organization devoid of nerves, (as well as our senses can inform us,)—as in the case with some polypi and the vast class of microscopic animals,—possess even more delicate excitability than the organs of man, we must agree, with Haller, that irritability is not the exclusive property of nerves; but then we are, by the same reasoning, inclined to reject his opinion that it is the exclusive property of muscular fibres, when we find that animals are, as ALBINUS remarked, more irritable in proportion as they more nearly resemble a gelatinous mass devoid of any distinction of parts; and thus we are induced to believe, with the physiologist just named, that this jelly is the proper seat of irritability: and here Dr. Alard will come forward, and say that this moving jelly could not possess the power of contraction and locomotion if it were not organized, if it were not enclosed by a diaphanous cellular tissue which has selected it from various alimentary matters, which causes it to flow and reflow in the interior of this tissue, and at last rejects portions of it in order to replace them by others. This cellular tissue, then,—composed of filaments more or less parallel to each other, and differing only by its tenuity from that of larger animals,—is evidently the means of sensible contractions, since it is the only solid part existing in the most simple mode of animal organization: and we may even see, as we rise in the scale of animal beings, some of these filaments merely placed in juxta-position, without being united with the least cellular texture, from the first type of the muscular organs, as is manifest in certain insects. Even the human embryo is similar in its apparent structure, in the early stages of its existence, to the more simple forms of animal organization; being constituted only of a gelatinous mass, interspersed with absorbent vessels, the orifices of which open externally, instead of into an internal cavity as in the medusa.

The view which Dr. Alard has taken of this subject, is conformable with the simplicity which some men take delight in imagining to exist

in the means employed by nature. In inorganic bodies, attraction is sufficient in order that each portion of matter may find its proper place in the vast universe. In animals' bodies, sensibility produces the innumerable phenomena of vitality. The means on which absorption depends become, in muscles, in consequence of a particular disposition of parts, the powerful motive agent by which an animal transports itself from one place to another. The same vessels which are every where else moveable, are in the bones gorged with calcareous phosphate, in virtue of the same principle, and thus become points of support and levers to the motive organs previously designated.

As the absorbent system is the part first formed in the fetus, so does it predominate in the body during the earlier periods of life, or until the growth is completed: it then gradually loses its vitality; the fluids formerly absorbed by the exhalant order of vessels, are either retained in the arteries, or they are returned into the torrent of the circulation by the veins; the absorbents decay in their structure as well as in their functions; the glands cease to appear reddish and pulpy; the fluid, with which they had ordinarily been filled, disappears, and the organs themselves are often obliterated with age, and a plethora of the blood-vessels is the result. The bones also present signs of this species of change in the economy. The exhalant absorbents had carried to these structures the gelatinous and calcareous matter on which their cohesion and formation depended; the resorbing order of vessels gradually remove those vessels in old age; as this period of life extends, the bones become less hard and less weighty than in adult age; the compact substance of them diminishes in thickness; the cellules of the spongy tissue become more ample, the medullary cavity enlarges; the calibre of their blood-vessels increases; and, in a word, their calcareous matter returns into the torrent of the circulation, and is deposited in various parts; thus producing ossifications which often hasten the arrival of the final period of human existence.

The successive changes in the human body above slightly alluded to, are discussed in a very particular manner by Dr. Alard, who finds in them many phenomena, which he employs, with much force, in support of his views of the functions of the absorbent system in the animal economy. We can only recommend the more studious part of our readers to peruse his work: our limits oblige us to pass over the important and often ingenious considerations it presents, in a very rapid manner, and in many instances wholly unnoticed. Dr. Alard's general conclusions certainly amount to an hypothesis rather than to a theory; but it is an hypothesis that has much of plausibility, and the disquisitions adduced in its support are rendered extremely interesting by the multitude of curious facts,—many of which have been drawn from unmerited obscurity,—which the very extensive erudition of the author has enabled him to bring forward; as well as by the cool, candid, and intelligent manner in which they are conducted.

The proposition argued for in the ninth chapter, is that "*the absorbent vessels of the nervous system draw from the blood the matter of the living solid.*" This chapter involves speculations which, to us, appear to have less of plausibility than any others in the work, and

such, indeed, as we do not think congruous with the author's general hypothesis. He thinks that the nerves are the organs first formed in the fetus, and that the nerves "form in their turn all the other organs; by means of the expansion of the numerous filaments which compose them, and which are converted,—we might almost venture to say, which re-appear,—in cellular filaments, muscular fibres; in a word, in the nutritive parenchyma of the organs." Why the author deprives the absorbents of powers, in the higher classes of animals, or those possessed of nerves, that he had conceded to them in a general manner, we cannot well discern.* The formation of nerves implies nutrition; which is a sufficient argument against these organs being the essential agents of this function. The priority in respect to the period of their formation to that of the other organs, admitting this antecedency to be proved, is but a feeble argument for the inference that these organs are produced by the nerves. This priority of formation is, however, denied by some eminent physiologists, amongst whom is CARUS, who has shown so much accuracy of research in his work on the Nervous System. A pulsating vessel is said, by the physiologist just alluded to, to be the first distinct organ.

The arguments in favour of the proposition which is the subject of this chapter, only, as we think, tend to render it highly probable that the nerves have much influence in the functions of nutrition in the higher classes of animals: whilst it must, at the same time, be considered that nutrition is effected without nerves, not only at the first development of the human embryo, but in fully formed animals; unless we admit, with OKEN, that a polypus is a mass in which the nervous matter exists in a state of fusion uniformly extended throughout the animal; but this proposition involves the existence of a faculty without the presence of the organ of that faculty, or it implies the existence of properties in inorganic matter, that are afterwards said to be the peculiar attributes of a certain mode of organization. This sneaking, shuffling way of calling in the Epicurean philosophy to our aid, can never lead to any good: let us profess it openly and candidly; if we do resort to it as the best means of escape from difficulties which perplex us.

* "We have seen," says the author, in another place, "that, of the three orders of vessels which compose the bases of the solids of man, the only one to which we may give the denomination of the acting animal, according to the happy expression of Hunter, is that of the absorbents. We have seen that this order of vessels is solely charged with the labour of nutrition; that it presides over growth in infancy, and is alone productive of decrease in old age; that, by means of a property quite exclusive, it draws from the air and from alimentary matters the principles capable of sustaining life; that it is the same vessels which combine them, assimilate them, and distribute them throughout the organs, and repel, by the excretories, the substances henceforth inappropriate for the exercise of the functions and the maintenance of existence."

In another part of the work, the author speaks of the absorbent system as being "allied, confounded, and identified, with the nervous system." By this last statement, it is true, he may be considered to obviate the charge of incongruity, above advanced; but then it is by, as it appears to us, entering into extremely vague, or at best very hypothetical, and not very plausible, assumptions.

"The absorbent system is not a whole, the parts of which are connected together, like the sanguineous system; it is composed of several isolated systems;" is the proposition the author endeavours to establish in the ninth chapter of the work before us. This is so obvious an inference from the views already exposed, that we need not enter into any discussion in order to show in what way it has been arrived at. Having established it, on the grounds already indicated, the author treats, in succession, of the division of the absorbent system charged with transmitting to the blood the lymph and the chyle; the division of the absorbent system charged with the resorption of the fluids which should no longer constitute part of our organs; of the division charged with mingling with the blood the oxygen drawn from atmospheric air; of the division charged with the secretions; of the division exerted in the acts of reproduction and generation. At the same time that the author describes the phenomena dependant on the functions of those several divisions of the absorbents, he finds, in those phenomena, arguments in support of the general hypothesis which it is the principal object of the work to establish. He takes occasion, when treating of the division of the absorbent system charged with the resorption of the fluids which should no longer constitute part of our organs, to enter into a discussion of the opinions which have been advanced respecting the order of vessels by which absorption is effected, in the body generally.

His own notions on this subject must be obvious. As the veins are every where continuous, in their ramified extremities, with arteries, according to his views, it is impossible that these vessels can be the agents of absorption, unless their parietes are capable of absorbing. The agents of absorption, according to Dr. Alard, are either the vessels ordinarily termed absorbents, or those pellucid vessels which, as he has inferred, spring from the parietes of the blood-vessels, venous as well as arterious, and present their open mouths in every point of the substance of the organs, and on the whole of the membranous surfaces of the body. As we have already remarked, it is only necessary to admit of the existence of such a mode of structure, in order that the difficulties which have beset this subject may be obviated, and the apparently opposing results of different experiments shown to be perfectly congruous and referable to the same mechanism. The facts furnished in the results of injections, noticed in a former part of this article, present, perhaps, the best arguments for the support of his opinions; but physiology and pathology are not wanting in the supply of others from which warranted inferences may be drawn that tend to establish the same propositions. In the first place, Dr. Alard remarks, if we compare all the orifices by which the arterial blood is expended, with the two lymphatic trunks by which it is supposed that all the materials supplied to the blood must pass, we shall see that, if these trunks were the only channels by which the blood could receive supplies, there would soon be no venous blood. This consideration would point out that a considerable proportion of the fluids absorbed pass almost directly into the veins, without entering the thoracic ducts. The greater volume of the venous than the arte-

rious blood furnishes, also, an argument in favour of the same inference. If, viewing this point in another direction, the veins below the subclavians received only the portion of arterious blood which has not been expended in nutrition, the secretions, and excretions, we should, it seems, find them contain a less volume of fluids than the arteries.

Purulent matter has been found in the abdominal veins, without any signs of inflammation of those vessels, when there has been collections of pus in the belly. This has been frequently observed by Professor CHAUSSIER, especially after peritonitis; but this fact presents, it may be said by the partizans for venous absorption, equivocal indications. The anatomical facts in favour of the inference that pellucid vessels, having orifices opening on the surface of the peritoneum, and communicating with the adjacent veins, and those which seem to show that the venous ramifications are every-where continuous with the arteries, must turn the balance in favour of Dr. Alard's explanation.

Dr. RIBES, who believed in venous absorption when he related* the fact we are about to notice, says that he had conjectured that the lymphatics in the medullary cavity of the bones, which absorb the marrow, terminated in veins; and experience has partly verified his conjecture, by showing that, by injecting the hepatic veins, he could fill the superficial lymphatics of the liver. We have before noticed this point of anatomy; and shall therefore only add now, that this experiment, repeated several times with the same success, satisfied him that all the vessels of the order of lymphatics do not terminate in the thoracic duct. After the discovery of such a fact, it seems strange that the phenomena which at that time were considered to prove venous absorption, should any longer be thus interpreted. Dr. MAGENDIE thinks he has obviated these equivocal indications by some recent experiments, and proved that venous trunks absorb by their parietes. We shall elsewhere notice his memoir on this subject.

The only circumstances that have been brought forward as proofs of the ramifications of veins presenting open mouths on the surfaces of the membranes and the cavities of the cellular tissue, are that fluids injected by the veins have been effused in those parts. But Dr. Ribes, who relates these facts, acknowledges that such results were to be obtained only when the experiment was made on a body in which putrefaction had commenced; and it seems, hence, so highly probable that rupture of the minute venous ramifications took place, so as to render this argument one of very little force in favour of venous absorption, or the communication of the cavities of those vessels with extraneous surfaces in the natural state of the parts.

The views of Dr. Alard seem to be alone qualified to explain the promptitude with which a considerable quantity of certain drinks sometimes passes from the intestinal canal to the urinary passages; how it happens that asparagus, prussiate of potash, and many other substances, when introduced into the stomach, soon manifest signs of their presence in the urine, whilst the venous blood, at least that

* In the eighth volume of the *Memoires de la Société Médicale d'Emulation*.

drawn from the arm, does not appear to contain them, or to have suffered any alteration;—how a liquid charged with odorific principles, introduced into the abdominal cavity, immediately manifests itself in the veins; whilst an analogous liquid passed into the intestines, promptly appears in the lacteals, and remains a stranger to the veins;—how the milk possesses the odour and other predominant qualities of food and other substances received into the stomach, (by which a child is purged when its nurse has taken a cathartic;) whilst the blood and other secretions have no such odour or qualities;—and, lastly, how certain substances, as milk, water pure or mingled with turpentine, inserted in a wound of the integuments, penetrate the trunks of the valvular absorbents, and not into the veins; whilst certain other substances, as the *upas lieuté*, for example, inserted in the same manner, pass rapidly into the veins, and do not appear in the valvular absorbents.

All these phenomena are readily explicable, if we admit the existence of absorbent lymphatics every where arising from the textures, and terminating in veins; so that little centres of absorption are formed, each of which, being modified according to the arrangement of the parts which give origin to it and the diversity of the exciting agents affecting it, directs the absorbed matters to the veins, the lacteals, or certain excretory organs; whilst diversities in the sensibility, at different times and in different parts, enables them to exert that specific action by which they select and transmit certain matters, and refuse to receive others of a different kind.

Details on the rest of the points treated on in the divisions above enumerated, do not come within our intentions: we pass on, therefore, to the next chapter, in which the author treats of the structure and physiology of the mucous membranes, the cellular tissue, and the skin. The author's peculiar notions of the structure of these organs must be obvious, from what has been stated in former parts of this article. Excepting his assimilation of the functions of their capillary vessels to those of the absorbents, commonly so termed, his physiological considerations are characterized by a judicious exposition of what had already been advanced, rather than by any remarkable novelty. He dwells much upon the similarity of the structure of the skin and mucous membranes, and the analogy, in many respects, between their functions. We find some of the most remarkable proofs of the latter assertion in the lower classes of animals: the zoophyte which has the form of the finger of a glove, (the *hydra fusca*,) for example,—the interior of which is its digestive organ,—may be turned inside out, and yet the animal will live, having the functions of its two surfaces reversed.

This, and the two ensuing chapters, in which the author treats of the development of animal heat,—on the influence of the absorbent vessels on the establishment of the constitutions proper to each period of life,—and on the modification of temperaments in individuals,—seem to have been introduced in order that the account of the physiology of the absorbent system given in this work might be rendered complete, rather than with the intention of proposing any thing novel

of remarkable importance. His disquisition on the temperaments is, as he acknowledges, principally derived from the writings of LORRY, CABANIS, and HALLE; with which we suppose our readers are well acquainted.

The subject just mentioned terminates the first volume of the work; the second will become the subject of an article in the ensuing Number of this Journal.

Principes généraux de Physiologie-Pathologique, coordonnés d'après la Doctrine de M. Broussais. Par L. J. BÉGIN. 8vo. pp. 390. Mequignon-Marvis, à Paris, 1821.

[In continuation from page 345.]

THE fifth chapter of the work of Mr. Bégin treats of *the local effects of irritation*. On this subject Mr. Bégin advances nothing of considerable importance that has not been stated in this Journal, in the articles professedly devoted to the doctrine of Broussais.

Mr. Bégin labours hard to support the doctrine of Broussais respecting the origin of morbid growths and alterations of structure;† and he seems to be well aware that he has undertaken a perplexing task. With respect to the origin of all morbid tumors from irritation, (in the sense in which Mr. Bégin uses this term,) there is the strong objection, that such tumors are not unfrequently found in the lungs and other organs of new-born children: and then we are perplexed to the utmost, when we attempt to imagine how an augmentation of vital action simply, differing only in being more or less in degree, can produce the numerous varieties of morbid structure witnessed in the human body.

We pass over this vague and unsatisfactory disquisition to the next chapter, which treats of *the general effects of irritation*. In this chapter, also, there is but little to notice that has not been already considered in this Journal: the value of the author's discussions consists chiefly in details which cannot be comprised in the limits of this Journal. Mr. Bégin, however, we should not neglect to remark, has combatted very forcibly for the truth of the most essential points in the hypothesis of Broussais relative to fever, though he differs from him in some points; and especially in regard to the order in which the phenomena of this condition of the economy are developed. Broussais seems disposed to infer that fever, when it does not arise from primary irritation of the stomach and small intestines, takes place from irritation affecting those organs sympathetically from disease of some remote part. He thinks that diseases in remote parts,—as, for example, a severe wound in one of the legs; or a phlegmon in the arm,—does not excite fever, unless they sympathetically produce such irritation; and he is disposed to believe that the heart and the brain are

* See the Numbers of this Journal for January and February 1820, for details on this subject.

affected, in the development of fever, subsequently to the stomach. Mr. Bégin thinks that sometimes one of those organs is affected first in order, and sometimes another: he considers, however, that a knowledge of the order of those affections is not a matter of so much importance as Broussais and some of his partizans have imagined; and that what is of greatest consequence to the physician, and that which he should always bear in mind, is that, "whenever there is fever, there exists irritation, more or less considerable in degree, of the mucous membrane of the stomach, the heart, and the brain.

Mr. Bégin dwells much, with good reason, on the influence of civilization, and the great excitement of the nervous functions which accompanies its progress, in the origin and development of diseases, "We often find, for example," he says, "according to the accounts of travellers, that the most extensive and painful wounds hardly attract the attention of the savages of North America or New Holland; and, if abundant suppurations, or other consecutive accidents which depend on the nature of the affected parts, do not cause them to perish, they become cured spontaneously and with facility, without having been sick. Their internal diseases present the same simplicity, and an analogous absence of any considerable sympathetic accidents." Amongst other illustrations of this point, derived from sources more near to us, he relates the following anecdote.

"On leaving Moscow, in the disastrous campaign of 1812, the tenor of my sensations led me to make an excursion over the fields which were one of the latest and most brilliant theatres of our military glory,—the fields on which had been fought the battle of the Moskowa. The villages which had surrounded them were all destroyed; the calmness of death reigned over those plains where, only a few days previously, six hundred thousand men disputed a victory, the consequences of which were to become so fatal to the welfare of France. Our army, already overwhelmed with ills of every kind, passed in silence, and almost without recognizing them, the very places which had been rendered immortal by its valour. At the extremity of the open country bordering the road, and near to the confines of a forest, I was suddenly roused from my reverie by cries of lamentation that seemed to come from a spot very near to me. I looked around in vain; I could see nothing but putrefying bodies. The complaints, however, continued, and I could not then doubt that a living man was hidden amongst the adjacent ruins. Having descended from my horse, I, after much pains and long researches, discovered, at the bottom of the ditch bordering on a bastion, and in the interior of a horse, a Russian soldier, whose leg had been carried away by the charge of a cannon. The unfortunate man had escaped the attention of those who, soon after the battle, had gone over the field for the succour of the wounded; and he had remained there for six weeks, finding in the body of the animal his food and habitation. He had deprived the ribs of their flesh, and removed the internal parts, and thus converted the thorax into a sort of cage, still enveloped with the skin, in which he lay in a half-recumbent posture. An abundance of pus flowed from his wound, and added to the stench of the carcass on which he fed.

Several other horses, lying near to that in which he was situate, bore marks of his voracity; large pieces of them having been detached by means of an old knife which he had about him. This man seemed to be hardly sensible of the danger he had encountered. He was pale and meagre, but his strength appeared to be but little diminished; and his movements were characterized by firmness and assurance. The surface of the stump, which was exposed, was unequal, but covered with granulations of a favourable appearance."

On comparing this case, Mr. Bégin says, and others of a similar kind, with those presented by the practice of great cities, we are tempted to believe that they have occurred in subjects of quite a different species.

Mr. Bégin discusses in this chapter, besides the phenomena of the several forms of fever, termed by nosologists idiopathic, inflammatory, synochal, typhous, eruptive, &c.—all of which, he argues, are connected, as effects, with irritation of the mucous membranes of either the alimentary canal or the respiratory organs,—dyspepsia, (which he attributes, generally, to chronic inflammation of the mucous membrane of the stomach, (a point of doctrine that Dr. Parry has done much for the establishment of in England,) dysentery, (from irritation of the mucous membrane of the large intestines,) *tâbes mesenterica*, *scrofula*, *phthisis*; in conformity with views of Broussais that have been already exposed in this Journal, in which they are all traced to irritations of some one or other of the mucous surfaces.

Diseases of the heart are next considered, without, however, any thing remarkably novel being adduced. After this, Mr. Bégin treats on the more chronic forms of inflammation. Although less violent than those produced by acute inflammations, the disorders caused by chronic irritations are not less remarkable, and do not present a less degree of interest. Persons affected with chronic gastro-enteritis, are exposed, Mr. Bégin says, to the greater part of the forms of mental alienation; mania itself is very often referrible to this cause; melancholy and hypochondriasis almost always depend on it. Mr. Bégin thinks that chronic inflammation produces mental affection as it is seated in different organs. Individuals having chronic gastro-enteritis readily despair of their life; many of them feel an horror of existence, and put an end to it by suicide; "whilst others, a prey to constantly-renewed fears, dread the action of all external bodies, and regard with terror the approach of death, which they consider as inevitable." Chronic lesions of the lungs produce opposite effects on the intellectual functions: persons in *phthisis* preserve, to the last instant of their existence, the full powers of their intellects, and expire in the midst of preparations for future enjoyment. Organic affections of the heart exert a peculiar influence on the mental faculties: the subjects of them seem to fear all acute moral impressions, which cause disturbance of the circulation, and aggravate the evils which accompany them. Men who once evinced the most undaunted courage, become, on suffering disease of this organ, timid in the extreme, and cannot support even the idea of the smallest danger.

A point of very great importance in the history of irritations in ge-

neral, and especially of chronic irritations, is the disposition they have, in certain cases, to occur in a periodic manner after regular intermissions.

"In what do intermittent differ from continued irritations?" is a question which Mr. Bégín discusses, as one of very great importance in regard to the etiology of intermittent fevers. If we consider, he says, the organ which is the seat of the former, during the paroxysms of their accession; or if we examine the general phenomena they produce, it is not possible to discover any difference which may serve to distinguish them from the latter. Ophthalmia, cephalalgia, erysipelas, rheumatism, &c. do not differ in any respect, when they are of an intermittent kind, from what they are when they are continued; and there exists between the periodicity of their occurrence and their absolute continuity, a multitude of shades which connect the two conditions. The excitement is sometimes too intense to be dissipated otherwise than very imperfectly: we then observe simple alternations of exacerbation and remission. At other times the diminution of the morbid action is more considerable, and the return of the paroxysms is marked by a rigor of greater or less severity; the disease is then termed remittent. In some other varieties, the "vital concentration" has hardly diminished, when it is reproduced, and develops another paroxysm; and, lastly, the intermission may be complete, and vary in duration from the period of a few minutes to that of two, three, or even seven or more days; and hence the multitude of distinct species of fevers of this kind arranged in the tables of nosologists. All these conditions are closely allied, and we may frequently, by stimulating the affected organs, transform a quartan fever, for example, into a tertian or quotidian, or even approximate the paroxysms so that it will be regarded as a continued fever; and, in all these cases, the morbid phenomena undergo no other sensible change than that of not disappearing at certain epochs.

The periodical returns of intermittent irritations in the same textures, when prolonged for a considerable period, produce disorganizations similar to those which are the results of continued chronic phlegmasiæ. Indeed, although perfectly intermittent during its early stages, the inflammation, on being frequently renewed, gradually becomes continuous: each accession adding to the stimulation, the organ returns in a less complete manner to its natural state. It remains red and irritated during the apyrexia; but this excitement is but little remarkable at first, because the slight degree of phlogosis it determines is hardly manifest after the state of intensity which accompanies the paroxysms. It is only after a long time, and when the local disorder is very extensive, that the symptoms no longer, in appearance, completely disappear, and that we witness the signs of the disorganization of the tissues. It is easy to trace precisely all these gradations, when the disease affects the exterior parts of the body; and we may acquire, by means of the general symptoms, almost as satisfactory knowledge of their succession when they have their seat in the viscera. In cases of intermittent fever, we find, after their prolonged duration, the mucous membrane of the stomach thickened, dark-coloured, and more

or less disorganized. The liver and the spleen,—which almost always participate with irritations of the stomach and duodenum, and which are, in each paroxysm, the principal seats of sanguineous concentration,—become affected with chronic engorgements or secondary chronic inflammations. The mesenteric glands tumefy, and suffer change of their structure, in the same manner as if the intestinal irritation were continuous.

Besides the local effects above considered, intermittent fevers occasion very remarkable phenomena in the economy more generally. The first period of each paroxysm is accompanied by the concentration of the vital actions on the digestive organs; the blood, directed to the interior parts, engorges the liver, the spleen, the lungs, and reddens the irritated mucous surfaces; the heart is surcharged, and exerts itself to propel the fluid which accumulates in the large vessels, from the small vessels of the exterior parts being devoid of blood: hence results derangement of the respiration and circulation, and, as a consequence, disorder of all the other functions. The lungs especially gradually contract chronic irritation of their mucous membranes, which not unfrequently leads to phthisis. The heart suffers, in some cases, more or less extensive dilatation of its parietes, especially of those of the right auricle and ventricle. The circulation and respiration being thus periodically disturbed for several hours, the process of sanguification becomes less perfectly effected; accumulations of serum take place in the cavities of different parts of the body; digestion is effected with difficulty; the reparatory fluids become less abundant than is necessary, or possess ill qualities; and hence emaciation, debility, and sometimes all the phenomena of scurvy.

The alterations which the fluids undergo, from derangements of the organic functions, constitute the subjects of some discussions by Mr. Bégin, and he expresses his intention of considering this part of pathology more fully at a future period, in another work. At present he produces but little more than an obvious development of the notions of Broussais already stated in this Journal, with some additions, however, relating to the existence of more or less acrid secretions in cases of irritations of the organs, and to the generation of specific poisons; which comprise, of course, nothing more than a history of known effects; the causes of those changes being inscrutable by the senses.

[We have yet to notice the author's therapeutical disquisitions; which will form the subject of an article in the next Number.]

Medical and Physical Intelligence.

ALTHOUGH it is not a proper subject for a *critical analysis*, we are disposed to make a few remarks, in this place, on a new translation of the Pharmacopœia of the Royal College of Physicians of London, that has been produced by Mr. COLLIER.* "Having for some years been employed in instructing medical Tyros in the Latin language," (Mr. Collier says, in his Preface to the work just alluded to,) "and supplying those deficiencies in chemistry and botany which form the *sine qua non* to their passing their examination before the Court of Examiners at Apothecaries' Hall, it was natural for me to inquire by what means I could most easily accomplish my task, to my own credit and that of my pupils. Since the passing of the Act of Parliament 53d of Geo. III. entitled 'An Act for the better regulating the Practice of Apothecaries throughout England and Wales,' something like a classical education seems indispensably necessary for pupils, if they would go through their examination with credit to themselves. Yet has it been a standing complaint with that 'honourable and intelligent body,' that many appear before them for examination who are wholly ignorant of the Latin language and the common rudiments of chemistry. It is in consequence of these deficiencies that I have had so many young men under my care; and it has been my labour to give them such a competent knowledge of the language as should enable them to translate physicians' prescriptions and the Pharmacopœia of the Royal College of Physicians. Now, to effect this in a short time, I knew no better method than to put into their hands a literal translation of that work; and, not being able to find any, I naturally enough set about the business myself, and have endeavoured to give it them in that state which I hope will answer their purpose and mine." Mr. Collier has also annexed to each preparation, in which chemical operations are concerned, a theory of those operations, which, he thinks, "will give them (alluding here to "private practitioners" and druggists,) that succinct idea of the chemical decompositions, as shall enable them to avoid many errors."

On the value of such a mode and extent of instruction, as well as the fact that Mr. Collier has "hitherto instructed near a hundred pupils in the same plan, and never yet had one rejected from the Hall," it is not our business to make any critical remarks;—the history of no small proportion of the Theses produced at some of the most celebrated Universities, as we all know, need not make students thus *ground* very much dissatisfied with themselves:—but it is proper that we should say something respecting the manner in which Mr. Collier has fulfilled his intentions. The translation is, with a few exceptions,† as strictly verbal as it is well possible to render it, at the

* Published by Highley and Son, 1821.

† As, in the "Preface of the College," (for example,) in the concluding sentence of the second paragraph, where "*always*" is introduced;—in the next paragraph, "*so well*" is superfluous,—and "*their* sanction and authority," where

same time that it is not remote from some degree of neatness of expression: a specimen of success requiring a greater command of language than those who have not attempted such a task may probably imagine. Mr. Collier's theories of the chemical processes are correct, in conformity with the established principles of chemistry. In the few obscure examples of chemical operation which the *Pharmacopœia* contains, (as those in the preparation of tartarized antimony and antimonial powder,) he agrees with the best chemists, excepting on one point, that of the composition of the black sulphuret of mercury; where he does not appear to be informed of the observations of GUIBOUT, who has shown it to be constituted, not of a uniform sulphuret of mercury, but of the red sulphuret, or cinnabar, mingled with merely minutely-divided metallic mercury. This part of the work would have been more useful to students than it is at present, had the author comprised, in his own descriptions, the names of the several compounds used in the new chemical nomenclature: for example, when objecting to the term *sub-muriate* of mercury (applied to calomel), and stating that it is a muriate at the minimum of oxydizement, he might have added, or a *proto-chloride*; that the oxydurate is a *deuto-chloride*, and so of the rest. This would give them more precise ideas, and at the same time enable them to understand such terms when they may meet with them in the medical writings of other nations. As it is, the notes form an addition to the translation that should obtain for it the preference to the others by persons who are not themselves well versed in chemistry,—without paying any regard to the fulfilment of the author's express intentions of rendering the work more peculiarly useful to any particular class of students.

As the extent to which the space of this Number of the Journal is occupied by articles of Review which we have been unable to render more concise, still prevents our fulfilling our promise to give an abstract of the whole of the papers in the first Number of Dr. MAGENDIE's Journal for Experimental Physiology, we shall at present produce only an account of the memoir on the *Mechanism of Absorption in Red and Warm-blooded Animals*, by Dr. MAGENDIE; and complete our abstract at future periods, as the arrangements of the Journal may permit.

The memoir commences with a demonstration of the fact, that ab-

the pronoun is without an antecedent; at the same time that the version is not quite correct, any more than that, in the next paragraph, of "*ea tandem et stabilia maximè et utilia fore*";—where "*medicos*" is translated "*practitioners*";—where "*Salutius igitur esse putavimus*" is translated "*we therefore thought it wiser*";—where, alluding to the division of the gallon into parts, "*pro arbitrio*" is omitted;—and where "*suavissima*" is translated "*most sweet*:" with a few others, in different parts of the work, of no further import than that they are deviations from a "literal" translation; amongst which there is, however, one (which frequently occurs,) that we should notice, because it would indicate a grammatical fault in the composition of the original which does not exist: it is where Mr. Collier translates "*sepone, ut fiant crystalli*," "*set it aside, that it may crystallize*," and then says, "*dry these (crystals), &c.*" Here the pronoun and the noun are without a relative antecedent,—from his using the verb *crystallize* instead of the noun *crystals*,—which is, as it is obvious, not the case in the original.

sorption is effected in animals. Having proved the fact, the next point is to determine the agents of this function. Dr. Magendie says, that the results of experiments related in his former memoirs on this subject shew that—

“ 1°. The sanguiferous veins are endowed with the absorbent faculty.

“ 2°. It is not demonstrated that the vessels which absorb the chyle can absorb other matters.

“ 3°. The absorbent power of such of the lymphatic vessels as are not chyliferous, is not yet established on sufficiently satisfactory evidence.”

Then follow remarks that these inferences are qualified to explain how it is that substances are absorbed in parts devoid of lymphatic vessels, (as Dr. Magendie thinks proper to say,) as “ the brain, the eye, &c. ;” and an attempt to ridicule the notions that the supposed absorbent vessels exert a sort of “ discernment” in the selection of the fluids they transmit, (which ridicule is just as well applied here as it would be to the notion that the larynx has a sort of “ discernment,” since it admits air and refuses to admit brandy ; or that the stomach possesses such a faculty, since it retains and digests a potato, and rejects a root of ipecacuanha.)

Dr. Magendie then states that his experiments have shown that, when a certain quantity of warm water has been injected into the veins of an animal, absorption is impeded ; and when the quantity of blood is diminished, by abstraction, absorption is accelerated ; and that, when a certain quantity of blood is abstracted, and replaced by the same quantity of warm water, absorption is effected as rapidly as it is under ordinary circumstances. These facts have led the author to infer that absorption is a purely physical action in the animal economy, and depends on “ the capillary attraction of the vascular parietes for the matters absorbed.” After having adduced some considerations which he thinks favour this explanation, he proceeds to relate the results of experiments which he believes prove it to be true. This explanation, he remarks, would indicate that absorption should be effected by vessels after death, as well as during life ; and this he considers as also proved by his experiments. Dr. Magendie says—

“ I took a portion of the external jugular vein of a dog : (this portion of vessel, in length nearly an inch, received no collateral branch :) I deprived it of surrounding cellular tissue ; I attached to each of its ends a glass tube, by means of which I established a current of warm water in its interior. I then plunged the vein in a slightly acid liquor, and I carefully collected the fluid which formed the interior current.

“ It is evident, from the disposition of the apparatus, that there could be no communication between the interior current of warm water and the exterior acid liquor.

“ For the first few minutes the liquor which I collected had suffered no change ; but, after five or six minutes, the water became sensibly acid. Absorption had then taken place,

“ I repeated this experiment on veins taken from dead human bodies : the effect was the same.”

Similar results were obtained from similar experiments with a portion of an artery, when it was besides remarked that, "the more acid the external fluid, and the more high the temperature, (below a certain distance from the boiling point,) the more rapidly the phenomenon was produced."—"If capillary absorption occurs in large dead blood-vessels, why should it not take place in similar living vessels?" Aware of the obvious reply of a sceptic, Dr. Magendie put this question to the test of experiment. He says—

"I took a dog about six weeks old, at which age the vascular parietes are thin, and consequently particularly calculated for the success of the experiment. I laid bare one of the jugular veins, isolated it completely throughout its whole length; carefully separated it from the parts adjacent, especially the cellular tissue and some small vessels which ramified from it; and placed a card beneath it, in order that the vessel might have no contact with the surrounding part. I then let fall on its surface, at the middle of the card, a thick aqueous solution of an alcoholic extract of *nux vomica*, a substance the action of which on dogs is very energetic: I took care that no portion of the poison should touch any other part than the vein and the card, and that the course of the blood was free in the interior of the vessel. Before the lapse of the fourth minute, the effects which I expected were manifested, at first but feebly, but afterwards with such severity that insuflation of the lungs was necessary for the preservation of the life of the animal."

Similar results were obtained, on repeating the experiment on an adult dog. It remained to try it on an artery. This was effected on the carotid arteries of two large rabbits. The effects were developed in both animals, and one of them died. "To assure myself," Dr. Magendie says, "that the poison had really traversed the parietes of the artery, and that it had not been absorbed by the small veins which might have evaded my dissection, I carefully detached the vessel which had been the subject of the experiment; I divided it throughout its length; and I persuaded the persons who assisted me to taste the little blood which had remained adherent to its interior surface: they all recognized, as well as myself, the extreme bitterness of the extract of *nux vomica*."

In order to ascertain whether the minute vessels also permitted fluids to traverse their parietes, Dr. Magendie made the following experiment:—"I took," he says, "the heart of a dog who had died on the preceding evening; I injected into one of its coronary arteries, water of 30° centig. (85° Fahren.) This water readily returned by the coronary vein into the right auricle, whence it flowed into a vessel. I poured into the pericardium half an ounce of water slightly acidulated. At first the injected water showed no sign of acidity; but, on the lapse of five or six minutes, it presented unequivocal evidence of the possession of that quality." Thus, the fact in question appeared to be proved in respect to dead minute vessels: in regard to living vessels of this class, Dr. Magendie says, "it was not necessary to have recourse to new inquiries, nor to sacrifice more animals. The experiments which I mentioned in my memoir on *the Organs of Absorption in the Mammalia*, left no doubt on this point, according to the judgment of the Academy."

In respect to the permeability of membranes by certain fluids during life, (evidence of which after death we have in the colouring of the parts adjacent to the gall-bladder with bile,) Dr. Magendie observes, that he has often seen membranes penetrated and coloured by substances placed in contact with them: for example, if a certain quantity of ink is introduced in the cavity of the pleura of a young dog, the period of hardly an hour is requisite in order that the pleura, the pericardium, the intercostal muscles, and the surface of the heart itself, may be rendered black.

Amongst his inferences from the foregoing facts, Dr. Magendie remarks, that "the capillary attraction of the parietes of the small vessels appears to be the cause, or, more exactly, one of the causes, of the absorption called venous;" whilst he takes care to add, that "this conclusion does not 'touch' in any respect the absorption which is effected in the small intestine on the chyle, by the chyloferous vessels, and still less the absorbent property of the lymphatics; though the experiments I have just described seem to indicate, that if, in the greater number of instances, these vessels do not absorb, this depends not on their parietes, which have properties similar to those of the veins, but to the want of a continuous current in their interior."

BIOGRAPHICAL NOTICE OF THE LATE DR. GREGORY.*

DR. JAMES GREGORY was descended from an ancient family in Aberdeenshire, which for above two centuries has enjoyed a distinguished name in the annals of science, and been particularly eminent for mathematical genius. It is a remarkable fact, that a great number of the descendants of this family have filled with great repute professional chairs in different Universities.

Dr. Gregory's father was professor of medicine, first in Aberdeen and afterwards in Edinburgh. His grandfather was professor of medicine in Aberdeen. His great grandfather, the celebrated cotemporary of Newton, and inventor of the Gregorian Telescope, was professor of mathematics, first at St. Andrew's and afterwards at Edinburgh. The other branches of the family equally contributed to support, in this respect, the honour of the name. The elder, (being that of the Gregories of Kinardy,) even boast of a much larger number. Among them may be noticed Dr. David Gregory, professor of mathematics at Edinburgh and Oxford, the father of Dr. David Gregory, formerly Dean of Christ-Church. Mr. Innes, professor of Philosophy, in Aberdeen; Mr. Charles Gregory, professor of Mathematics, at St. Andrew's. His son, Mr. David Gregory, who succeeded him in the same chair: and lastly, the celebrated Dr. Reid, professor of Logic, at Glasgow.

Dr. John Gregory, well known to the medical profession by his admirable "*Lectures on the duties and qualifications of a Physician*," and to the world in general as the author of "*A Father's Legacy to his Daughters*," left three sons and two daughters, by Elizabeth, daughter of William Lord Forbes, a lady who, to great beauty and engaging manners, joined a very superior understanding, and a more than common share of wit. The eldest son, James, (the subject of the present memoir,) was born at Aberdeen in January, 1753, and received the rudiments of his education at the grammar-school of his native place. During the winter 1764-5, he studied at King's College, Aberdeen; from that University he was removed next season to Edinburgh, and the following year was entered at Christ

* We have received this notice from an authentic source. The previous arrangements of the Journal had left us so little space when we received it, that we have been obliged to omit several parts of the original narration, but we believe we have preserved whatever is of most importance to the character of the subject of it as a physician, professor, and man of science. The expressions of regret for his loss and just eulogy of his talents, with which the biography was preceded, will too readily occur to every reader to require that they should be expressed by the historian. *Editor.*

Church, Oxford. He resumed his studies at Edinburgh towards the close of 1767, and continued there until the winter 1773, when he was sent to London to prosecute his medical education. Here he became a pupil of St. George's Hospital. In June, 1774, he graduated at Edinburgh. His thesis is entitled, "*De Morbis Celi Mutatione Medendis.*" It treats in detail of phthisis, pulmonalis, hypochondriasis, and gout; and concludes by noticing the advantage of change of air in the prolonging of human life. It is marked by the same elegance of composition for which he soon after became so conspicuous. As a medical work, it is chiefly interesting, as containing a very clear exposition of the doctrine of *irregular distributions of blood.*

During the whole of the year 1775 he was on the Continent, and in the course of his tour visited Holland, France, and Italy. Many of his letters written at that time are still extant, and prove that he was feelingly alive to all the beauties of nature, and a fervent admirer of the treasures of ancient art.

In June, 1776, he was appointed (at the early age of twenty-three,) to the professorship of the Theory of Physic; and he continued to teach this class with great distinction for twelve years. As a text book for his lectures he published in 1779-80, the first part of his "*Conspectus Medicinæ Theoreticæ.*" The work was completed in two large octavo volumes in 1782. It rapidly acquired a high reputation all over Europe, not only in consequence of the scientific merits which it possessed, but the singular felicity of classical language in which it was written. Several editions of it have since been published, but it is greatly to be regretted that it never received from the author those additions and alterations which were required to adapt it to the present state of physiological, and more particularly chemical science. Hence its value is now considerably diminished to the student of medicine, but it is still perused with the highest advantage for the general principles of the science, and as a model of classical latinity it will probably remain unrivalled.

In the year 1790 Dr. Gregory was appointed, in consequence of the death of Dr. Cullen, to the chair of the practice of medicine; and for thirty years he sustained, and even increased the celebrity which the eminence of his predecessor had conferred upon the office. His lectures, besides their intrinsic merit as the vehicles of sound medical learning, were characterized by a richness of illustration which we imagine to be quite unprecedented. From very early infancy Dr. Gregory had been remarkable for a most retentive memory, and this he had diligently cultivated. The advantages which it gave him as a teacher of medicine are incalculable. He has been frequently heard to describe cases which occurred in the earliest years of his practice with a freshness, and *particularity* of detail, which made them appear like the observation of the day. No case ever seems to have escaped his memory which could enforce some principle of pathology or practice; and he could draw therefore, almost *ad libitum*, upon these stores, for the most striking illustrations of the doctrine he was inculcating.

It is much to be regretted that he never carried into execution the design which he had long contemplated of publishing a *Conspectus Medicinæ Practicæ*. The mass of medical information which he possessed was immense, and we much fear it has perished with him. No notes of his lectures can, we imagine, ever be considered as giving a correct view of his opinions or practice. We are led to this conclusion by considering, how remarkably *precise* he was in the choice of his expressions, how frequently he varied his lectures from the mere extent of the subjects before him, and with what candour he modified his views according to the experience by which he was continually profiting.

As a literary man, Dr. Gregory long enjoyed a high reputation. He was early devoted to the study of metaphysics, and in combating the doctrine of necessity he displayed uncommon ingenuity. His "*Literary and Philosophical Essays,*" in two vols. were published in 1792. As a philologist, he is known by a very profound essay on "*the Theory of the Moods of Verbs,*" published in the second vol. of the Transactions of the Royal Society of Edinburgh. Though not a professed mathematician, his mind was deeply imbued with the spirit of mathematical science, inherited from his celebrated ancestor; and this can be traced in all his writings, conspicuous alike for closeness of reasoning, clearness of arrangement, and the utmost precision of language.

His controversial writings are numerous, and display an exuberance of talent, a lively wit, and an extent of general information, which is astonishing, when we consider with what assiduity he cultivated, from a very early period of life, his professional pursuits.

In 1781 he married Miss Ross, daughter of James Ross, esq. of Stranraer. By this lady, who did not long survive her marriage, he had not any children. In 1796 he married Miss M'Leod, daughter of Donald M'Leod, esq. of Glenties, in Ross-shire. By her he has had eleven children, of whom seven are living; five sons and two daughters. The eldest son is practising at the Scotch bar; and the second, who bears his father's name, is now qualifying himself to follow that profession, of which his father was so bright an ornament.

To great merits as a teacher of medicine, and high reputation as a man of science and literature, Dr. Gregory added a distinguished character as a *practical* physician. From the time he was first appointed to a chair in the University, he took an active share in the clinical practice of the infirmary; and the lectures by which he illustrated it are spoken of by those who heard them, in terms of unbounded admiration. About the period of his second marriage he began to be actively engaged in private practice. This increased so much upon him, that in less than ten years afterwards he was obliged to give up his clinical lectures, to the great regret of the students.

In gardening, Dr. Gregory early found a great source of amusement, and he continued to indulge it to within a few days of his decease. At one period of his life he was a zealous archer, at another he entered with spirit upon the duties of a soldier, and never ceased to look back with pleasure upon the time he carried a musket as a private grenadier. Possessing a fine relish for ancient poetry, he occasionally attempted to infuse its spirit into our own language. He was more happy in his Inscriptions, many of which are worthy of the proudest days of Roman literature.

In spring, 1818, he experienced a severe misfortune, in the breaking of his arm, by the accidental upsetting of the carriage in which he was travelling. The injury which he received was very severe, and his health soon after began sensibly to decline. In April, 1820, he visited London, after an absence of twenty-eight years, as one of a deputation from the University of Edinburgh, to congratulate his Majesty on his accession to the throne. On that occasion he was honoured by a private audience of his Majesty; of whose gracious condescension he cherished the most grateful remembrance. During the last year, he suffered severely from attacks of difficulty of breathing, which ultimately ended in hydrothorax, which put a period to his valuable life, soon after having completed his 68th year. His health did not permit him to give any Lectures, during the last winter, after Christmas. The course was completed by his nephew, Dr. W. P. Alison, in a manner which, we hear, has gained the approbation of a very numerous class.

His remains were interred in the family-vault in the Cannongate church-yard, with great solemnity, on Saturday, April 7th; the funeral being attended by the magistrates, professors, and other public bodies, the students of his class, and many private friends. In the Cannongate, where Dr. Gregory at one time resided, a very strong testimony of respect was paid to his memory, by shutting up the shops during the time the procession passed. An universal feeling of regret has indeed been expressed for the loss of one whose life was at once so honourable and so useful. Uniting, in a degree seldom equalled, the studies and acquirements of a man of science with the taste and honourable feelings of a gentleman, he shed a lustre upon the medical profession, whose influence will, we trust, be as lasting as it has been extensive and important.

The loss of such a man will be felt as occasioning a blank, almost irreparable, not only in the academical celebrity of Edinburgh, but even in the national distinction of the country.

MONTHLY CATALOGUE OF MEDICAL BOOKS.

A Monthly Journal of Popular Medicine: explaining the Nature, Causes, and Prevention of Disease, &c. Conducted by Charles T. Haden, Surgeon to the Chelsea and Brompton Dispensary, &c. Nos. I. and II. 1s. 6d.

A Treatise on Acupuncture. By James Morris Churchill. 4s. boards.

A Practical Treatise on Diseases of the Heart, &c. By H. Reeder, M.D. 10s.

A Manual of the Diseases of the Human Eye. By George C. Montezath, M.D. Illustrated with Engravings. 2 vols. 8vo.

A View of the Structure, Functions, and Disorders of the Stomach and Alimentary Organs of the Human Body. By Thomas Hare, F.R.S.

A Supplement of the Pharmacopœias. By S. F. Gray. New Edition.

Practical Observations on Disorders of the Liver, &c. By Dr. Ayre. 2d Edit.

[HIGHLEY AND SON, FLEET-STREET.]

METEOROLOGICAL JOURNAL.

By Messrs. WILLIAM HARRIS and Co. 50, Holborn, London.

From March 20 to April 19, inclusive.

Day of Month.	Mo. or Noon.	Rain gauge.	TEMP.	BAROM.	Defec. or Hygr.	WIND.		ATMOSPHERIC VARIATION.		
			Therm.	Barom.						
Mar										
20			39 50 38	29 30 29 30	53 55	W	WNW	Fine		Cloud.
21			40 50 34	29 26 29 50	55 55	W	NW	Fine		
22		09	38 47 32	29 66 30 00	55 56	NNW	N	Fine	Sho'ry	Fine
23			34 45 33	30 06 30 00	54 54	N	SSW	Fine		
24		20	37 49 35	30 00 29 85	53 57	SW	SW	Fine	Sho'ry	Rain
25		13	39 49 36	29 73 29 60	60 59	SW	W	Rain		Fine
26			38 51 36	29 51 29 45	56 55	SW	S	Fine		Cloud.
27			41 50 38	29 40 29 41	55 58	SW	SW	Fine		
28		16	40 55 40	29 27 29 15	60 60	SSE	SE	Cloudy	Rain	
29		29	41 52 38	29 23 29 37	60 59	S	SW	Rain		
30			41 49 40	29 70 29 72	58 58	WSW	SSW	Fine		
31		27	42 48 36	29 43 29 57	58 57	S	W	Rain	Sho	Fine
Apr										
1		11	39 50 47	29 66 29 53	58 58	WNW	SSE	Fine		Rain
2		21	49 53 45	29 33 29 33	63 62	W	W	Rain		
3			47 55 47	29 33 29 35	60 55	SW	SW	Fine		
4			45 57 49	29 38 29 41	57 57	SW	WNW	Fine		
5			45 53 43	29 78 29 90	55 56	N	NNW	Fine		
6		19	47 51 42	29 95 29 99	57 59	N	N	Fine		Rain
7			45 55 49	30 06 30 17	63 62	N	W	Cloudy		
8			52 60 51	30 16 30 13	60 60	NW	W	Fine		
9			55 61 52	30 05 29 93	58 58	WNW	WNW	Fine		Cloud.
10			56 60 48	29 82 29 74	57 60	W	SW	Fine	Cloud.	
11		24	53 59 42	29 67 29 54	53 57	SW	SSW	Showery	Fine	Rain
12		10	46 53 44	29 45 29 40	55 57	WSW	SW	Fine	Sho'ry	Cloud.
13			47 52 41	29 37 29 71	56 56	W	W	Fine		
14		13	45 59 40	29 67 29 43	55 56	SSE	S	Fine	Rain	Cloud.
15		18	43 50 38	29 55 29 54	55 56	SW	S	Rain	Sho'ry	Fine
16			42 52 41	29 51 29 65	56 60	S	S	Fine		
17			43 55 39	29 77 30 00	57 60	SSE	S	Fine		
18			42 54 44	29 82 29 60	57 56	SE	ESE	Fine		
19		04	50 59 43	29 51 29 49	58 60	E	E	Cloudy		

The quantity of rain fallen in the month of March,
is 2 inches and 64-100ths.

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JUNE, 1821.

[NO. 268.]

NOTICE.

Another of the series of PRÆMIA to the several volumes of this Journal, (which commenced with that to the forty-third volume,) comprising a History of the Progress of Medicine and its auxiliary Sciences for the half-year immediately previous to the period of their production, respectively, will be published on the last day of July. One of the especial intentions of those Præmia, is to present a comprehensive view of the state and progress of Medicine throughout Europe generally, and in the United States of America; an object that cannot be effected in the regular monthly Numbers of the Journal, because of the small extent of space which can be there appropriated to this purpose.

Original Communications, Select Observations, etc.

On Animal Excitability. By Dr. KINGLAKE.

ANIMAL excitability is a property inseparably connected with the living arrangement of matter. The innate powers of material agency are various, and are exerted according to the peculiar circumstances of organization in which they may be found to exist. It would seem that matter, in its minutest divisions, in its atomical as well as in its aggregate form, possesses an inherent motive power exerted in a repelling form, by which elementary and constituent substances are alike kept at certain distances, in the spheres or spaces of which the variously contrived agencies of life are incessantly performed. Whether this motive power be furnished by any generative faculty in the atomical and aggregated surfaces themselves, or be supplied by an animating principle or spirit with which all matter is, as it were, impregnated and vivified, and which becomes infinitely embodied and diversified agreeably to the particular arrangement into which it may enter, is more a question of theoretical refinement than of practical utility to determine. It is evident that the phenomena of organic life in existing circumstances cannot have a being without either the generative or mechanical influence of matter. To know the properties of life, therefore, in material arrangement, is an attainment of the highest importance; and to understand them in the various efficiency with which they are exerted, is to comprehend and distinguish the general and particular forms of animal existence. It is most clear that matter, whether originating or exhibiting life, is the only tangible and cognizable subject of reference, in contemplating and estimating the properties and conditions of the principle of animation.

During the continuance of life, the power by which the animal frame is impressed and affected by agents is appropriately termed excitability, because it refers to a condition that may be excited by the different agencies that are necessary to the living state. It is with this excitability that physiological speculations on the nature and properties of life are chiefly concerned. The general nature of this excitable property is shown by its diffusion over the whole animal system, enabling every part to be acted on agreeably to the scheme of life with which it is connected. This extended character of the property may be regarded as the constitutional excitability: it belongs to every part of the frame, and forms an aggregate of impressible or excitable power on which the nervous functions of life depend. Whether this excitability be the creature or the embodied cause of animal being, it matters not, with regard to its vital effervency: it is the great property with which medical inquiries have to do, and by which the validity of theories on the nature and cure of disease must be decided. The manner in which the excitability of organic life is distributed throughout every part of the frame, amidst the vast variety of structure and function that presents, evinces that its motive power is not limited to any part, but that it holds an undivided and an indivisible connexion with the whole. By this provision is secured an integrity in the phenomena of life, on which depend the order and harmony that characterize the healthful state of animal existence.

Animal life consists of a variety of functions; its objects are too numerous and diversified to admit of fewer offices. To furnish the means of executing what is necessary to life, parts are so constructed as peculiarly to fit them for the particular function they were respectively designed to perform. It is in this peculiarity of structure is found an excitability that may be denominated specific, because it possesses certain powers that enable it correctly to fulfil the intention for which it is provided. The dissimilar parts of the frame are all marked by conditions of excitability that adapt them to their destined uses. The structure that evolves an appropriate excitability, is an object deserving close attention, as being likely to unfold important truths, and to exhibit latent conditions of both the healthy and morbid states of life that indispensably require to be correctly known and duly regarded. The different textures of the animal frame are necessarily associated with such corresponding states of excitability as are requisite for the performance of their respective functions. The brain has its peculiar arrangement, its specific organization; and, in connexion with that structure, exists an appropriate excitability that constitutes the vital conditions and special motive powers of that

organ. Its function is to feel, and to extend that power over the whole frame. On this sentient property, emanating from peculiarity of structure either generatively or mechanically, are founded the rational capabilities of the mind, and all the active energies of the body arising from the sensorial powers of animal life. An unequivocal, or even an intelligible, comprehension of the nature of that peculiar power by which the nervous structure performs its complicated function, is probably an arcanum that will remain locked up in the cabinet of nature, as a curiosity not necessary to be known, as an extent of research to which the prying inquisitiveness of man should not be carried; or, in other words, it is an object for the detection of which the structure itself does not appear to have furnished adequate means.

In the peculiar excitability occurring in different arrangements of organic structure, that of the visceral parts is most evident, and is connected with purposes of the greatest consequence to animal existence. The heart stands pre-eminently high in the scale of general, as well as particular, excitability; it may be justly regarded, perhaps, as the *primum et ultimum mobile* of animal life. Its structure endues it with its power, by furnishing it with an excitable aptitude for its great and unremitting function. The slightest deviation, whether in the excitable state or in the manner in which it may be excited, is attended with serious ailment,—an ailment felt at the very source of life,—at the origin of arterial action,—at the fountain from whence is derived the active energy that is intended to pervade the whole system. If this viscus be unduly excited by disease, or if its excitability should be altered and distempered by change of structure, no health can be experienced whilst such grievance remains. In common with all other textures, it is liable to change of tone, to oppressive fullness, to inflammatory action, to augmented and diminished bulk, to osseous conversion of structure, and even to ulcerative decomposition. This viscus, like others, too, has its appropriate stimulants, which are requisite to its healthy function,—such as the oxygenous principle of the atmospheric air, and having its cavities duly distended with the circulating fluids. Where these are wanting, disease arises; and, if furnished in excess, deranged health is equally the result. The provision for health which is laid in the fabric and correspondent excitability of the organ itself, admits of slight changes from the natural state, unconnected with altered structure, without danger. These changes, however, might induce morbid susceptibilities for becoming unduly affected. The excitable power might be either too readily or not sufficiently acted on; either immoderate or torpid action might equally, though differently, disturb the

healthful state. The function of the heart, in all its relations to the several offices of the other viscera, and in all the morbid deviations to which it is liable consistently with the recoverable state, are objects well deserving the earnest attention of philosophical pathology.

The pulmonary structure is peculiarly adapted to the function of respiration. Its provisions for the distinct transmission of circulating and aerial fluids, are exquisitely contrived to insure an exact performance of the vital offices with which they are severally connected. From this peculiarity of structure results a condition of excitability, that precisely fits it for the purposes meant to be served by it in the animal economy. The standard and variable states of the specific excitability evolved by this structure, furnish a wide scope for the healthy and morbid action of the respiratory organs. Into these peculiarities researches should be carried, in correctly estimating the nature and tendency of pulmonary diseases. The membranous, vascular, vesicular, glandular, and cellular textures, of which the general pulmonary structure is composed, should be distinct objects of exact inquiry. When so much depends on diversity of structure in the relative actions of health and disease, it cannot be too minutely examined, nor its vital and morbid conditions be too closely scrutinized. The secret processes of nature, and the recesses in which they are carried on, are legitimate objects of inquiry; and they are not so hidden and inaccessible, but the labour in endeavouring to explore them may be rewarded, if not with complete success, most certainly by the acquisition of important additional knowledge.

The peculiar fabric entering into the organization of the stomach, presents a visceral arrangement of very singular and vast powers in the animal economy. The function of what may be called the gastric system, is elaborate in its contrivance and most important in its effects. The vital machinery adapted for such an office must necessarily be endued with excitable powers appropriate to such use. The precise nature of these powers, and the laws by which they are regulated, deserve the most correct attention. The stomach, as an original or insulated viscus, is of the utmost moment to life and health; and its relative and sympathetic connexions with other vital organs, and indeed with the various structures composing the animal frame, are highly influential in both the salutary and morbid actions of life. The gastric juice furnished by a secreting action performed by the fabric of the stomach, possesses, in its natural state, a solvent power, that is indispensably necessary to the digestive process carried on in the stomachic cavity. The fibrous, membranous, and cellular textures of the stomach, have respective uses assigned them, on the concert and accuracy.

of which the integrity of the gastric function essentially depends. The influence exerted by the excitable conditions of the stomach, under the vast range of susceptibility that may be induced by occurring circumstances, is too extensive to be defined. It obtains, more or less perceptibly, on all occasions of disease, going the length of affecting its healthy excitability. This will be either transient or permanent, according to the extent of impression that may have been made, or to the degree of deviation that may have been induced from the natural state. Whether original or sympathetic, therefore, the affections of the stomach are of the most serious concern in pathological consideration.

The intestinal canal is destined for digestive and transmitting offices; its structure, therefore, is adapted to these objects. It consists of membranous, cellular, and fibrous textures, which, in their separate and conjoint powers, possess excitable properties suited to the purposes for which they are designed. The constituent arrangement of this fabric generates the vital conditions on which the peculiar excitability and function of the intestinal portion of the first passages depend. To know minutely the structure of this part is important, as it suggests the extreme caution that should be observed in estimating the true cause and appropriate mode of treating the diseases to which it is liable. Anatomical knowledge unfolds lines, dimensions, figures, and textures, which are the legitimate grounds on which rational speculation may be raised as to the real nature of the living principle, in its various healthy and morbid states. A just conception of power, whether atomical, organic, or chemical, cannot be formed without understanding the precise machinery through which, or rather by which, it becomes operative. Effects cannot be comprehended without tracing them back to their causes, and thus unravelling the chain of influence by which they have been produced and manifested.

The liver is a viscus performing an important function in the animal economy. Its bulk is extensive, and is provided with a variously-tubulated and parenchymatous structure, adapted to the peculiar uses for which it is intended. It receives a large portion of the venous blood of the whole frame, from which is extracted, by a secerning process, the bilious fluid, which appears to be at once necessary for digestive purposes in the first passages, and to remove from the sanguineous mass alkaline principles, that would injure if retained in the system. The bilious fluid is materially, but not formally, present in the blood; the liver, therefore, by virtue of a vital process, for which it is fitted by peculiar organization, arranges the biliary constituents, and presents them in the format state of bile.

The circumstances connected with the formation of bile in respect of the bulk of the hepatic organ,—the quantity of venous blood passing through it,—its comparatively slow progress,—its occasional detention, inducing congestive fullness and disease,—the low degree of irritable and sentient power in the liver, are peculiarities contrived for indispensable uses. The fabric that has furnished the mechanical requisites for giving effect to vital processes that will remove materials from the circulating mass that would be injurious if retained, and to arrange them into a compound that is necessary to the digestive function, and that also serves to regulate the reflux of the venous blood to the heart, is endowed with a structural excitability that renders it capable of exactly performing its allotted office; and the deviations from that healthy state of natural excitability, are shown in diseases that must always refer to the precise conditions that may have induced the morbid change. The liver is distinguished from every other visceral organ, by the vicarious office which it executes of transmitting more venous blood than belongs to its own fabric. It has its own arterial and venous blood in common with other viscera, but, superadded to this, it receives and forwards to the heart the reflux or venous blood of the abdominal, or chylopoietic, viscera. This auxiliary function associates it intimately and extensively with the healthy and diseased states of other parts; and therefore, perhaps, subjects it to a greater range and variety of morbid affection than is liable to befall any other vital organ.

The kidneys, with their appendages, the ureters, urinary bladder, and urethra, present a system of contrivance for the removal of the superfluous water from the mass of circulating fluids, that at once challenges admiration and merits the closest physiological attention. The construction of the renal and urinary machinery, like that of all other parts of the animal frame, is simple, and exquisitely adapted to fulfilling the purpose for which it is designed. The hepatic structure secretes, or forms, by a vital process peculiarly its own, the bilious fluid: the renal texture, exerting, in like manner, its peculiar powers, separates an aqueous fluid more or less impregnated with mucilaginous and saline materials, of an excrementitious nature, and which require to be periodically removed from the system. What is the peculiar structure that performs this function? The anatomist replies by demonstrating and detailing all that the knife can reach, and other devices for exploring hidden recesses can trace; but this knowledge, no more than that of the respectively peculiar fabric of the liver, brain, lungs, or any other visceral organ, affords no satisfactory solution of the question, how, or why, such an effect should result from such a

cause. The principles of causation may be rigidly scrutinized, and all their authorities strictly applied, yet not a step is advanced in the inquiry beyond the simple knowledge that certain vitalized machinery produces definite effects necessary to animal life. *Causa latet, res ipsa notissima*. Like the liver, the lungs, and all other secreting organs, the kidneys have secerning vessels, by the exertions of which are effectuated the specific arrangement that constitutes the peculiar nature of the secreted fluid. Why should the *tubuli uriniferi* form urine? and why should they not? are correlative questions, only answerable by a solution of the respective difficulties of the subject. They are endued with life, and placed in relative circumstances of organic connexion, of temperature, of chemical and mechanical influence, that would render such an effect inevitable. In all vital processes, an elective, assimilating, and an organizing power, is exerted, that can neither be controlled nor imitated by artificial contrivances. It is necessary that the superfluous water of the blood, and other excrementitious materials of which the urinary fluid is the vehicle, should be seasonably removed from the system, or hurtful and eventually-destructive redundancy must speedily ensue. If any portion of the constituent substances of the blood should escape through the kidneys, it evinces a diseased state of the healthy secretion that should have obviated such an occurrence. The serous part of the blood, and even the blood itself, occasionally pass the urinary organ; as do also the same fluids, in hepatic diseases, through the biliary ducts of the liver. All secreting offices are executed by an appropriate mechanism, so vitalized as to possess the peculiar excitability necessary for the specific purpose meant to be effected.

The spleen is an organ executing, no doubt, an important office in the animal economy. Its obvious use is not so striking as that of other organs, which are evidently performing secreting or other functions. The glandular arrangement for extracting and transmitting some peculiar fluid from the blood passing through it, does not exist. Unlike either the uriniferous or biliferous tubes, it possesses no visible means of either forming or conveying any fluid that may have been secreted from its circulating blood. What, then, is its function? It is not a superfluous organ; it does not furnish an instance of being a work of supererogation, seated almost in the very centre of the animal frame. It is probably an auxiliary organ to the several functions of the stomach and liver, supplying these viscera with the additional, and perhaps modified, blood that is necessary to furnish the requisite quantity and quality of gastric and biliary fluids for digestive purposes. Alimentary substances would most likely not prove sufficiently nutritious to sustain

the wear and tear of vital action, if the solvent power possessed by gastric and biliary fluids, secreted by respectively peculiar vessels, were not exerted on them. An adequate supply, therefore, of these agents is indispensably necessary to the intended effect. The spleen, not unlike the vena porta of the liver, for evident bilious purposes, would appear to furnish the necessary quantity of sanguineous fluid to the stomach to insure a due secretion of gastric juice for decomposing the dietetic substances which are used for nourishment. The spleen has a bluish appearance, and its texture is different from that of any other part. This peculiarity fits it for its specific use. Were its office that of an hydraulic machine, merely for the transmission of a fluid, that purpose might be effected by simple tubular arrangement; but the vital action of the spleen most probably induces such changes in its transmitted fluid as are necessary to the ulterior gastric and biliary secretions with which it seems to be connected. The peculiarity of fabric and living power likewise subjects the spleen to specific aberrations from the healthy state, and, in strictness, entitles it to the pathological discrimination that is due to every instance of dissimilarity in animal structure.

The pancreas is an abdominal organ, destined to aid in the grand work of digestion. It furnishes a fluid of an aqueous bland quality, resembling saliva, which serves to dilute the bile, and in other respects to render it more suitable to the digestive purpose for which it is employed. This auxiliary organ, like the spleen, does not exist in vain; it performs an important function that could not be effected in any other mode. The peculiarity of structure, fitting it for its specific office, induces certain vital differences, relations, and correspondences, with the general and particular mechanism of animal life, that render it a just object of physiological and pathological notice, in estimating its diseases and the most appropriate method of treating them.

The testes, the vesicular seminales, the uterus, and the ovaria, are parts concerned in the generative processes of animal being. If structure can be so vitalized as to be rendered capable of the vast diversity of function that presents in the animal economy, from the evolution of thought to the arrangement of saliva; if all that is admirable in contrivance, perfect in design, and exquisite in effect, can result from difference of fabric involving peculiar vital affinities or elective powers; then it may be easily conceived that the entire formation of the animal machine, in its fetal origin and progressive enlargement to the adult state, may be in the power of the genital organs, or parts especially constructed for that creative purpose. Whether the animal fibre be considered abstractedly as a simple elongation of.

organic matter, or regarded in all the various and dissimilar arrangements, combinations, and contextures, into which it is fabricated and distributed in the animal frame, whatever be the substance, and however constituted, it is equally liable to the inherent conditions of vital organization, determinable by the particular use it may be designed to fulfil. The external differences of animal organs are striking, but those which refer to the interior and evanescent structure are not less real; and it is in an accurate analysis of them that correct and efficient information is obtained concerning the precise power, agency, and effect, which they may be respectively capable of producing.

Inherent power is incessantly operative, it directly goes to its object: it may be perverted or misunderstood by erroneous speculations of its energies and uses, but it will still prevail; and all the effects which are exhibited in both the moral and physical world, may be justly regarded, however unperceived and unknown in numerous instances, as the unremitting exertions of its established influence. Correctly to know and appropriately to direct this power, form the rudiments of all science and art, of all natural and acquired intelligence. The regions of knowledge are infinite, but they are not dark and impenetrable; they may be explored and demonstrated by the light of truth, by which alone can be unveiled the minute and latent realities of inherent power in all the various arrangements of the material universe. To attempt a correct analysis of any species of power, whether atomical, chemical, mechanical, vegetative, or animal, is to undertake an arduous task. An outline and all the main relations of any particular power may be given; yet the more minute bearings, the recondite conditions on which specific properties and influence depend, are not so directly perceived, and are always rather objects of speculative calculation than of mathematical demonstration.

The great land-marks of general science, in common with these referring more particularly to the animal economy, are as evident as meridian light, and they infallibly supply, in spite of all misapprehension and sophistry, the information that is indispensably necessary for rational existence. They form, indeed, the exhaustless sources of intelligence incessantly furnished to common-sense inquiries; they are the alphabet of knowledge,—the straight-forward course that is at once discerned and understood. But this vast magazine of knowledge is composed of various parts, all which are fit objects of distinct research. The examination that disconnects and rejoins these constituent portions of knowledge, is instructive by developing powers, relations, properties, agencies, and effects; that emanate from circumstances which the decomposing aid of strict

analysis could alone reveal. All things which exist are legitimate objects of inquiry, and they should, for purposes of correct and useful intelligence, be investigated in every absolute and relative situation appertaining to them with unsparing scrutiny.

Taunton; March 25, 1821.

Cases of local Affections of the Leg, successfully treated by Plaster-Strapping. By C. W. SMERDON, Surgeon.

I AM not aware that any other author than Sterne has dedicated a chapter exclusively to the subject of hobby-horses; still we are not to infer, from this want of respect in the ancients for such favourite animals, that they were unknown to them, but rather that they were too common to excite particular attention: indeed, taking it for granted that human nature is the same now as it ever was, we need make no scruples of conscience about determining that they were in use from the earliest periods, and will be so to the end of time.

Among the professors of the healing art, hobby-horses are, and have been, very common,—and very serviceable, too, if not to the public who have frequently been kicked by them, they have been to their riders, although sometimes they brake down with them when ridden too hard, and were then totally incapacitated from rising any more. Since the publication of Mr. Abernethy's excellent work on Local and Constitutional Diseases, and the dissemination of the no less excellent Lectures of the late Dr. Curry,—disorders of the biliary functions and of the digestive organs, have been quite as hobby-horsical among the faculty, as were the spasm of Cullen and the splendid doctrines of Brown.

It is quite natural that the promulgator of a new hobby for public use should ride it himself; and, if Mr. Abernethy has ridden his on all occasions, and sometimes too in a hard trot, the generality of people will readily excuse him for it, because the hobby is his own, and has been upon the whole of considerable benefit to them: but this excuse cannot extend to others, who, having borrowed it of him, have galloped it most unmercifully and indiscriminately, and ridden over every body and every thing that came in their way. Indeed, this hepatic hobby was such a delightful one, that the public soon got hold of it, and presently became connoisseurs in fecal evacuations: nor was it uncommon to hear females in fashionable life lament their having any liver at all; for such patients as these will always be disposed to believe the greatest incongruities which plausibly account for the disorders that they feel, rather than attribute them to their legitimate source,—the turning of day

into night, and night into day. The following case will illustrate these preliminary remarks, and will show what errors may arise from generalizing too much.

Miss F—, on the 1st of last September, applied for a singular affection of her left ankle and foot, which she has had for the last nine months. Her age, judging from her appearance, may be 30. She is of a nervous temperament; her complexion is rather florid; countenance animated, bearing upon the whole the marks of health; and her habits have always been domestic. The following account of her case I learnt from herself. About the latter end of the year 1819, she first began to feel an uneasy sensation in her ankle, but so trifling that she took no account of it, and did not prevent her from using her accustomed exercise. It did not long, however, remain in this quiescent state, but became increased in the evening after her dinner, sometimes accompanied with flushed countenance and palpitation of the heart: in the morning she usually became quite easy, the ankle exhibiting no other appearance of disease than a very trifling swelling. Her bowels were generally regular, and her appetite good. She described this uneasiness as a thrilling or creeping sensation, and sometimes, when it was worse than ordinary, she felt as if a number of needles were pricking her. To her own feelings, likewise, the ankle, during the excitement, was unusually hot, although in reality it was not so; and this morbid or erroneous feeling was increased, if the leg was not placed in the horizontal position. Sometimes her toes, and particularly the great toe, became affected with the same sort of pain. At length the disease became more severe, but still with the same character of commencing in the evening. She was obliged to curtail a portion of her accustomed exercise, and her evenings were spent very uncomfortably in retirement. One circumstance connected with this lady's temperament deserves to be noticed,—which is, that she could never be near the fire for any time without feeling a general uneasiness which she could not describe. Hot and close rooms likewise were always uncomfortable to her. This I conceive to be a mark of delicacy, particularly of the nervous system, having remarked similar peculiarities in delicate females who were highly susceptible to nervous impressions.

Her foot and ankle still becoming worse, she was induced to apply to a surgeon of the first eminence in London. He told her that her disease was constitutional, arising from a disordered state of the digestive organs, particularly of the liver; and he desired her to take three grains of mercury with chalk, every second night. This prescription she regularly followed for three weeks, without the slightest benefit: on the contrary, her bowels having been unusually confined

during this time, she became more than ordinarily heated after her dinners, and her ankle and foot more painful and swelled. After having taken twelve of the prescribed powders, she returned to her surgeon again; and, as she was no better, expressed a desire that he should examine her leg. This he declined, being convinced that the affection there was merely symptomatic; and desired her to continue her powders as before. She took six more of them; when, not finding any amendment, she applied to another surgeon, who coincided in the opinion of his predecessor.

A short time after this she went to Bath. The carriage motion occasioned by the journey was torture to her, and increased considerably the heat and thrilling sensations in her ankle and foot. Fomentations of warm water, which she now applied to abate the inflammatory appearance, only served to increase the evil. In Bath, she applied for relief to a surgeon of eminence, who prescribed for her cold lotions and embrocations. Another practitioner of respectability, who was strongly recommended by a female friend, again recurred to the hepatic theory. He told her that all her complaints arose from an obstruction of the liver, which could only be removed by a course of purgatives, rest, and low diet. Although she was not a proselyte to this doctrine, she was anxious to try any means that held out a prospect of relief. She was under the care of this gentleman near two months. His first plan of treatment was active purgatives, abstinence from animal food and wine: but, alas! the obstruction was obstinate; and therefore, contrary to her better judgment, she was bled twice within the space of a week, and she escaped from a third operation by coming to Clifton.

In July last, she first became a patient of the late Mr. Baynton. At this time her health was suffering from the active treatment she had lately submitted to. She was much weakened, and, consequently, the palpitations and other nervous feelings were much increased. The journey from Bath, and a little subsequent exertion, had dissipated the trifling benefit her ankle and foot had derived from a long course of rest; and her spirits, which had never before forsaken her, were now much depressed. Mr. Baynton's opinion was totally different from his predecessors: he considered that the disease was local, and set about treating it accordingly. He applied plaster-straps to her leg, from the foot to the knee; he enjoined her to perfect rest, and to live as usual with respect to diet, omitting her wine. The straps gave her so much pain, that, after the third day of wearing them, a firm calico bandage was substituted in their stead; and she was desired to rub her ankle with an embrocation composed of one part of Goulard's extract, two parts

of soap liniment, and three parts of rose-water. She remained under the care of Mr. Baynton, with the most marked benefit, so far as regards ease, comfort, and restoration of general health, up to the time of his death. The evening recurrence of pain in her ankle rarely troubled her while she wore the bandage with its necessary degree of firmness; but if this were left off for ever so short a time, and the limb suffered to hang down, the usual sensations would return, and not leave her until the bandage was replaced.

The shock occasioned by the suddenness of Mr. Baynton's death, brought back her nervous disorders, and with them the painful sensations of her foot and ankle; her leg also, along the course of the tibia, felt uneasy and sore to pressure. At this time it was that I first saw her. I found her mind extremely agitated by the loss of a person, from whose skill she had confidently expected the restoration of her health. Notwithstanding the long continuance of occasional pain in her ankle and foot, no external marks of disease were perceptible on them, except a very trifling degree of redness and swelling. The veins were not in the least varicose. Her extremities, she said, were generally cold; still fire was always disagreeable to her.

A little reflection on the whole of this case made it obvious to my mind, that either it was a constitutional disease producing local effects, or *vice versa*: consequently, a local disease did exist; to which remedies might be, and had been, applied with the best effect. It appeared to me reasonable that the pain which she felt in the ankle and foot, was occasioned by a morbid action of the cuticular nervous extremities of the part; and to this, I thought, might be attributed all the local affection. Pressure, by compressing the cuticular nerves, and by that means depriving them of a certain portion of their power, had been of considerable service, and might, I conceived, be rendered much more efficacious if the pressure were increased, at the same time that the skin was more firmly supported by a close adherence of the remedial means to its surface. I urged the permanent use of the plaster-strap again; but as, by their irritation, she had formerly experienced much distress, I proposed that they should be made of the soap-cerate, which, as a plaster-bandage, may be applied with an equal degree of firmness as the usual adhesive-strap; whilst it is perfectly free from any irritating quality, and may be removed without injury to the thinnest skin. She was, however, desirous to try the calico bandage only, for a short time longer; and she removed from this place to Cheltenham.

Here she applied to three medical men: the first of whom recommended her to immerse her leg nightly in a saturated solution of salt in hot water;—the second assured her that her

disease arose from an affection of the kidneys! and recommended her powders of soda, rhubarb, and columba;—the third was of the same opinion as myself with respect to the plaster-strap; and, as she consented to a trial of the plan, he applied them, according to Mr. Baynton's method, from the toes to the knee. Thus equipped, she returned to Clifton, and experienced, during the journey, the greatest comfort from the additional pressure. A few days' wearing of these straps, however, convinced her that the new irritation which they excited would effectually counteract all the good effects of this plan; and she therefore readily assented to my second suggestion of the soap-cerate. This, indeed, fully answered the most sanguine hopes and expectations of either party. The straps thus prepared were applied firmly, as before, from the toes to the knee, with a calico bandage over the whole; and she was desired to walk as much as she could without producing pain or fatigue.

At the end of a week, when the plasters were removed, she had experienced little or none of her old complaint, although walking to some distance had been daily persevered in, and progressively increased: the straps, likewise, were removed without occasioning any additional irritation. I applied them three times, allowing an interval of a week between each application; when she left this place, with every prospect of a permanent recovery.

I am well aware that, by attributing this affection, and other anomalous diseases of the same kind, to a morbid action of the nervous system, I am adducing an hypothesis, the truth of which may justly be disputed. It is, indeed, of but little moment in practice whether painful local affections, which are neither the consequences of inflammation nor ever produce it, be attributable to morbid nervous action only, or to a languid state of the circulation of the affected part, such as we know is the case in a varicose state of the veins. In both instances firm and regular pressure is the most appropriate remedy, and is as certain of producing effectual relief as the most approved medicine is for any other disease. In the one case, the nervous extremities are blunted as it were by the pressure; in the latter, the weakened veins receive an important support from it, and are thus enabled to carry on the circulation with vigour.

I have at present a patient who long had a painful affection of the knee-joint. This is a stout woman, 45 years of age. The disease commenced without any apparent cause. She complained of acute pain and heat on each side of the patella; extending behind it into the interior of the joint. External pressure likewise was painful to her; while rotating the leg, and pressing the articulatory surfaces together, gave her no uneasi-

ness. All that could be seen externally were a few tortuous cutaneous veins, a little enlarged, on the inside of the knee,—such as are generally seen as the commencement of white swelling. The pain was always worse when she was at rest; and was sometimes so considerable at night, when in bed, as to induce her to get up and walk about. Leeches, blistering, cold lotions, and fomentations, gave her but temporary relief: the pain and uneasiness returned again, as soon as the influence of these remedies was gone off. A large varicose vein which now for the first time made its appearance on the leg, determined me to try the effect of pressure. A series of plaster-straps, made with the soap-cerate, was applied, firmly commencing from the lower end of the varicose vein, and extending upwards so as to include the whole of the knee-joint; over this a calico bandage was likewise as firmly applied as possible. The relief which she received was as great as the most sanguine expectation could anticipate. So long as by these means the pressure is kept up, she has no pain in the knee or leg, and she is able to perform the laborious occupations of cook in a large family, with ease and comfort. Whether or not the cure will be permanent, I am not at present able to determine, as she has not ventured to leave off the pressure; but appearances decidedly indicate that it will be so. It is but right to say that the bandage alone, which was fairly tried before the straps were had recourse to, did not produce any alleviation of the disease.

The following cases, showing the good effect of firm pressure in herpes phlyctænodes affecting the leg, being intimately connected with the foregoing subject, may with propriety be inserted here.

Mrs. T—, by falling from a chair on which she was standing, wrenched her knee, which subsequently occasioned a good deal of pain, swelling, and inflammation. The usual remedies were applied, and lastly blisters. Two days after the removal of the last blister, (namely, Sept. 1, 1819,) the vesicated surface became greatly inflamed, hot, and very painful. Poultices, fomentations, rest, and purgatives, were prescribed. On the third, phlyctænæ in patches make their appearance, surrounded by a deep erysipelatous areola. The skin was thickened, unyielding, and glossy. In spite of saturnine and mercurial lotions, ointments of various kinds, and mild mercurial purgatives, the disease spread rapidly around the knee and down the leg, producing, by a constant succession of eruptions, numerous deep, cellular, or honey-comb, ulcers, several of which were large enough to receive the top of the finger, accompanied with a discharge of thin ichorous matter. Beside the increase of heat, and the intolerable itching of the part, which usually came on

worse at night, she felt a constant dull pain in the tibia, which gave her the sensation of something gnawing it. On the 27th of September, I determined on trying the effect of pressure. A series of plaster-straps, beginning from the middle of the leg, was applied upwards to the knee, and drawn as tight as possible; over these a calico bandage was also applied, from the foot upwards; the ulcers having been previously washed with a zinc lotion. The pain consequent on this treatment was fully compensated by the comparative ease which she felt after it was accomplished. She was desired to walk about now and then; a luxury she had not dared to indulge in for the last four weeks. This plan of treatment was daily renewed, until the 14th of October, when the ulcers were entirely healed, and have ever since continued so.

About twelve months since I met with another case of herpes, affecting also the upper part of the leg. This was in a woman, aged 43, of an unhealthy bilious habit. She had been suffering for some days before I saw her, and had applied constantly a Goulard lotion to the part, without any benefit. The disease was precisely the same as the one just narrated, and gave way, without any difficulty, to the same plan of treatment.

Clifton; April 8, 1821.

Observations on the Position of a Patient after the Operation for Hernia. By R. WADE, Member of the Royal College of Surgeons, and Apothecary to the Westminster General Dispensary.

HOWEVER unimportant the following communication with regard to position after the operation for strangulated hernia, may at first sight appear, yet its highly beneficial effects, in the case I am about briefly to relate, were so evident, and the subject but so very slightly noticed by writers on Hernia, that I am induced to request a page in your Journal for its insertion, with a view to recommend a more general adoption of the treatment.

The patient, Mrs. —, aged 58, and residing at No. 4, Crown-court, St. James's, had a strangulated femoral hernia, attended by the usual symptoms; and it will be sufficient for my purpose to state, that the remedies usually had recourse to in such cases were employed in the first instance, without the slightest diminution of urgent symptoms; when, on the fourth day of strangulation, the operation was performed by Mr. A. Copland Hutchison, surgeon to the Dispensary, with as little injury to the protruded parts as their adhesions to the sac would permit. Soon after the operation, the patient's bowels were freely relieved. Her vomiting ceased, and no bad symptoms

occurred, until the third day after the operation, when the wound became painful,—the parietes of the abdomen again very tender to the touch,—occasional vomiting,—a quick, small, and intermitting pulse,—and a violent purging, which amounted to upwards of twenty evacuations in the course of twelve hours. The sutures were removed from the wound by Mr. Hutchison; the patient was turned on her side well over towards her front, so as to make the external wound depending, and, after which movement, two or three tea-spoonfuls of ill-conditioned pus immediately escaped from the wound.

Such decided relief from all unfavourable symptoms so quickly followed the change of position in this case, as to leave no doubt on the minds of Mr. H. and myself that the patient's life was in this instance saved chiefly by moving her round from lying on her back to her side, turned well over to her front, as before stated; and this is also the conviction of the patient herself. But it is at the same time correct to state, that, when the above dangerous symptoms first showed themselves; I was directed to bleed her to the amount of sixteen ounces; and some other remedies were at the same time prescribed, such as fomentations, &c.

The conclusion I have drawn from this case is, that, though adhesive inflammation soon closes the external wound, yet, from the unavoidable injury done to the parts (beneath the situation of the sutures) during the operation, in separating the hernia from its adhesions, suppuration will consequently take place, from the sides of the wound, not so closely kept in contact as the external labiæ or line of incision, and the matter must, by the patient lying on her back, fall into the cavity of the abdomen; and thence produce pain, peritoneal inflammation, and diarrhœa, as in the case I have just alluded to. And I am the more induced to believe that matter finding its way into the cavity of the abdomen in this case, was the sole cause of all the urgent symptoms that did occur, because they so immediately gave way to the treatment above adverted to,—namely, change of position.

We are favoured with but very few dissections after the operation for hernia when death has ensued; I should, therefore, consider it well worthy the attention of the practical surgeon to examine minutely, after the death of patients subsequently to this operation, into the causes of dissolution; and whether matter finding its way into the abdomen, be not frequently the cause of a fatal issue.

21st April, 1821.

History of a Case of Lumbar Abscess attended with Premature Abortion in the Groin. By W. E. HOWEN, M.D. Demonstrator of Anatomy in the University of Pennsylvania.

ON the 15th of August last, I was requested by Dr. Gebhard to examine a patient of his, who had died in Callowhill, near Garden-street, in consequence of psoas abscess. The account of the case afforded me was as follows:

James Culberton, a carter, aged twenty-four years, of a very strong and athletic constitution, in consequence of some injury or strain received in the prosecution of his business in August 1814, felt a severe pain arise in the lumbar and iliac regions, which was followed with fever and the common symptoms of inflammation.

Bleeding, purging, and other means usually resorted to on such occasions, were adopted, without any other effect than that of alleviating in some measure the intensity of the attack. From the inadequacy of the remedies to produce discussion, suppuration took place: the matter forming passed out under Poupert's ligament, and produced a prominent and fluctuating tumor, a little below it. This tumor was opened with a lancet, and a considerable quantity of pus discharged. With little subsequent attention the wound got well, and the energies of the patient's constitution seemed to have restored him in a short time to his ordinary state of health.

In the middle of the next winter, from being exposed to a snow-storm, he caught cold: the place in the groin which had been occupied by the tumor, and in the vicinity of the incision, became heated, painful, red, and tumefied; suppuration followed, and a spontaneous opening was made, through which the pus, mixed with feculent matter, was discharged. Doubt might have been entertained at this time in regard to the real nature of the feculent discharge; but the patient having eaten some rice shortly before, it so happened that a few grains of it passed undigested through the opening. This accident demonstrated that a connexion existed between the intestinal canal and the orifice in the thigh. A number of perplexing considerations grew out of this circumstance. 1st. It was thought that a femoral hernia, lying under the tumor, might have been wounded by the plunge of the lancet; but, upon a rigid scrutiny into the case, it appeared that the patient had never in his life been affected with any of its symptoms, and a local examination of the part itself at the time indicated nothing of the kind. It, however, was considered possible for it to have had a temporary existence, and to have been afterwards returned by the voluntary action of the bowels, assisted by the

position of the patient. 2d. If the primitive inflammation in the lumbar and right iliac regions had invaded the intestinal canal, and, by the process of adhesion to it and of ulceration into its cavity, established a communication, it was still a desideratum to ascertain what portion of the bowels was thus affected. In ten days this orifice healed, and continued so till April 1816, a period of three months. At the expiration of this time another inflammation about the orifice took place, followed by suppuration and ulceration; feces were again discharged for ten days along with the pus. The parts then healed, and continued so till February 1817. During this interval the health became sufficiently good to induce him to consider himself exempt from the chance of similar attacks; but these hopes were destroyed, about the middle of February, by a recurrence of the inflammation in the thigh, with discharge of feces.

Between this date and the following August, four fresh attacks supervened; after that, he suffered from them every three or four weeks till December. It is now necessary to observe, that, in the recent recurrences of the disease, the old orifice, which had been situated not far from the anterior superior spinous process of the ilium, remained cicatrized, and a new one formed nearer the pubis. Also, that the inflammation which had been the precursor in every instance of the discharge of feces, was not attended latterly with suppuration, but that the integuments affected by it, being protruded by the accumulation of feces, burst and gave vent to them. The inflammation then subsided, and the part healed. It was therefore apparent that the feces occasioned the inflammation in the groin. The inflammation had, during this time, extended itself into the contiguous parts, and, amongst others, affected the inner side of the thigh, two-thirds of the way down to the knee.

The sequel of the case, as communicated to me by Dr. Gebhard, is as follows:—In December 1817, he first saw it. The patient was then labouring under a violent symptomatic fever: the most rigid antiphlogistic plan was pursued, such as bleeding, low diet, refrigerants, &c. By these means he was relieved from the constitutional affection. The subsequent exhibition of bark seemed to strengthen him, and to diminish the intensity of succeeding paroxysms, which took place in connexion with the bursting of feces through his thigh every two or three weeks till his death.

In February 1820, he became anasarceous, and continued so until the middle of July: he was then seized with a sore throat, which, in its sensations of pain, was extended along the oesophagus into the stomach. The sensibility of those parts be-

came so highly excited, that the swallowing of such bland fluids as flaxseed tea, milk and water, &c. was excruciating. Examination showed the pharynx and palate inflamed, but no swelling of the tonsils. Gargles being ineffectually exhibited, the application of a blister on the throat relieved him. The dropsical affection began to disappear simultaneously with the commencement of the sore throat; and, by the time the latter was cured, the former had subsided entirely. A diarrhoea had attended the dropsical affection from its beginning; his health sunk apace under its influence, notwithstanding the continuance of a good appetite. The exhibition of medicines suppressed the discharge *per anum*, but produced no effect on that through the artificial opening. Wasted by this continual drain from the alimentary canal, he at length died on August 15th, 1820.

Examination post mortem.—The appearance of the abdominal viscera, generally healthy. The psoas magnus and iliacus internus muscle, which were considered to be the seat of the primary affection, had lost entirely their muscular fibres, were diminished in size, somewhat indurated, and converted into a ligamentous-like mass. The head of the colon, besides being bound down to the right iliac region by the usual reflection of peritoneum, had contracted an extensive adhesion to the contiguous muscles. The peritoneum at the part alluded to seemed unaffected by disease. It is known that the colon, at its head, is covered only in two-thirds of its circumference, and that anteriorly, by the peritoneum; the rest of this portion of the gut being in contact with the iliacus internus muscle, and connected to it, in health, by loose cellular substance: it is the latter part that I allude to, as having united itself to the iliacus muscle, by a preternatural adhesion, which was strong and so compact as to produce a continuity of substance with the muscle itself. At this place, about an inch and a half above the valve of the colon, two orifices existed through the parietes of the gut, each large enough to admit a finger: these orifices communicated with one fistula of the same size, in the centre of the iliacus internus. The fistula passed out of the abdomen, continuing in the centre of the muscle, till it had got beyond Poupart's ligament; it then became superficial, and terminated in the orifice of the groin, so often alluded to. A prosecution of the dissection exhibited another fistula arising from the lower termination of the first, and extending downwards six inches, parallel, or nearly so, with the femoral vessels. It was not known whether this fistula had arisen at the commencement of the disease, or from the aggravation of the local inflammation which took place about August 1817: its connexion with the

other was subsequently demonstrated, by the ability of the patient to discharge feces from the external orifice, by pressing along the course of the adductor muscles from below upward. The femoral artery and vein were imbedded, as far as the place where they perforate the tendon of the triceps adductor, in a ligamentous sheath, which, I presume, had been fabricated in order to protect them from the extension of the disease in their vicinity. The parietes of each fistula were so thick and perfect, that they might have been, with ordinary facility, dissected from the contiguous parts, and preserved in their membranous form. The vertebræ of the loins were in a healthy condition.

This case I believe to have been unusual, in regard to the communication established between the abscess situated in the iliacus internus and psoas magnus muscle, and the cavity of the colon; and it may, perhaps, prove serviceable, by calling, in similar cases, the attention of the practitioner to the cause of a series of symptoms which embarrassed exceedingly all the medical gentlemen who were consulted about it. It is also a good example of the species of lumbar abscess, which, in the language of Mr. Abernethy, proceeds from phlegmonoid inflammation in the part. The circumstance is familiar to most surgeons that there are two species of lumbar abscess: the one preceded by pain, tumefaction, increased heat, throbbing, and a hurried circulation of blood through the loins, and a secretion of coagulating lymph, by which the parts are united to each other, and the boundaries of the abscess circumscribed; constituting, by all these symptoms, common inflammation. The other species belongs to chronic abscesses, in which collections of matter take place without any evident act of inflammation. In these latter, the surrounding parts remain in a great degree unaffected by the diseased action: the purulent discharge commences from very small beginnings, increases gradually, accumulates indefinitely; its boundaries are soft and unattended with thickening, affording but little impediment to its gravitation; and the matter, therefore, passes from one part to another, appearing sometimes in the groin, sometimes just above the knee, and occasionally in the perineum, giving occasion, by this change of place, to the distinction which Mr. Hunter has made between abscess *in a part*, and *of a part*. This last form of the disease, supposed to depend on a scrofulous habit, has its peculiar mode of treatment, which is very satisfactorily illustrated in Mr. Abernethy's Surgical Observations: but the former, differing essentially from it, and partaking largely of the attributes of common inflammation, has its remedial indications accordingly. The case just recounted seems to me, from the primary symptoms, and from the alteration of struc-

ture in the parts affected, to have been one of phlegmonoid inflammation; and would have been cured by the treatment, had not the diseased action extended itself through the parietes of the colon, and by that means produced a constant evacuation of feces into the cavity of the abscess, which eventuated in incurable fistula and artificial anus.—(*Philadelphia Journal of the Medical and Physical Sciences*. Edited by Dr. CHAPMAN. No. 11.)

Some Observations respecting the Structure and Functions of the Udder of the Cow. By Mr. JAMES WHITE.

THERE is a curious provision in the structure and economy of the udder of the cow, which appears worthy of the attention of physiologists in general. I have shown, in some Papers written for the Bath Agricultural Society in the course of last year, that the cow's udder is not a gland, as it is commonly supposed to be, but merely a receptacle for milk, which is chyle, and is formed in the fourth stomach. This is capable of a clear and simple demonstration. When the cow arrives at a certain age, and begins to approach towards that state when she is no longer capable of breeding or forming milk, there is a provision for a gradual obliteration of the udder; the cells of which it is composed being broken down, and changed into a thick purulent fluid, which may be drawn off from the teat by stroking it. Most commonly, however, it is left entirely to nature, and a gradual thickening and hardening of the quarter, as it is termed, takes place. The pus appears to become solid and organized, or the purulent fluid may in some instances drain off; but in either case the quarter is gradually filled up, and changed into that fatty kind of substance which we observe in the udder of cow-beef.

A similar process (but without any purulent fluid being formed, which happens only, I believe, in old cows,) takes place in the animal at all ages; when kept from breeding; and, when the animal is young and fat, the udder approaches more nearly to the same state,—that is, good fat.

I should have observed, that the cow's udder is composed of four distinct receptacles, which are commonly named quarters, each of which is supplied by a distinct milk vessel. The two trunks of these vessels may be seen under the cow's belly, when the udder is distended with milk; and, if punctured when in this state, the proof I have before alluded to may be obtained.

I have observed, in fat cows, that the teat is converted into fat; but, on cutting off the teat transversely, some mark of the cavity is generally seen.

The discovery I have made of the structure and economy of the cow's udder, and the experience I have had in the diseases of cattle within the few years that I have devoted a considerable portion of my time to the subject, has enabled me, I trust, to make some considerable improvements in this branch of veterinary science. The result of my researches will soon be published, in two volumes: the one, a Compendium of Cattle Medicine; and the other, Essays on the Structure, Economy, and Diseases of Cattle, and especially of the digestive and lactiferous system of the Milch-Cow.

Bath; April 5, 1821.

Cases of Gonorrhœa, successfully treated by Cubebs. By MILLS MARLEY, Member of the Royal College of Surgeons.

AS we are daily hearing contradictory reports respecting the efficacy of cubebs in cases of gonorrhœa, and as I have given that medicine fair and repeated trials, I think I do no more than my duty in laying the results of my experience before the profession. I must here intimate that it has been my aim to compress the substance of this communication into as small a compass as possible, and to state nothing respecting the remedy that might undeservingly enhance its reputation. I have written it with a mixed feeling of confidence and distrust, hoping that it may apologize for itself by its good intention, namely, that of strongly recommending a medicine capable of quickly curing one of the most frequent disorders that comes under the practitioner's care; and at the same time, fully aware of the opposition the remedy has already met with, I do not mean to assert that the medicine under consideration is an infallible remedy in all cases, but candidly acknowledge that it has seldom or never answered my expectations in gleets, or in cases of above a month's standing. In cases of a recent nature, I think I may call it a specific. Much depends upon the medicine being genuine, which I am inclined to think is not always the case; and to that circumstance I, in a great degree, attribute the failures which have occurred in the use of it. The dose is likewise a matter of importance: I never give less than from one to three drachms three times a-day, having found that smaller doses are of no service. During the use of the medicine, it is necessary to restrict the patient to the antiphlogistic regimen. In some instances I have been led to think that small doses of calomel expedite the good effects of cubebs; and, with this view, in cases not of a very recent nature, I generally give a couple of grains at bed-time, about every other night. The head and stomach are always more or less affected during the exhibition of cubebs, and there is generally a tendency to

constipation, which must be obviated by some of the neutral salts. I mostly desire it to be taken in milk; but, should the stomach prove too irritable to retain it in that form, coffee will be found an excellent vehicle. To guard against a relapse, I always continue the use of the medicine three or four days after the disease has disappeared.

CASE I.

Jan. 28th, 1821.—Robert Lincoln, aged 24, had connexion about nine days ago; complains of considerable pain in making water, and has done so for the last four days; he says, he has had scarcely any sleep of late, owing to chordee. He perceived the discharge on the fourth day: it was white, and of a thin consistence, and has remained so to this time. Let him take of powdered cubebs, ʒij. three times a-day.

30th.—No pain on expelling his urine, nor has he been troubled with chordee. The discharge not altered.

31st.—The discharge much thicker. Continue as before.

Feb. 2d.—The discharge almost gone. Continue.

4th.—Says the discharge stopped yesterday, (the 3d,) and has not since returned. Continue for two days longer.

CASE II.

Dec. 14th, 1820.—A young gentleman, aged 19, had connexion eight days ago; yesterday perceived a discharge of a purulent yellowish matter, slightly tinged with blood, accompanied with a tickling sensation in the course of the urethra. To-day he complains of much pain on making water. Let him take of powdered cubebs, ʒij. three times a-day.

15th.—Little or no alteration. Continue.

16th.—Complains of a slight heat, not amounting to pain, in the urinary passage; the discharge is thicker. Continue.

18th.—No unpleasant sensation in the urethra, and the discharge is lessened considerably. Continue.

20th.—No symptom of the disease now remains. Continue for two days longer.

CASE III.

Oct. 19th, 1820.—A man, aged 24, consulted me for a discharge of a thin yellowish matter. He had connexion a fortnight ago; perceived the discharge about five days after; thinks it increases in quantity every day; complains of much pain on making water, and has had chordee for the last three nights. Says he has had the disease five or six times, and has never been cured in less than two months or six weeks. Let him take of powdered cubebs, ʒij. three times a-day in milk; and of calomel, two grains, every other night at bed-time.

20th.—Took but one of the powders yesterday, his stomach being irritable. Continue; let the medicine be taken in coffee.

22d.—The powders have been retained on the stomach; no ardor urinæ; the discharge is thicker, and less abundant.

24th.—I was not a little astonished to find my patient free from all symptoms of the disease. Continue for three or four days longer.

CASE IV.

Oct. 26th, 1820.—A gentleman's groom, 22 years of age, has had a discharge of purulent matter from the urethra for the last five days; it does not seem to increase in quantity; perceived nothing unpleasant till yesterday, when he felt a scalding pain on expelling his urine, accompanied with redness and swelling at the end of the urethra. Let him take of powdered cubebs, ʒij. three times a-day, and apply cold lotion to the penis.

27th.—The discharge unaltered; the swelling, redness, and ardor urinæ, much diminished. The left testicle is nearly twice its natural size; which I attribute not to the medicine, but to the patient having taken a long rough-ride yesterday afternoon. Continue the powders as before. Support the testicle by means of a suspensory bandage, and apply cold lotion.

29th.—The swelling of the testicle has completely subsided; complains of nothing now but the discharge, which is gradually diminishing.

Nov. 3d.—Is quite well. Continue two days longer.

CASE V.

Jan. 25th, 1821.—Mr. L— was infected six days ago; did not perceive any thing unpleasant till yesterday morning, when he discovered a discharge of a straw-coloured matter, accompanied with a scalding on making water. Has never had this form of the disease before, but had chancres some time since, for which he took mercury, and was cured in the course of six weeks. Let him take of powdered cubebs, ʒiij. three times a-day, in milk.

26th.—Complains much of nausea, and begs of me to change the medicine. The discharge lessened, and of a thicker consistence. Continue to take the powders in coffee.

29th.—Is perfectly free from the disorder. Continue four days longer.

CASE VI.

Feb. 27th, 1821.—P. H. esq. aged 25, had connexion nine days ago; three days after, he found an abundant discharge of yellowish matter, which has continued ever since; complains of

pain and difficulty in making water; is troubled much with painful erections, and a blush of inflammation surrounds the orifice of the urethra. Take of cubebs in powder, ʒiij. three times a-day. Apply cold lotion to the penis.

29th.—No pain on making water; has been but little troubled with chordee; the discharge is much the same in quantity, but begins to assume a thicker appearance. Continue.

31st.—The discharge is almost gone. Continue.

March 2d.—Cured. Continue two days longer.

CASE VII.

Jan. 27th, 1821.—A man, aged 35, has had a discharge of purulent matter from the urethra for the last four or five days; he occasionally complains of slight pain on making water. I desired him to take ʒiss. of powdered cubebs three times a-day.

30th.—The discharge is scarcely perceptible; no pain on making water.

Feb. 1st.—The complaint is completely removed. Continue two days longer.

CASE VIII.

Nov. 24th, 1820.—A gentleman, 40 years of age, had connexion five days ago; complains of scalding on making water, with much difficulty in retaining it; has a profuse discharge of purulent matter, with slight excoriation of the glans. Let him take of powdered cubebs, ʒij. three times a-day.

25th.—There is little or no alteration. Continue as before.

26th.—All the symptoms are considerably relieved, but the discharge, which still remains profuse. Continue.

28th.—Complains of nothing now but the discharge, which is rapidly subsiding. Continue.

Dec. 1st.—Is quite well. Continue three days longer.

I have now to trust that the success of the above cases, taken from many others of a similar kind, sufficiently show the grounds upon which I was induced to make the present statement.

Vigo-lane, Burlington-gardens;

March 16th, 1821.

COLLECTANEA MEDICA:

CONSISTING OF

ANECDOTES, FACTS, EXTRACTS, ILLUSTRATIONS, &c.

*Relating to the History or the Art of Medicine, and the
Auxiliary Sciences.**Floriferis ut apes in saltibus omnia libant,
Omnia nos itidem depascimur aërea dicta.**Extracts of the Report from the Select Committee of the House of
Commons on the Doctrine of the Contagion of the Plague, in 1819.**Veneris, 26^o die Martij, 1819.*

SIR JOHN JACKSON, Baronet, in the Chair.

SIR ARTHUR BROOKE FAULKNER again called in ; and Examined.

I THINK you wish to add to your answer respecting the air of Malta?—I was observing that a high wind, from whatever quarter it blew, was always accompanied with some increase of the number of infected.

Can you give an undoubted proof of the contagion of plague, from any fact that came under your own eye or personal knowledge?—At the military barracks, I think, certainly.

What was it?—The attacks being *consecutive*, as from contact, instead of being *simultaneous*, it was impossible to trace immediate contact with an infected person in barracks, where soldiers were so much together, and lived so gregariously.

If the air was the general cause of plague, must not that have operated as the cause in the instance to which you have alluded?—Certainly not. If the air had operated as the cause, the disease would not have extended consecutively, as I have shown to be the case, but would have been produced through the corps simultaneously. The same would have happened through the population of the island ; especially as there were several parts of the island, from local circumstances affecting the air, more exposed to contamination than Valetta.

Is Valetta a lower part of the island than the other parts?—I am not quite sure respecting that ; but it is universally allowed to be one of the most healthy parts of the island, the most free from marsh fevers ; and, as an instance, I may mention that, in 1801, when marsh fevers were very fatal and numerous, Valetta entirely escaped.

Was there any fever, except on-board the San Nicola, at the time?—I cannot speak with positive certainty, but I think it not unlikely, as vessels from Alexandria, an infected port at that time, were not excluded from entering the harbour contiguous to Valetta.

Whereabout was Salvador Borg's house?—In Strada St. Paulo.

Is that far from Valetta?—It is in Valetta.

Is Valetta the fort?—It is a considerable and well-fortified city, the largest in the island. It is the last spot where one would look for

marsh-fevers or diseases of any kind, being perfused with pure sea air in every direction, and the soil being perfectly dry.

Would not the numerous instances you have alluded to, of persons having escaped the plague who had come from the bosom of families afflicted with it, be an inducement, if not a proof, for deeming plague not contagious?—Neither an inducement nor a proof; for we see in other diseases allowed to be contagious, the small-pox for example, that many such escapes have taken place.

Have you any better proof for deeming plague contagious than the preceding observations on my question for deeming it not contagious?—I think that when the whole evidence I have given, respecting the propagation of the plague in a direct line, be well weighed and considered, and when we see that any other supposable cause than contagion is inadequate to account for such extension of the disease in a direct line, the proof is made out as far as presumptive evidence can well render it.

Do you know how many died of the plague in the harbour?—None in the harbour: two, I believe, died on their passage to Malta from Alexandria, and two died in the lazaretto after the infected crew were sent on shore.

You know the ship *San Nicola* was sent back to Alexandria, without unloading, from Malta?—It was notoriously stated.

And do you know that the goods were landed at Alexandria, and none of the persons took the plague who went with her?—I believe none of the persons who navigated her back took the plague, but arrived in perfect health; farther I know not; I know nothing of her unloading. I have said, there was a report that some bales of goods were missing; but I only speak from public conversation.

Is it not more likely that the persons who went with her to Alexandria should have received the plague, than that a bale of linen carried into Malta from her should have produced the plague?—The ship had undergone a very considerable quarantine; and I heard that means were taken to prevent her from infecting those engaged in her navigation.

Do you suppose her goods were unladen?—I believe not. In every other respect, as far as circumstances would admit, without unloading, I have heard she was expurgated from infection above decks; but I am not certain of this.

Do you believe it would be possible to ventilate the goods without unloading them?—I think it would be impossible.

What number of military were at Malta in 1813?—To the best of my recollection, about 4,000.

What was the greatest number of military afflicted with the plague at the same time?—I am not prepared to answer, as the returns of the whole army were never in my possession; but I can speak as to those under my own care; I have the return of them in my pocket. 4th July, the first case was placed officially under my care; the second was the 8th July.

What is the greatest number at any one time?—I beg to read the return. The third case was the 20th July, the fourth case the 21st

July, the fifth case the 21st July, the sixth case the 23d July, seventh 25th July, the eighth case is the 28th July, the ninth case is the 2d August; I believe that is the last officially under my care.

The sick of all the persons were under your care?—Not of all the forces; some of them were attended by their own regimental surgeons within their own barracks. Those sick only properly belonged to me officially that were sent into a general hospital, although I did attend also in the regimental hospitals. We were at one time so ill off for a general pest-hospital, that medical staff officers were under the necessity of attending plague-cases at the barracks under canvas.

Have you any recollection of the number of soldiers altogether taken ill of the plague?—In all, our army had not hitherto lost above twenty, up to the date before mentioned.

What date?—Up to October.

Were any taken ill after that?—I believe some solitary instances occurred after that. We lost very few in all.

Was any medicine administered which was successful in these cases?—Some articles seemed to be very successful, but there could be no dependence placed upon any.

Would you like to mention any?—Those which I found most beneficial, were the cold affusion and turpentine; to which of the two it is difficult to ascribe the good effects.

Did you use mercury?—I did.

Blood-letting?—Yes, by leeches,—topical blood-letting. Camphor I also used, and several other medicines. Calomel was reported to me as having done some good under the care of the garrison battalion surgeon, but I never found it myself of any use.

Of those infected with the plague, how many do you suppose recovered among the soldiers?—I can only speak as to De Rolle's regiment; I think nearly one half.

Was there any plague at Sasi?—None; nor at some other cassals in the island.

To what do you attribute that?—To infected persons not having made their escape to those cassals.

Was there an order from the commander in chief not to feel the pulses of the plague-patients, under a fine of eighty days' quarantine?—There was. I find now, on referring to my notes, that I have made a mistake: the medical men were interdicted from feeling pulses only under a penalty of quarantine for a *considerable* number of days; the mulct for feeling pulses (even through a tobacco-leaf,) being not less than fifteen or twenty days.

Did you go among or near many persons afflicted with the plague, or did you keep personally from them, except among your own soldiers?—I volunteered my services to examine the patients in the lazaretto, some of whom I saw and examined.

Do you recollect any who were ill in the lazarettos?—I rather think I was prevented from seeing the living cases of plague within their wards, and that I examined more particularly the dead.

Do you consider dead cases capable of producing plague?—From

the general opinion which is abroad on this point, I should conceive they are not so liable, if liable at all.

Has the quarantine in Malta been regularly attended to, prior to the breaking-out of the plague?—I have had no opportunities of judging of that; my attention to quarantine-laws was not particularly arrested, till I saw danger in our neighbourhood.

Is it not likely to suppose that cases of plague have frequently arrived in Malta, prior to the breaking-out of the plague?—I understood that it had repeatedly been known in the lazaretto, but was stopped by prompt precaution. I yesterday produced the title-page of a book, in which there was an engraving of a monument to the memory of a grand master in 1743, who had arrested the plague.

Did you remain at Malta after 1813?—I think I left Malta in April or May 1814.

Was there a plague in May 1814, at Malta?—I never heard it recurred in that year.

What was done with the clothes and bedding of the persons who died of the plague?—I understood, generally destroyed; though in some instances not until long after their being infected.

Upon the whole, do you consider the plague to be a disease propagated by contagion, like the small-pox and other eruptive diseases?—I do.

Do you believe it may be so propagated, independent of any influence of the atmosphere?—I believe it may.

Do you consider insulation by the means of quarantine, the most effectual method of preserving against the plague?—By far the most effectual.

Do you know of any cause of communication of plague, besides contagion?—None that will explain its production and dissemination, except a contagion *sui generis*.

Explain your meaning of *sui generis*?—I believe it signifies whatever is quite peculiar to the thing spoken of, consequently the contagion of plague is quite peculiar in producing the plague. Take other examples: the contagion of the small-pox is peculiar in producing small-pox only; the contagion of the measles is peculiar in producing measles only; and so in like manner the contagion of the plague is peculiar to itself, or what is termed specific.

By peculiar, do you only mean that the contagion of the plague will produce plague, and the contagion of the small-pox, small-pox?—I mean that only.

Then you do not allude to the mode of producing the effect?—We know nothing of the *modus operandi* of the contagion; it is quite inscrutable.

Do you wish to state something as to the arrangement for the guards?—I should have mentioned, in my examination on this head, that some part of the police guards were enrolled in the month of July, by the gentlemen placed at the head of the police.

Have you ever heard of the plague in England?—I have.

Mention the year?—I have read of several having visited England,

but my recollection respecting them is not distinct: I recollect most of that plague which occurred in the year 1665.

Do you consider Dr. Sydenham's account of the plague as the best?—I think Dr. Sydenham's account did not result from any very patient or philosophical investigation of the disease.

Does Dr. Sydenham consider it the real Levant plague?—I think not; but it is so long since I read the work, that I cannot charge my memory with any of his observations.

Do you consider it yourself as the real plague of the Levant?—I do, decidedly.

Have you ever heard of plague since that period?—I am not prepared to answer that question. Whether it has visited England since or not, I should apprehend, could only be answered by those engaged about the health-office.

If the expurgators of goods at the quarantine establishment have never received plague from opening the bales from the Levant, even those which come with foul bills of health, should you not from thence conclude that there is no matter of infection contained in the bales?—No; the caution enjoined in the operation of expurgation may be sufficiently great to prevent the reception of contagion of plague, even though it existed: the intensity of the contagion may be so blunted by length of time, and by care, as not to be calculated to excite the disease readily.

Has it not astonished you that, for a period of 154 years, there should have been no occurrence of plague in England?—I cannot say it astonishes me more with respect to England than other places. I find it has paid visits, at the distance of long intervals of time, to other countries as well as England; and we do not know enough of what constitutes aptitude in persons to receive the contagion, in order to know under what circumstances the disease must necessarily be produced.

Do you consider the quarantine establishments in England to have been one of the causes of preventing plague from being introduced?—I should think the quarantine establishment, taken in all its bearings, abroad and at home, has had a great share in preventing the introduction of the disease.

Do you suppose the corn is capable of producing infection, and therefore ought to be the subject of the quarantine laws?—On that point I am not able to speak: I do not believe it is considered an article of high susceptibility.

To what articles should you attribute the greatest probability of plague infection?—I cannot be certain, but I should rather think woollen clothes and cotton. Upon this part of the subject I would rather not answer: I am very doubtful with respect to the degrees of susceptibility of different articles.

Were the arrangements made at Malta respecting the plague in 1813, very expensive to government during the plague?—I should think they must have been a very great expence; but I only speak from surmise, concluding from what I saw.

CRITICAL ANALYSES

OF

RECENT PUBLICATIONS, IN THE DIFFERENT BRANCHES
OF MEDICINE AND SURGERY.

"I would have men know, that, though I reprehend the easie passing over of the causes of things
 "by ascribing them to secret and hidden vertues and properties; (for this hath arrested and laid
 "asleepe all true enquiry and indications;) yet I doe not understand but that, in the practical
 "part of knowledge, much will be left to experience and probation, whereunto indication cannot
 "so fully reach: and this not only in *specie*, but in *individuo*. Yet it was well said, *Per se actus*
 "*cuse per causas scire*."—BACON.

*Transactions of the Association of Fellows and Licentiates of the
 King's and Queen's College of Physicians in Ireland.* Volume III.
 8vo. pp. 505. Cumming, Dublin; and Longman and Co. London.
 1820.

THIS volume comprises several papers of considerable value; but, on the whole, its contents are much less interesting to the generality of medical men than those of either of the two former collections of the Transactions of the Association. A considerable proportion—above three hundred pages—of the present one is occupied by observations on the late Epidemý of Ireland, and Reports of the Fever-Hospitals of Dublin and Cork. So many Reports of this kind have been published by several physicians of eminent and various talents, that, excepting what relates to the views of *medical policy*, but little new matter of remarkable interest could be expected; at least, not from the physicians who had, but a very short time previously, published the results of their experience and reflections. One of these Reports is by Dr. GRATTAN, and has been already noticed in this Journal; another is by Dr. O'BRIEN, whose views of the nature and treatment of fever have also been presented to our readers. Two of them are the productions of physicians—Dr. ROBERT REID and Dr. PICKELS—who now first inscribe their names amongst those of men whose writings will be hereafter referred to, as the sources of information of sterling value respecting the epidemý which has of late contributed so much to the desolation of Ireland. The Report of Dr. Pickels relates to the South Fever-Asylum of Cork, and is chiefly of a statistical character; which is, however, illustrated by corollary observations on the relative value of different modes of treatment in the malady above alluded to. The chief intentions of Dr. Reid, in his paper, are to show how far some of the most important points in the etiology of fever may be explained by the principles which he had already advanced, in an Essay published with his Treatise on Tetanus and Hydrophobia. Although Dr. Reid does not attempt to form a general theory

of fever,—(the effecting of which cannot, rationally, be expected in the present state of physiology,)—and has mingled conjectures with his precise inferences, his notions are worthy of attentive consideration: as they will, however, be noticed in the Proëmium to the next volume of this Journal, we shall not examine them in a particular manner in this place.

Taking the rest of the papers in this volume in the order in which they occur, the first we have to notice is one giving an account of “*a Case of Hæmorrhage, supposed to be from the Spleen, in consequence of Injury done to that Organ.* By WM. HARRISON, M.D. Licentiate of the King’s and Queen’s College of Physicians; late Surgeon of the 36th Foot.”

The patient was a serjeant in the 5th Dragoon Guards, who had for many years been employed as a rough-rider in that regiment, “a tall, thin subject, with considerable muscular strength; aged 50; fair complexion, with rather florid cheeks; had accustomed himself to spirituous and porter drinking.”—“He was riding a horse which he had in training, at the same time leading another by the bridle; the latter started, and dragged him off his seat, when he fell with violence between them. He was then very much hurt at the region of the spleen, and had not been able since to follow his occupation, but suffered much with pain and soreness, with a scalding at the *scrobiculus cordis*.” Ten days after this, “I was frequently called on during the day,” Dr. Harrison says, “but did not see him till seven o’clock in the evening, when I found him in a very helpless state; pulse scarcely perceptible; face and lips pale; voice and lips sunk, and articulation so indistinct as to be unintelligible; had passed a very great quantity of dark-coloured blood, mixed with other fluids, by the mouth and rectum; there were a wash-hand bason and two jugs, each containing above two quarts of this fluid, with a trifling deposit of sediment, resembling the grounds of coffee: when this fluid was made to trickle down the side of the bason, it presented the red arterial colour. I then understood there had been much more passed, which they found necessary to throw away, on account of the fætor, but it contained little feces.” No part of the fluid had coagulated, and there was a small quantity of cream-coloured fluid floating on it in one of the jugs. There was no coughing at the time of its passing by the mouth. “On pressing the abdomen with my hand,” Dr. Harrison says, “he did not complain of pain at any part.” The patient had been seen by a medical man in the course of the day, who had prescribed some liquid medicine which was said to have had “the effect of quieting the stomach.” Dr. Harrison “directed him to continue his medicine; his drink to be quite cold; to

have cold air admitted freely into the room, and alum-why given him for drink."

No recurrence of the sanguineous discharge had happened by the ensuing morning; when, on examination of the abdomen, he complained of pain about the left lumbar region. An oily enema was ordered; but, from the patient fearing that it would produce another flow of blood, the administration was neglected until Dr. Harrison's visit in the evening. "He had three evacuations in consequence of the enema, in large quantity, blended with a small quantity of feces: he now again was very languid. The following day, he said he had rather a comfortable night: I now directed sulphuric acid to be given him in mucilaginous drinks."

On the next day, it was found that he had had two stools, which, on examination, were "very dark-coloured, and exceedingly fetid, diluted largely with the same fluid, and in large quantity."—"On the third evening," the author continues, "I found the heat of body much increased; pulse somewhat hard, and conveying to the finger the catgut feel, with a double stroke, the lesser quickly following the greater, (*pulsus dicrotus* of Solano.) He complained of much pain and tenderness in the splenic region, and the abdomen appeared very tense. I now considered that, if a return of hemorrhage occurred to any amount, he, in all probability, could not survive long; and perhaps I may be considered rash in determining to abstract blood by the arm, which was done to the amount of twelve ounces, *ad deliquium animi*. Cold perspiration supervened, which not a little alarmed me: however, a comfortable, calm sleep continued during that night, and the next day he expressed himself much easier. Since this period nothing very particular occurred, but he gradually arrived to a complete state of convalescence, and is now following his occupation in very good health."

The author remarks respecting this case, that "it appears not easy to determine, with any degree of certainty, whether the blood made its way in such quantity from the spleen, by the vasa brevia, into the stomach; or that an enlargement of the vessels connected to the intestines followed, and that this very large evacuation of blood took place, which afterwards became increased by a flow from the surface of the alimentary canal, holding in suspension feculent matter."

The etiology of cases of this sort is not at all well understood. The most prevalent notion seems to be, that the sanguineous fluid is furnished immediately by the mucous membrane of the intestinal tube; but there are many facts which seem to oppose this notion. In an analogous case related by Mr. COOKE, in one of the Numbers of the Edinburgh Medical

Journal, no adequate disease of the stomach and intestines was perceptible after death ; but the liver and spleen were both in a very morbid state. We have seen two analogous cases (the blood evacuated being florid, not like venous blood, as in what is ordinarily called *melæna*;) occur without any evident cause; or any external injury, in both of which there was unusual tenderness and fulness in the region of the spleen ; and we have witnessed a similar instance, with similar symptoms, which happened five days after a severe concussion of the head. They all differed remarkably from ordinary *melæna*, especially in not being accompanied with the excessive depression of spirits, and dread of imminent death, which so constantly characterize common *melæna*.

The next paper is by Dr. ROBINSON, and gives an account of some cases of small-pox, and two of a modified form of that disease, which occurred at a charity-school at Dublin. The interesting facts of the report are these: A girl, who had not undergone any kind of pox, sickened, on the 16th of July, with what at length became confluent and well-marked small-pox. Another girl in the same school, who slept in the same room or ward, "*who had been vaccinated*," was attacked with fever on the 3d of August, and had what would be termed by Dr. THOMSON *varioloid disease*. Another girl sickened on the 5th of August, and experienced a similar affection. On the 6th of August, another girl, who had undergone neither small-pox nor cow-pox, was attacked with fever, and had small-pox. The rest of the children of the school,—thirty of whom had been vaccinated, and were, for the sake of the experiment, inoculated with matter taken from the last patient with small-pox,—escaped the influence of the infection. If Dr. Robinson should publish more of such cases, we venture to suggest to him that they should be narrated in a more particular manner. It is not satisfactory to be informed that a girl "*had been vaccinated*;" nor is his description of the eruption, in the cases of modified disease, sufficiently minute and perspicuous. In one of the cases, no mention whatever is made of the appearance of the cicatrices left by the pocks.

As the observation of such cases in Dublin seems to be novel, although so common in England and Scotland, we shall give Dr. Robinson's account of them in detail.

"E. Connor, *who had been vaccinated*, on the 3d of August was attacked with fever. 4th. Fever continued. 5th. Eruption appeared. 6th. Febrile symptoms subsiding. 7th. Eruption vesicular, not numerous, distinct, containing lymph ; pulse natural. 8th. Vesicles of a pearl colour, some changing to yellow. 9th. Incrustation commenced. 10th. Pustules rapidly drying on the face; some remain

on the limbs; convalescent, and walking about. 11th. Some pustules still on the feet. 12th. Incrustation extended to all the pustules. 13th. Crusts of a yellow colour; where they have fallen off, the skin appears irregularly raised in the centre of the spot which they had covered.

"M. Montgomery, who had been vaccinated, on the 5th of August was attacked with fever; in the evening an eruption, similar to that on Connor, appeared. 6th. Vesicles forming, not numerous, distinct, and containing lymph; fever continues, but is not oppressive. 7th. Fever more excited. 8th. Vesicles of a pearl colour; fever subsided. 9th. Nearly as yesterday. 10th. As yesterday. 11th. Vesicles changed to yellow; incrustation commenced. 12th. Some pustules remain on the face and arms. 13th. Some of the crusts are yellow, some brownish, shining, and of a horny appearance, about a line in diameter."

After the remarks we have made on this account, it is due from us to Dr. Robinson to observe, that it was published previously to the late discussion of questions which have made more precise descriptions appear so essentially interesting in the history of diseases of this kind.

The ensuing paper gives an account of "*a Case of Idiopathic Emphysema*." By RICHARD STANLEY IRELAND, M.D."

The patient was nine months old. He had suffered severe pneumonia, which was succeeded by a troublesome cough, after a severe fit of which the emphysema appeared, at first above the clavicles, and at length about the whole of the neck and head. The pulse became small, quick, and irregular. "Great distress was apparent in the child's countenance; the face became pale, and, towards the end of life, of a livid colour; and a cold sweat broke out over the body." The child was bled every day by means of leeches, by which measure the distress in breathing was temporarily diminished. Purgatives and glysters were also administered, with apparent benefit. On the fourth day, the difficulty of breathing became extreme, and it was proposed to make punctures in the tumefied parts; but this measure was obstinately opposed by the mother of the patient. Death took place on the ensuing day. Examination of the body was not permitted.

Dr. Ireland refers to some analogous cases; but he does not appear to be acquainted with the observations of Dr. LAENNEC on this affection, which is one of the diseases of the thoracic organs which that physician has the merit of first describing, in its origin, in a satisfactory manner.

"*On Affections of the Cranial Brain, occurring in Infants*," is the title of the next paper. This is the Essay referred to by Dr. WHITLOCK NICHOLL, in his "*Elements of Pathology*," as

a development of views which he had but slightly sketched in the work just mentioned. It is an excellent and highly valuable memoir, and should be perused attentively by every medical practitioner. Since it first occurred to our notice, we have witnessed several cases which have presented proofs of the propriety of the author's views, and of the truth and minute accuracy of his distinctions, as well as of the particular success attendant on the practical precepts they have indicated.

Dr. Nicholl commences his dissertation with some appropriate critical remarks on the disposition which exists amongst medical men,—more extensively and more commonly, perhaps, than most of us are inclined to acknowledge,—to confine their regards to *consequences* of disease, and, with respect to morbid states of the brain of infants, to that which is often only a very remote consequence of what should engage our express attention—*hydrencephalus*. Dr. Nicholl then proceeds to develop his views of certain states of the brain of infants, which are distinguishable by observation of their phenomena during life, and which should, respectively, become the objects of peculiar modes of treatment. He first observes that—

“ There is a state or condition of the cranial brain in infants, which may be called a state of irritation, an irritated state, or, in a word, *erethism*. What this peculiar condition of the cerebral structure is, I cannot explain: It is a state distinct from that which is called inflammation, for it may exist without any perceptible increase of the quantity of blood received by the cerebral blood-vessels. It is a state in which inordinate effects arise in the cerebral structure from ordinary impressions upon different parts of the nervous system. In its perfect form, and under a high degree of it, it is a highly sensitive condition of the cranial brain,—a condition the very reverse of that state under which sleep occurs. Under such a condition of the cerebral substance, the child is wakeful, restless, attentive to every sound and to every object of sight; irritable in temper; the retina is highly sensible to light, the pupil generally more or less contracted; the limbs much in action; the head suddenly moved about, or shaken from side to side; and a degree of animation and a quickness of observation are present, much beyond what is commonly seen in children of the same age: so that, although a morbid condition of the cranial brain be present, the infant may be considered as perfectly healthy, on account of its being lively and sensible to the most trifling impression. By an attentive observer, other symptoms may be noticed: the child starts when asleep; when awake, a sudden frown passes over the forehead, and then disappears; the eyes are sometimes closed irregularly, or alternately; a winking of one eye, or of both eyes, may sometimes be detected; the hand is often carelessly passed over the forehead, or over the side of the head; the child cries without any evident cause, at other times it shrieks; the fists are clenched, the fore-arms bent upwards on the arms. Such a state of the cranial

brain constitutes the simple form of what may be termed *sensitive erethism*.

"There is another form of erethism of the cranial brain in infants, in which there is a great want of animation, the child being dull, yet fretful if roused; the head, perhaps, being suffered to droop, or being reclined on either side; there being an absence of sleep, a state that can scarcely be called waking; an indisposition to move; an indifference towards all objects; a general pallor and chilliness of the body; a dull state of the eyes, a rolling or turning-up of the eyes; a plaintive moan, or occasional shriek; the child awaking from sleep with a note expressive of pain or of displeasure; a wrinkled state of the integuments of the forehead; the hands raised towards the head; the pupil more or less contracted; there being apparently no notice taken of any object of sight; the body and lower extremities being, perhaps, extended; the head thrown backwards. This form of erethism may be distinguished by the term *torpid erethism*."

The term *torpid erethism* is not, we think, well chosen; but that there exists such a peculiar state as the author describes, we are well assured by experience, and we think it is most forcibly characterized by the observation that the child is "dull, yet fretful if roused." The patient will lie in a state of apparent sleep, for hours even, if left in perfect quietude; but the slightest exciting cause is sufficient to produce great agitation and fretfulness: it is often impossible, with the most gentle manners and soothing and patient conduct, to feel the pulse, until after a long period of agitation, when the object is defeated by the great disturbance of the circulation that has been produced. The child will lie as if it were asleep, (excepting that the eye-lids are not quite closed,) but, after the most gentle approach to it, the slightest touch of the fingers to its wrist will rouse it to a state of agitation of the hands, and perhaps of the whole body, that cannot be appeased, whatever may be the age of the infant, by any soothing intreaties. It is but very rarely possible to inspect the tongue, except in a very imperfect manner, as it may be seen when the child cries; nor can it hardly ever be ascertained whether there is tenderness in the belly; but, as far as we have been able to observe, the tongue is ordinarily dry, somewhat smooth, and of a brownish colour.

Previously to a consideration of the evident exciting causes of erethism of the brain of infants, regarded generally, Dr. Nicholl adverts to a state of that organ consisting in some peculiar original structure, which predisposes to it. Sufficiently close and deep investigation must,—as is shown in a modern work, in which this point is first discussed in a thorough and determined manner,—indeed, lead us to this unknown term of causation in most diseases.

The exciting causes of this erethism enumerated by Dr. Nicholl, are most of the common exciting causes of disease; but especially undue impressions on the expansions (or, as Dr. Nicholl terms them, the anti-cerebral extremities,) of the nerves spread out in the substance of the liver and the alimentary tubes; and an increase of the quantity of blood circulating through the vessels of the brain.

"As, on the one hand," he says, "we find that an increase of the quantity of blood that circulates through the cerebral blood-vessels tends to the production of erethism of the cranial brain, so do we find that erethism of the cranial brain may cause the cerebral arteries to receive more than their due share of blood. So that, after erethism of the cranial brain has existed for an uncertain time, an increase of the quantity of blood that circulates through the cerebral blood-vessels may be superadded to the erethismal state. The combination of a great increase of the quantity of blood that circulates through the cerebral blood-vessels with erethism of the cranial brain, constitutes that state which is called inflammation.

"Inflammation of the cranial brain in infants is, then, a state which includes in itself erethism of that substance; but erethism of the cranial brain in infants may exist independently of any perceptible increase of the quantity of blood that circulates through the cerebral blood-vessels. When erethism of the cranial brain exists in combination with a great increase of the blood that circulates through the cerebral blood-vessels, such erethism may either precede or be superadded to that increase.

"A highly sensitive form of erethism of the cranial brain in infants, coupled with a great increase of the quantity of blood that circulates through the cerebral blood-vessels, constitutes what is called active, or acute, inflammation of the cerebral substance. A less sensitive form of erethism of that substance, combined with a more moderate increase of the quantity of that blood, constitutes, probably, what had been termed a sub-acute form of inflammation of the cranial brain; and a moderate increase of the quantity of that blood, joined to the more torpid form of erethism, may, possibly, constitute the chronic form of cerebral inflammation."

After a description of the symptoms of inflammation of the brain, Dr. Nicholl speaks of *simple plethora* of the cerebral blood-vessels of the brain of infants; the causes of which—as general increase of the quantity of the blood; impediment to its return from the head; impediment to its distribution to other parts of the body; diminished tone, and consequently diminished resistance to its entrance in them, of the blood-vessels of the brain; increased temperature of the head, and "certain impressions on the anti-cerebral extremities of nerves, as on those in the liver, in the alimentary canal, in the gums,"—are passed in review. That fullness which depends on simple ob-

struction to the course of the blood from the veins, he terms "congestion in the cranial brain." He then adds that—

"The quantity of fluid poured out by exhalants in different parts of the body, is very much influenced by the state of the nerves in the vicinity of such exhalants. Whatever produces such a state of those nerves as is expressed, rather than explained, by the term *irritation*, disposes the exhalants in their neighbourhood to pour out an extra quantity of fluid. In like manner, we find that erethism of the cranial brain disposes the cerebral exhalants to pour out an increased quantity of fluid into the cavities of that substance.

"In some cases, when an impression on distant anti-cerebral extremities of nerves, (such as those which are spread out in the liver, or throughout the alimentary canal,) produces such an effect at the cerebral termination of those nerves,—i. e. on the cranial brain,—as causes the cerebral exhalants to pour out an increased quantity of fluid, we find that there are scarcely any symptoms present which denote the existence of erethism of the cranial brain; the symptoms which arise being chiefly, if not entirely, such as may be attributed to the presence of a collection of fluid in the ventricles of the cerebral structure. And, in some of these cases, the increase of the quantity effused by the cerebral exhalants is so moderate, that a considerable length of time may elapse ere such a collection is formed as may give rise to marked and decided symptoms. In such cases, it is very probable that a collection of fluid in the ventricles may take place before any suspicion is entertained of diseased action going on in the cranial brain, or of the existence of any source of irritation in any of the contents of the abdomen."*

After some further discussion of the means of accumulation of serous fluids in the brain, Dr. Nicholl observes, in a summary way, that—

"In inflammation of the cranial brain in infants, there exists a two-fold cause of an undue flow of fluid from the cerebral exhalants. In the first place, such a state includes erethism, which, as I have already stated, disposes the cerebral exhalants to pour out an increased quantity of fluid. In the second place, the cerebral arteries, during inflammation of the cranial brain, receive an increased quantity of fluid, and, consequently, an increased quantity must pass by the terminations of those arteries, of which terminations the cerebral exhalants form a part.

"A faulty state of the exhalants of the cranial brain may dispose them to give passage to a preternatural quantity of fluid.

"As the fluid usually poured out by the cerebral exhalants is, under a due and healthy action of the cranial brain and of its vessels, removed in proportion as it is effused, by which removal an accumulation of that fluid is prevented, it follows that, if the vessels whose office it is to remove that fluid perform that office imperfectly, an accumulation

* In cases of this kind, instead of a contracted pupil, we may find the pupil of a natural size, or it may ultimately be dilated.

of fluid in the cavities of the cranial brain will take place, although the exhalants pour out only a due and natural quantity."

The next object of the author is to distinguish the states of simple plethra and congestion.

"In simple plethra of the cerebral blood-vessels, there is increased heat of the head; a full eye; a redness of the countenance, together with a want of animation; a heavy, listless state; indisposition to move the head; uneasy sensation of fullness in the head, causing the child to seek support for the head; giddiness; sometimes shaking of the head; uneasy respiration; the pupil of a natural size, or perhaps rather dilated; sickness; loss of appetite; heat and dryness of the mouth and skin; an inactive state of the bowels; a sensibility to impressions, but a heedlessness of them; a turgid state of the vessels of the head; the pulse not much accelerated, full, perhaps oppressed.

"In congestion in the cranial brain, there is a general coldness of the body; the child throws the head backward, or leans it on either side; the child lies in a comatose state; the eyes prominent and fixed, the pupils fully dilated, not contracting on exposure to strong light; stiffness of the body, and of the limbs; irregular actions of the muscles of the face and eyes; a more or less entire absence of vision; a more or less complete state of insensibility to all impressions; torpid state of the bowels; scanty or suppressed secretion of urine; pulse, for the most part, slow, and oppressed."

Nearly similar (too nearly alike to be distinguished in many cases,) to the symptoms of congestion, are those of undue accumulation of fluid in the brain.

The author concludes his observations on the points above noticed, with stating that "erethism, plethra, inflammation, and congestion, may severally so affect the cranial brain as to bring on death without having previously produced effusion into the ventricles; or these states may severally subside without giving rise to such effusion; or these states may severally continue, effusion being also present; or these states may cease, and effusion may remain as a consequence."

Besides the conditions above designated, Dr. Nicholl says "there is a state of the cranial brain which may be termed torpor, or insensibility; a state characterized by a general insensibility to impressions of every kind, and by all those other symptoms which have been enumerated as the consequences of congestion in the cranial brain, or of effusion into the ventricles." And, he adds,

"Torpor of the cranial brain may arise from whatever causes that substance to be unduly compressed between its unyielding parietes externally, and its own blood-vessels internally. Thus it arises from a plethoric state of the cerebral blood-vessels; from congestion in the cranial brain; from a collection of fluid in the ventricles,* (arising

* During a state of torpor of the cranial brain, do those vessels whose office it
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from any of the causes already mentioned, whether erethism, simple plethora, inflammation, congestion, or a faulty state of the cerebral exhalants, or of the cerebral absorbents;) from effusion of blood; from collections of pus; from tumors; from thickening or indentation of the cranium, and the like. It may arise from concussion of the cranial brain; it may be the consequence of a preceding high degree of the erethismal state of the cranial brain; it may arise from some peculiar alteration of the cerebral structure, or from loss of a part of the cerebral substance; or it may be connected with, and dependant upon, original formation. It may arise from a loaded state of the alimentary canal, or from some morbid condition of the passage, especially of the internal membrane of the canal; from worms in that canal; from disease of the mesenteric glands; from congestion of bile or of blood in the liver, or from some other faulty state of that viscus. It may arise from loss of blood; from extreme cold; from diminished action of the heart; from disordered action of the lungs; from vitiated atmosphere; from respiring carbonic acid gas; from intoxication; from sedatives, externally applied or inwardly administered; from poisonous substances, &c."

The different states of the brain of infants, as considered by Dr. Nicholl, may then be arranged under the heads of Erethism, (sensitive and torpid;) Simple Plethora; Inflammation; Congestion; Collection of Fluid in the Ventricles; and Torpor.

"I think that I am correct," Dr. Nicholl says, "in stating that each of these conditions of the cranial brain may exist as a distinct affection; and that, in the pure, uncombined form of each of these states, each condition has distinguishing characters by which its presence is denoted. But, when we review these various conditions, and consider how they may be blended; when we look over the list of causes from which they may severally arise, and see how many of the causes of each condition may exist at the same moment; and when we take into the account the influence which the various modes of treatment have upon disease, how much they alter its features, and how much they modify and interrupt the train of its symptoms; we shall be prepared to meet with appearances very different from those which are traced upon paper, and we shall not be surprised at being called upon to treat cases in which there is an assemblage of symptoms that baffle all our attempts at classification, and that defy all nosological distinctions."

Several of the diagnostic phenomena of those conditions are, in the next place, discussed in a particular manner: but we cannot follow the author on this occasion; and we consider, too, that the principal inferences have been already indicated in our transcriptions.

is to remove the fluid usually effused by the cerebral exhalants, ever partake so far of the general torpor as to perform that office imperfectly, or to cease entirely to perform it? If they do, torpor of the cerebral substance may cause a collection of fluid in the ventricles, although the cerebral exhalants pour out only a natural and due quantity of fluid.

Having discussed in this manner the subjects of his dissertation, the author takes a somewhat more general view of their relations in the economy.

"Whatever," he says, "may be the form under which a diseased condition of the cranial brain in infants manifests itself, certain facts must be borne in mind, namely,—That, although the cranial brain appears to be the seat of disease, yet that the primary cause of the morbid condition of that substance may exist in some part far distant from the cranial brain; as, for instance, in the neighbourhood of some anti-cerebral extremities of nerves, which are spread out in the liver or in the alimentary canal; and that, as long as such cause remains and continues to operate, it is absurd to expect to get rid of the affection of the cranial brain. Secondly, that, although such cause be removed, or have ceased to operate, yet, if it have existed for a considerable length of time or have been very powerful, it may have induced a state of the cranial brain, which may remain after such cause is removed. Thirdly, that either of the states of the cranial brain which I have mentioned may arise from various, and from very different, causes. Fourthly, that the different states require different modes of treatment. And lastly, that either of these states may induce, or may pass into, a different state: thus, simple plethora may induce erethism; or may be induced by this last state, and, in either case, inflammation will be present; or plethora may bring on torpor; torpor may succeed to erethism, to plethora, to inflammation, to congestion;* sensitive erethism may pass into torpid erethism; in inflammation, the plethoric state of the cerebral blood-vessels may subside, and erethism may alone remain."

The most common of these affections, is that which Dr. Nicholl has endeavoured to express by the term *erethism*; the other conditions being, in many cases, consequences of that state. The predisposition to this state is often manifest for a long time before any disease is supposed, by the parents of the patient at least, to exist. The most common attendants of it are, a precocity of intellect,—as we remarked in our review of Dr. Cheyne's Essay on Hydrancephalus;—a general appearance of the child ordinarily intended to be expressed by the term *scrofulous*; "either a highly sensitive state of the retina, with a contracted pupil, or a general languor of expression, with dilated pupil;" great irritability of temper; and an extraordinary degree of liveliness and animation.

The influence of improper diet, as an exciting cause, is somewhat extensively discussed by the author. When noticing the agency of disorder in the alimentary canal on the brain, he remarks, "we must not forget that a morbid condition of the cranial brain influences the secreting action of the liver, either increasing that action, or altering it, or lessening it: we must,

* May not torpor of the cranial brain induce congestion in that substance?

therefore, be very cautious to distinguish between cause and effect in these cases; or else it may happen that, while we are endeavouring to improve the action of the liver, in order that we may remedy the condition of the cranial brain, we ought to be endeavouring to improve the condition of the latter, in order that we may produce improved action of the liver. The converse of this supposed case may also occur. The same observations are applicable to imperfect or faulty action of the alimentary canal, as connected with the state of the cranial brain."

We are convinced that the morbid condition of the alimentary canal is much more frequently a consequence of the disorder of the brain, than some late writers on this subject would have us believe: the black and otherwise disordered feces very frequently do not appear until the cerebral affection has evidently existed for some time. This is particularly remarkable in cases originating from blows on the head, which is a frequent origin of the disease amongst poor children, who are so commonly nursed by very young girls, and are thus exposed to such accidents. Dr. HARRISON has mentioned to us several interesting facts related to this point, which he has witnessed in his practice in the treatment of spinal diseases. The dark colour and otherwise disordered state of the feces, (which very commonly present a remarkable odour, that he says was pretty well designated by the mother of one of his patients, when she compared it to that of *the washings of a gun*,) which so commonly accompany a certain extent of spinal deformity, ordinarily disappears almost immediately, (without the use of medicine,) often on the next day, after a certain degree of removal of the deformity, even when this has been effected suddenly,—that is to say, in an hour or so. The ammoniacal state of the urine, which is sometimes, also, a symptom, disappears with equal rapidity.

The first interesting remarks by Dr. Nicholl on the treatment of the affections under consideration, are the following:

"In children of a very languid habit, conjoined with that state which shews a tendency to erethism, we must be cautious how we administer calomel too frequently, with a view to excite due action of the liver. Perhaps calomel is, in many cases, given in repeated doses for a considerable length of time, for the express purpose of removing an appearance of the stools which is solely kept up by the calomel itself: the appearance to which I particularly allude, is the green hue, resembling that of stewed spinach,—an appearance which is frequently represented as being characteristic of such a condition of the cranial brain as terminates in hydrencephalus. Calomel is given to promote secretion of bile; it is also given where that secretion is too abundant: the evil is, that it is given in a similar manner as a remedy in these opposite states. There is no absurdity, no inconsistency, in exhibiting

it as a remedy in both these states, if we adopt the mode of giving it to the particular state. For instance, if the object be to promote the secretion of bile, we should give small doses in repeated succession, so as to produce the specific effects of mercury on the liver: if we want to remove from the alimentary canal an inordinate quantity of bile which has flowed into it, we should give one powerful dose of calomel, so as to produce its effects as a strong purgative on the intestines. But, in very languid habits,—in habits which show a tendency to serofula, to erethism of the cranial brain,—if we want to promote a more copious secretion of bile, it will be more prudent to endeavour to attain that object by employing other remedies,—such as nitro-muriatic acid, internally and externally, or small doses of sulphat of potash, with extract of taraxacum, with a little aloetic wine; or, if we find it necessary to employ mercury to promote the action of the liver, the mercurial pill, the grey oxyde of mercury, the hydrargyrus cum creta, are less objectionable forms of that mineral. These several means may be assisted by warm salt-baths, by friction with salt, and by clothing the child in flannel.”

We are, perhaps, more averse to the continued use of calomel, or mercury in any form, than Dr. Nicholl, in the kind of patients he describes: it is, however, very fortunately, often found that one large dose of calomel will produce a due appearance of bile in the feces, without giving rise to the ill consequences which result from establishing the action of the mineral in the system generally. It is true that the green colour of the stools, with the absence of yellow bile from them, will frequently return after a few days, if other and effectual means be not employed to preserve the action of the liver; but such measures may often be found in less objectionable medicines than mercury. We always, at least, try the practice just designated; and, when we resort to a continued course of mercury, in case of the failure of that practice, it is with the suspicious caution with which a man follows a villainous guide, at night, in a strange country.

When erethism of the brain exists in a high degree; whatever may be its cause, it is necessary to use means for the immediate alleviation of it; for which purpose, Dr. Nicholl says he knows no remedy more appropriate than the pulvis ipecacuanhæ compositus. This medicine had already been forcibly recommended for concussion of the brain, and in the early stage of, as it has been called, “*hydreencephalus* :” but we think much advantage will be derived from Dr. Nicholl’s more precise indications of the states in which it is expressly proper, which are founded on the important distinction between erethism and inflammation. With this opiate, antimonial powder, especially in what Dr. Nicholl calls *sub-acute inflammation*, cold lotions to the head, quietude, nitre and acetate of potash, mild glysters, and a “*tépid bath of moderate temperature*,” may be

conjoined. "In children of a full habit," Dr. Nicholls adds, "if the symptoms of erethism of the cranial brain run high, leeches may be applied to the temples. But, in that form of affection which I have endeavoured to describe under the term *torpid erethism*, where there is great general pallor and coldness, insensibility, and contracted pupil, the abstraction of blood will be a dangerous experiment, and may hurry our little patient out of the world. In such cases, we must endeavour to combat the affection by the means already pointed out; and, in addition to these, the head may be blistered."

In all cases of erethism, Dr. Nicholl takes care to observe, "we must ever be on the watch for the supervention of a plethoric state of the cerebral blood-vessels, which, combined with the erethismal state, constitutes inflammation." If this occur, and is removed by the proper measures, an erethismal state may yet remain; when "it will be proper," Dr. Nicholl adds, "to give the pulvis ipecac. comp. in liberal doses, combined with James's powder and nitre, with small doses also of calomel, or of the hydrargyrus cum creta. For, if the erethismal state of the brain be not allayed, the child will continue restless, wakeful, and irritable; and we may expect that the plethoric state of the cerebral blood-vessels will, sooner or later, return, and that, in this way, the child may at length be worn out."

A case is related by Dr. Nicholl, in a subsequent part of this volume of Transactions, that exemplifies very forcibly the most important of his general observations respecting erethism of the brain of infants; and, as we have cited so extensively from his dissertation on this subject, or, in other words, have pretended to give a complete abstract of it, we consider ourselves obliged to cite also so striking an illustration of his views. The account of the case will not admit of abridgment, and is as follows:

"Mr. Acton, a very intelligent surgeon of this town, (Ludlow,) has an infant daughter, who is between eight and nine weeks old; she was, from her birth, lively, very wakeful, scarcely ever sleeping during the day; highly sensible to impressions: when she was scarcely six weeks old, she awoke as with a hesitation of breathing, and the muscles of the face were convulsed. She became still more restless, and was very fretful. Nothing amiss had ever been noticed in the character of her stools. She was suckled by her mother, a very healthy young woman. Her father gave her a dose of calomel, and put her into a warm bath: the stool which succeeded to the exhibition of the mercurial purgative, was perfectly healthy. After this I saw the child: it started when the door was opened, or when a chair was hastily moved, or when any one coughed, or if any part of its body

was touched. It cried very much and very loudly, and was only appeased, and that momentarily, by being placed in a sitting posture, by being carried about, or by being put to the breast. The pupils were of a natural size; there was no vomiting; no heat of skin, no heat of the head, no flushings of the cheeks, no increased throbbings of the arteries of the neck and head. When this highly sensitive and wakeful state had continued for several hours, the child became gradually more heedless of noises, until at length it ceased to notice them; the crying then subsided, and the child bore a horizontal position. In this state, the eye appeared as if insensible to the light of a candle, the pupil, which was rather enlarged, vibrating, as it were, between contraction and dilatation when strong light was thrown on the eye; the fore-arm bent on the arm; the fingers clenched; the thumb laid flat across the palm; the upper extremities, in this state, raised, in constant motion; the head sometimes moved about, but not much so; the lower extremities sometimes suddenly drawn up; the lips moving; no moaning; occasional rolling of the eyes; the eyes fully open; not a moment in which some muscles were not in quick action; the body bent backwards. When this state had continued for four or five hours, sleep came on, out of which the child awoke, and appearing in its usual state; its arms pliant; its hands open. Then came on the fretful, crying, restless state; then the torpid restless state, during which the muscles were in constant action; the fore-arm bent; the fingers clenched as before; then sleep; after which, apparent recovery. And thus did the sensitive erethismal state, followed by torpid erethism, by sleep, by recovery again, repeatedly run its course. The brain, after the highly sensitive state had been long kept up, gradually assuming a state approaching more and more to torpor, untill its actions were at rest, and then was sleep present; but, after a short rest, the brain *awoke* to its original state. It was remarked that, when the sensitive state of the brain recurred, the bowels were relaxed, notwithstanding the use of opium; the eyes were suffused; the child sneezed, and had an increased quantity of moisture in the nostrils and of saliva from the mouth: when the sensitive state declined, the bowels were no longer relaxed; the coryza disappeared, secretion having been increased by the erethismal state. At one period, during the torpid erethismal state, there was complete opisthotonos to a great extent, so that the spinal brain was affected also with the erethismal condition.

“The head, first of all, was blistered: during the state of opisthotonos, the whole of the spine was blistered. The application of the blister to the spine appeared to give much relief, especially by its first operation; afterwards it was thought to irritate too much. A grain and a half of Dover’s powder, was the remedy always resorted to: if given during the highly sensitive state, it allayed the irritation; and, when given during the more torpid state, sleep gradually came on. In one instance, the fretful and the sensitive state, and the more torpid state, occupied two nights and the intervening day, during the whole of which there was scarcely any sleep,—none for a longer period than a few minutes; then sleep came on, which lasted several hours. The

Dover's powder generally quieted the child in three or four hours; a tea-spoon full of syrup of poppies had no effect at any time. Musk had no good effect. The muscular actions generally came on at night. I gave decoction of bark in one of the intervals, a tea-spoon full every hour: I thought that this, combined with the p. ipecac. c., had a slight good effect; but it was not followed up, as Mr. A. thought that the child was in pain after taking it. James's powder made it sick. After the child had continued about a fortnight in this state, the train of symptoms being repeated every day, or every two days, it has continued for the last fortnight without any marked symptom of disease, being better than it has been since its birth; yet there is still an absence of sleep during the day; so that I suspect that there exists some congenital formation of the cerebral structure, which is incompatible with the long duration of health, and perhaps with that of life. The case, as yet, has been a well-marked one of pure erethism, unmixed with the slightest perceptible alteration in the state of the blood-vessels, and alternating with a more torpid state, which is the consequence of the previous highly sensitive state."

The paper we have to notice in the next place gives the history of a "*Case of Amputation at the Hip-joint, for the removal of an Osteo-sarcomatous Tumor.*" By RICHARD CARMICHAEL, Esq. surgeon of the Richmond Hospital, House of Industry, &c. &c."

The subject of this operation was a "delicate-looking girl, about nineteen years of age, of a fair, but now sallow, complexion." The tumor was of the species termed *fungous exostosis* by Mr. ASTLEY COOPER, and extended "from just below the knee, as far up the thigh as barely to leave room for the formation of the flaps; and the circumference of the limb, where it was most prominent, measured twenty-seven inches." The disease had been of twelve months' duration. The operation appears to have been performed with great dexterity and rapidity, and the patient appeared to be doing well until the fourth day after it, when she became restless and uneasy, and soon manifested signs of the existence of great constitutional irritation, which increased until the sixth day, when she died. The femoral artery was not tied, as a first step of the operation: the artery was commanded in the groin, by an assistant. "The entire line of the wound," of the stump, had united by the third day. Some tension was, however, now experienced in the part: the ligatures were removed on the next day, when the stump "appeared full and distended," and "it was suspected that an accumulation of serum had occurred;" but the patient would not permit a probe or director to be passed through the new-formed adhesions, that it might be ascertained if such were the case. On the fourth day, the stump was "greatly swoln, as if distended by some fluid; but, on intro-

ducing a director through the new-formed cicatrix, there did not come any more than half an ounce of thin ichor. Cold applications had been applied, and were continued. About half an ounce more of ichor came away on the next day. The following is the account of the appearances of the stump after death:

“ Upon examining the stump, which was done by separating the flaps that had united through their entire extent externally, a large spongy mass, not unlike a piece of unhealthy lung, presented itself. On cutting into it, we ascertained that it was the divided muscles which were thus strangely altered in so very short a space of time; for during the operation they presented a healthy appearance. It was this swelling of the muscles which caused the tumefaction of the stump already mentioned; for scarcely any fluid came away previous to death, on the introduction of the probe or director.

“ The little ichorous matter which did pass off, was ascertained to come from the upper part of the wound, where there was a considerable cavity, the bottom of which was formed by the acetabulum.”

No morbid appearances were observed in any of the viscera.

An account of a case of *Cynanche Laryngea*, in which tracheotomy was performed, that presents several interesting circumstances, is also furnished by Mr. Carmichael. The peculiar advantages derived from removing a portion of one or two of the rings of the trachea, as recommended by Mr. LAWRENCE, were particularly manifest in this case. An attempt was once made to keep a canula in the wound, but “ the excessive irritation which it occasioned, rendered its immediate removal necessary.” The edges of the external wound were kept separate by two retractors covered by adhesive plaster, which were connected to each other by two strings tied behind the neck. It was deemed necessary, however, to enlarge the opening on the evening of the second day, from the great quantity of viscid mucus which accumulated in the trachea obstructing the original one. The patient recovered. The wound had healed on the fifteenth day.

Another case in which tracheotomy was performed by Mr. Carmichael, gives this surgeon an opportunity for adducing several hints, which he thinks of some import to practitioners. He was called, late at night, to perform tracheotomy on a woman who was in a state approaching suffocation from an inflammatory affection of her throat, which she had complained of for more than a month previously. It had been attended with great difficulty in swallowing as well as in breathing, and had been treated by local and general blood-letting, vesicatories, and other measures, under the direction of Dr. MILLS.

On examining the fauces, Mr. Carmichael could not perceive any swelling of the tonsils, or of the posterior part of the pharynx. "The entire front of the neck was, however, so much swoln, that it was impossible to feel the trachea; and even the larynx itself was scarcely distinguishable." Considerable hemorrhage, and various other means of difficulty, in the thickness of the parts over the trachea, attended the operation; and, from the blood getting into that canal, Mr. Carmichael did not attempt to enlarge the wound (a simple incision) in the trachea, or to introduce a canula, for fear of renewing the hemorrhage. She was left "totally free from convulsive cough or stridulous breathing; some air passing through the wound, but the greater part by the glottis." She continued during the next day in a tranquil state; but in the evening the wound had nearly closed, and little advantage was obtained by endeavours to enlarge it by means of a probe. No air passed by the wound on the next morning, and the difficulty of breathing was extremely great. An attempt was made to enlarge the opening in the trachea, "when a smart hemorrhage occurred from an artery in the upper part of the wound, which was with some difficulty secured."

Towards the evening, the swelling had subsided, and Mr. Carmichael then was able to introduce a canula without any further assistance from the knife. The canula, after a few minutes, produced but little uneasiness. But the patient on the next day became totally unable to swallow, and a gum-elastic tube excited such irritation and convulsive efforts, as soon as it reached the seat of the obstruction in the œsophagus, that the attempt to convey nutriment into the stomach by means of it was necessarily relinquished. The patient suffered acute pain from hunger, and, as she could not speak, stated by writing that she was starving. Glysters of broth, milk, and opium, were administered. She sunk rapidly, and died on the next morning, (the fifth day from that of the operation,) almost immediately after having vomited "a quantity of purulent matter."

On examining the body, "an abscess containing about six ounces of purulent matter was discovered, extending from the second or third cervical vertebræ as low as the sixth or seventh, situated between the bodies of the vertebræ and the posterior boundary of the œsophagus; the walls of the abscess were firm and unyielding. The matter which she vomited just previous to death, as well as some which was found in the trachea, must have escaped through a small opening in the most prominent part of the abscess, opposite the upper portion of the larynx, as there was no other source ascertained from which it could have been derived."—"The vertebræ, larynx, pharynx, and œsophagus, were of a natural appearance; as were also the lungs,

except that there were extensive old adhesions between the pleura costalis and pulmonalis on both sides of the chest!"

"The practical lesson we derive from this case," Mr. Carmichael says, "is of the highest importance; for, if I could have been aware of the existence of the abscess, a simple puncture into it would, in all probability, have saved the patient's life."

"It is true," Mr. Carmichael adds, "that abscesses are every day met with on the back of the pharynx, obvious to the sight and touch; for these the remedy is apparent, and affords immediate relief. But my object is to draw the attention of the profession to those abscesses, which, as occurred in the cases just detailed, are situated so low as not to be visible, and are, in many instances, beyond the reach of the finger. Such cases may be indicated by the precise seat of the pain; the obstruction, in the first instance, to the passage of aliment; afterwards, as the swelling increases, to the passage of air; the slower progress of the symptoms compared to those of laryngitis; and the more rapid, when compared to those of stricture of the œsophagus, or of chronic tumors pressing upon that canal;* the obstruction to the passage of an instrument down the œsophagus, while those complaints about the fauces which might occasion obstruction, are absent; a general swelling of the anterior part of the neck almost approaching œdema, analogous to what occurs on the surface over any deep-seated abscess; and, possibly, the accession of irregular shiverings, although it was not remarked that this symptom occurred in either of the cases detailed.

"The existence of these, or one or more of these symptoms, should induce an examination with the finger; or, if the finger cannot reach the obstruction, a gum-elastic bougie or sound should be passed, and the obstruction will point out the situation of the abscess. If we are satisfied on this head, a curved trocar, somewhat similar to that recommended by Sir E. Home for puncturing the bladder through the rectum, may then be passed, the distance at which the sound met the obstruction being previously marked upon it: and here the stilet, being made to protrude, may be boldly plunged into the tumor, taking care to push it towards the central line of the bodies of the vertebrae, where no danger can arise from the puncture. If matter flows, we will save the life of the patient; but, if we are disappointed, no material injury, that I can conceive, will be the consequence of the attempt."

"If the practitioner is timorous about passing a trocar down the œsophagus, a silver catheter forced into the abscess may answer equally well, and in most hands may be found the safer instrument."

* These tumors are by no means unfrequent, numerous instances of which may be found in the writings of the older medical authors. Lieutaud alone details four cases of firm tumors impeding deglutition by pressing on the œsophagus.—Vide Lib. iv. Obs. 93, 95, 98, and 99.

A paper containing an account of "*a Case of Gangrene occasioned by the use of Mercury*," by Dr. GRATTAN, occurs next in order. This paper is a particularly interesting one, for several reasons: the case itself is highly so, and it is gratifying to contemplate the sentiments which prompted the publication of it. Dr. Grattan accompanies the narration by some good and appropriate considerations on the uncertainty of the agency of our remedies, and the danger we encounter of occasionally, either witnessing unfortunate results from them, when energetic, or of meriting censure for failing to employ them with due vigour in cases where, had they been thus employed, success would have crowned our efforts.

A girl, ten years of age, who had complained for some weeks previously without exhibiting any evident signs of fever, at length experienced this affection in a decided manner, and was removed to a fever-hospital.

"The child, when visited, obviously laboured under febrile excitement, and appeared to be in the last stage of disease. The head seemed to suffer most; and, from her general appearance, I was led to conclude that water either had collected in the brain, or was on the point of being effused into the ventricles. Delirium, with frequent screaming, alternated with occasional intervals of stupor or heaviness, black dry tongue, pulse seldom under 130, beating of the temples, face sometimes pale and sometimes flushed, were the most prominent symptoms. A two-grain pill of calomel was given, succeeded by a draught of the oleum ricini. Leeches were applied to the temples, and the head was shaved. The symptoms were not improved. The temporal artery was then opened, five ounces of blood were taken, and a blister applied to the occiput and nape. A pill, consisting of two grains each of calomel and ipecacuanha, was given twice, and sometimes thrice, a-day, with the exception of those days on which oil-draughts were administered; so that, by the end of the sixth day after admission, ten pills had been taken. The gums now became sore, and every alarming symptom disappeared. Copious ptyalism succeeded, accompanied with great tumefaction of the face and lips. This state had continued for four or five days, when a small vesicle was observed near the left angle of the mouth, which in a short time assumed a black colour, and in a few hours increased to the size of a sixpence. It was unaccompanied by the slightest perceptible inflammation, and occasioned no pain whatever. By the following day it was as large as a half-crown piece, and in appearance exactly resembled a superficial sore that had been dusted over with finely powdered charcoal. The salivation continued, attended with great fetor. The fermenting poultice was applied; bark and opium were administered internally. Wine was given; the sore was also occasionally washed with tincture of opium, and sprinkled with the powdered bark, but without effect. The gangrene extended itself until a part of the cheek, about two inches in breadth, was eaten away. The right angle of the

mouth was now affected, the disease commencing precisely as already described. The respiration became laborious and oppressed; and death at last took place on the nineteenth day after admission, being the thirteenth day from that on which calomel had been last prescribed, and the eighth from the time when the gangrene first shewed itself.

“ Whether the above was from its commencement essentially a case of hydrocephalus internus, I am by no means prepared to decide. Whether the accompanying fever was symptomatic of the primary hydrocephalic affection, or whether a low irregular fever, neglected at the commencement, and acting on an organ preternaturally disposed to derangement, might not have occasioned in the brain that particular morbid condition which the symptoms so strongly indicated, it is by no means easy to determine. From the malignant character which the fever assumed, and the marked typhoid symptoms which attended it, I am inclined to adopt the latter supposition. However, in either case, the brain being the organ affected, and under a state more or less of inflammatory excitement, the leading indication evidently was to allay that excitement, and to correct any mischief which it might have occasioned by its continuance. With this view the local abstraction of blood was resorted to; and, in order more effectually to accomplish the same object, mercury was prescribed so as to affect the system. In the course of six days twenty grains of calomel were taken; the mouth became affected, and immediately the patient appeared free from disease. So far every thing proceeded more favourably than, under such unpromising circumstances, could have been expected, and so far the propriety of the course adopted seems to be confirmed by the result. But, an unexpected train of symptoms succeeding, and terminating in a manner so unfavourable, we are naturally led to inquire into their causes. It becomes a matter of much importance to determine, whether they depended on the previous treatment, or whether they should rather be ascribed to a different origin. In the present instance, I think that the mercury, though it chiefly contributed to the cure of the primary disease, yet, acting on a system of extreme delicacy, was nevertheless the cause of the subsequent untoward event. It would appear, from the violent effects of the medicine, that there existed in this instance an unusual susceptibility of constitution, by which its ordinary influence was modified and rendered noxious; for much larger quantities of mercury are frequently exhibited to younger patients without producing salivation. Could this peculiarity have been foreseen, it is evident that the medicine should either, in the first instance, have been entirely withheld, or administered with greater caution. On the other hand, were we to anticipate in every other case a similar result, our practice would become so vacillating and timid, that a great proportion of our patients might die through our hesitation in not employing an active remedy until we had first satisfied ourselves, by cautious experiment, that it was not likely to prove injurious. A few exceptions ought not to preponderate against the great majority of well-established cases, in which a particular treatment has been safely and beneficially adopted. We do not consider

the remote possibility of aneurism, or of secondary hemorrhage from the temporal artery, sufficient to contra-indicate the employment of arteriotomy in a case of violent phrenitis. Thus, also, were I again to treat a patient similarly affected, I should not hesitate to have recourse to mercury, as the medicine on which, after blood-letting, I ought chiefly to rely."

In addition to the foregoing remarks, Dr. Grattan observes that a gangrenous affection, similar to that described, sometimes occurs when it cannot be attributed to mercury. Such an affection in children, under other conditions of the system than fever, has been described by several writers, and appears to have some analogy, in respect to the subjects of it and the course it takes, with the peculiar gangrenous affection of the pudenda which is occasionally witnessed in young girls. We recollect having seen two girls, near Macroom, in the county of Cork, whose faces—that is to say, the lips and cheeks,—were nearly destroyed by, according to the father's account, a gangrenous ulceration which occurred in the course of typhous fever, and which affected also the mother, who died of the disease. These persons had no medical assistance. But, whether or not the mercury had any influence in the production of the gangrene in Dr. Grattan's case, this seems to be certain, that his practice cannot justly be censured; for, as he remarks, we cannot "ascertain *a priori* the existence of such susceptibility with respect to the effects of particular substances;" and, still using his own expressions, "were we to anticipate in every other case a similar result, our practice would become so vacillating and timid, that a great proportion of our patients might die through our hesitation in employing an active remedy until we had first satisfied ourselves, by cautious experiment, that it was not likely to prove injurious."

"*A well-marked Case of Liver-dough, with some Cases and Observations tending to show how frequently the Lungs and other Viscera sympathize with Derangements in the Liver, whether organic or functional,*" by WILLIAM BROOKE, M.D. M.R.I.A. &c., is the title of the next paper.

The case expressly designated above exemplifies, in a very striking manner, the proposition of the author of this paper, besides presenting several other very interesting circumstances. We have, however, extended our account of this volume so far already, that we must confine our extracts to just two or three of the most important points in the first of the cases related by Dr. Brooke. A boy suffered an attack of severe and very obstinate cough, in October 1818, when he was twelve-years old, about a month after he had been placed at school. None of the remedies employed—which consisted of bleeding,

vesicatories, emetics, purges, sudorifics, expectorants, and opiates,—afforded the smallest relief. After a fortnight, he was recommended to try his native air, and, being taken home, gradually recovered, the cough having continued about five weeks. In January 1819, he returned to school, and was again seized as before, only that the cough was less severe, treated as before, with the exception of bleeding, without apparent benefit; but immediately recovered on being taken home. He returned once more; “but it was only to suffer a third attack of his cough, which, in severity and duration, was nearly equal to the first. A full consultation of the neighbouring practitioners was now called, and every effort made for his relief, but in vain; in eight or ten days he was able to travel, and went home with the advice never to return, as the air was supposed particularly to disagree with his constitution.” Early in May he had the measles, with which he coughed much: the cough, however, was such as is usual in that disease, and not at all, as his mother remarked, like the other.

At the end of January in the ensuing year, he was placed at a school in Dublin; and on the 8th of February Dr. Brooke was sent for to see him, from his having been attacked with the cough. This was the first time he was seen, with the cough, by Dr. Brooke. “Upon the most minute and patient examination,” he says, “I was unable to detect the smallest deviation from health, or the smallest derangement in any of the organs or their functions, save the cough alone. This was the sum total of all his ailment that I could discover; and certainly it was a most extraordinary cough, and unlike any thing I had ever heard before; he told me it was precisely similar to the former attacks, and appeared quite in despair of being able to obtain any relief. He made a deep inspiration, and then uttered a short cough or ejaculation, sounding somewhat like *haghs*, in an extremely harsh, loud, and disagreeable tone, conveying to the hearer an idea of great distress in the patient, but which really was not the case; as he never complained of any thing until evening, when he always became weary, and felt a general soreness in the muscles of the thorax and abdomen. This monotonous cough was repeated three times very rapidly; he then fully inspired, and coughed as before; and again for the third time inspired and coughed. After this, he became quiet for some time, the interval varying from one or two minutes to four or five; so that each paroxysm may be said to have consisted of three distinct inspirations and nine expirations, the air taken in by each inspiration being expelled by three coughs, or ejaculations, as above described.”

He had taken a purgative; and, “to gain time for reflection,” Dr. Brooke merely, at first, directed him to remain in bed; to

take a mild saline sudorific medicine, as he had been walking on the water-side, during a cold north-east wind, on the day previously to the attack; and to use a tepid pediluvium. On the fourth day, he took three grains of calomel, and two of James's powder, at bed-time; and on the next morning a saline purging draught: after this, he had "several large and very offensive bilious stools yesterday; not the least alteration in the cough; tongue white, and back part thickly furred,—a circumstance which I think I have," Dr. Brooke says, "often observed suddenly to take place in incipient hepatic affections, after a three or four grain dose of calomel; and probably to be accounted for from the peculiar influence of that medicine over the secreting function of the liver, as well as its action on the excretory ducts." On the next day, the patient complained of pain in the right side; and, on careful examination, a tumor, situated below the points of the seventh and eighth ribs, and "occupying a space that might in circumference measure about four inches, was very perceptible to both touch and sight." Leeches and warm fomentations to the side were then employed, and a course of mercury prescribed; by means of which, and a vesicatory, with some other appropriate medicines, he recovered his health. He has returned to school, after having been at home again, and has not experienced any return of the cough.

It should be remarked that this boy, when at home, like most of the youth of the Irish gentry, was "accustomed to a good deal of exercise in the open air, particularly on horse-back," and was then "a hardy healthy boy." He had probably never been at school, or similarly confined, before; for it is very common for the Irish boys to be good hunters before they are able to read.

Had not the subject of cough and more serious affections of the lungs dependant on disease of the liver, been discussed in a general way as it has been by Dr. WILSON PHILIP, we should, notwithstanding the manner in which we are urged by our limits to close our account of this volume, have adduced some extracts from Dr. Brooke's very judicious and practical remarks on it; but as it is, we can only point them out for the attention of our readers.

An account of a case of *Melena*, communicated by letter to Dr. Brooke, by Dr. WHITLOCK NICHOLL, in which the spirit of turpentine, a remedy of which the use was proposed and established by the former physician, was successful, occurs next to our notice; and, after this, the history of "*a Case of Ruptured Vagina, which terminated favourably; notwithstanding the Strangulation and subsequent Sloughing of a considerable Portion*"

Transactions of the College of Physicians in Ireland. 497
of the Intestinal Canal; by THOMAS M'KEEVER, M.D. Assistant to the Dublin Lying-in Hospital."

The patient, 26 years of age, was delivered by the crotchet, after having been in labour twenty-four hours, by a medical practitioner whose name is not mentioned. "This operation occupied fully two hours, and was ultimately accomplished with extreme difficulty, in part owing to the very large size of the child's head, but principally to a deficiency of room in the cavity of the pelvis." On the ensuing morning, one of the patient's attendants observed what proved afterwards to be a portion of intestine, hanging from the vagina; it was then about six inches long: no mention of this was made to the medical attendant. No evacuation from the intestines had taken place by the fourth day, notwithstanding the use of purgative medicines; but there was no nausea or vomiting, nor does it appear that there was tenderness about the abdomen. On the fourth day, one of the women about her pulled strongly at the loop of intestine: after this there came on great pain, hiccough, vomiting, and swelling of the abdomen. Two days afterwards, she was seen, for the first time, by Dr. M'Keever. He found her suffering all that might have been expected under such circumstances, and "near a yard and a half of her bowels coiled up under her, black and, to all appearance, putrid; exhaling a shockingly offensive odour." This portion of intestine separated on the third day from this time, after which the woman had a copious discharge of feces (the first since her delivery) *per vaginam*, and finally recovered her health, with the exception of the evacuation of her feces and urine by the vagina. The portion of intestine is three feet eleven inches in length. It does not appear what division of the intestines it had constituted; but, a month after its separation, it is said that "the stools were of a bright-yellow colour, of fluid consistence, and are altogether free from fecal odour;" and therefore there is reason for conjecturing that it is a portion of the small intestines, as the length of it would also indicate.

"*A Case of Diseased Heart, in a Patient who had suffered severely from acute Rheumatism.* By DANIEL FALLOON, M.D. Licentiate of the King's and Queen's College of Physicians in Ireland."

This case presented the ordinary and severe symptoms of chronic inflammation of the heart, with adhesion of this organ to the pericardium. The interesting and peculiar circumstances which we shall notice, are the great relief from very severe symptoms, indeed from apparent imminent death, and the preservation of the patient's life, in a tolerably comfortable state for the most part, for about eight months afterwards, by

means of blood-letting and other remedies directed by Dr. Falloon ; but especially, as it appears, by blood-letting, general as well as local, and this very frequently and to a considerable extent. The history of the case is too long for us to attempt to give an abstract of it, but we ought to remark that it presents very favourable indications of the author's talents, and of the energy with which he would employ the means prompted by rational views, under apparently desperate and hopeless circumstances.

The last case in this volume, which is one of *recovery from the effects of corrosive sublimate*, related by CHARLES LENDRICK, M.B. M.R.I.A. is also an interesting one, in consequence of the recovery having taken place after the effects of the poison had been manifested in a severe degree. It appears that the quantity of sublimate swallowed by the patient, who was an adult man, exceeded half a drachm. An emetic had been administered, and vomiting had taken place, before he was visited by Dr. Lendrick, who saw him with Mr. Buchanan. Orfila's remedy, the whites of eggs beat up with water, was then administered; and it was found, on visiting him a few hours afterwards, that "the alarming symptoms had diminished after the first exhibition of the eggs, and almost entirely subsided subsequently to the others."

From this time his recovery was progressive ; but it is interesting to remark, "symptoms resembling periostitis occurred over each tibia about the tenth day, and it was proposed to make an incision : as, however, he was now sufficiently recovered to attend to his ordinary avocations, this intention was relinquished, and the use of the warm-bath proved sufficiently effectual. A slight degree of paralysis of the right side, with nervous irritability in other respects, attended with some loss of memory, supervened ; and from these complaints he is not yet free." The paper is dated at the period of four months from the occurrence of the accident.

Practical Observations in Midwifery ; with a Selection of Cases.

Part I. By JOHN RAMSBOTHAM, M.D. Lecturer on Midwifery at the London Hospital, and one of the Physician-Accoucheurs to the Lying-in Charity for delivering poor Married Women at their own Habitations. 8vo. pp. 422. T. and G. Underwood, London. 1821.

MIDWIFERY is a department of the art of medicine in which books of the character of that before us are of especial utility, —it may, indeed, be said, of absolute necessity. General principles or precepts may direct us, with tolerable precision and success, in the treatment of most diseases; but, in the more

complicated and difficult cases of parturition, the practitioner seeks, with anxiety, for precedents which furnish examples of what nature has accomplished, and of what has been effected by art, on, as nearly as possible, similar occasions: he feels the insufficiency of general principles, which may not take especial cognizance of minute circumstances, that, in certain cases, become matters of paramount importance, though ordinarily of but trivial interest; and he knows that, in cases of imminent danger, there is not time for that reflection which might enable him to apply those principles to the occasion before him with due confidence and precision, even though they embraced all the indications which should regulate his conduct. Those remarks might, it is true, be almost equally well applied to some points in surgery; and hence it is that we all of us perused, in the course of our studies, the writings of POTT on Injuries of the Head with so high a degree of interest and gratification. This allusion to those Dissertations of POTT has occurred somewhat luckily for us, as it points out the means by which we may, in a few words, enable the reader to form a notion of the character of the work before us, by stating that the plan and method of the two are similar, only that Dr. Ramsbotham has precluded his cases illustrative of certain points of physiology or practice with general considerations adapted to a complete series of cases, in an uninterrupted manner; whilst POTT has often treated of the several points of his cases in distinct and separate discussions. The addition of these preliminary general reflections gives the work of Dr. Ramsbotham a decided superiority, as regards its method alone, without considering the value of the accounts of cases, to that of Smellie and other collections of observations of a similar kind; as it combines the utility of general principles with that of particular examples.

The subjects treated on in this volume are, in the first place, the physiology of the uterus as far as regards the parturient function; the phenomena of natural labour, and the conduct of the medical assistant, especially in respect to the management of the placenta; and the ordinary "occurrences after delivery:" after which are considered, in succession, *adhesion of the placenta,—retention of the placenta,—disruption of the placenta,—relaxation of the uterus after delivery,—collapse after labour,—protracted labours, with their different causes and degrees of difficulty,—and rupture of the uterus.* The author says, in the Preface, "Should the present attempt be favourably received, I may be induced, at some future time, to continue and extend practical observations to other cases of difficulty and danger in the act of child-birth."—"I have merely stated such facts as I have seen, (he also observes;) such, indeed, as have occurred

in my own practice : I may have omitted many remarks which ought to have been inserted, but I have not availed myself of the writings of others." This, we believe,—considering the writings on Midwifery that were already extant,—is the best way in which the author could have presented to the public the results of his observations and reflections. It is particularly interesting to practitioners to know what are the most important cases of difficulty which have occurred to one man in the course of his life, and what inferences such cases have led him to form; as those which have happened to one—bating only what depends on the age and particular opportunities for observation of the individual in question—is really likely to happen to every one; and the inductions made by a man of good talents from what he has himself observed, respecting such a subject as that under consideration, are of very different and superior value to those founded, to more or less extent, on the recorded observations of others.

It does not come within the province of the Reviewer to give a regular analysis or abstract of a work like this; we have only to remark, in a general allusion, that the author's reflections are characterized by good judgment and comprehensive views, and present, in his several discussions (already enumerated), excellent practical dissertations on the subjects to which they relate; whilst the cases which are narrated are, for the most part, very interesting, and strikingly illustrative of the points to which they are applied. We shall not have any of them transferred to our pages, with the view—as it is said by the followers of this trite practice—of giving a specimen of the author's manner; for, in truth, to divulge "a secret of our prison-house," this is only the Reviewer's excuse for filling a few pages with no other trouble to himself than the sending a direction that such a passage should be copied, with the book, to his printer. There are many books, of which it is possible to give a tolerably satisfactory view in a concise abstract, but this is not the case with one of the character of that before us, where the utility of it consists chiefly in the minute and precise details which it comprises; and, if the work possess the merit and utility we believe it to possess, the selection of a detached part would be as absurd an attempt to show it, as that of the Grecian fool who carried with him a brick as a specimen of the house he had for sale. Of the author's observations on *rupture of the uterus*, we shall, however, give an abstract, and connect it with some observations contained in a dissertation by Dr. DEWEES;* which, with the evidence to which we shall also refer, seem to us to be

* Published in the second Number of the *Philadelphia Journal of the Medical and Physical Sciences*.

fair and forcible arguments against the precept of HUNTER, GARTHSHORE, and DENMAN, for the treatment of cases in which such an accident has taken place, and which has hitherto favoured the practice, generally, in England, that resulted also from the example of SMELLIE; who says, in reference to a case of rupture of the uterus which he had met with, "In order to avoid reflections, this accident was kept secret."*

Dr. Ramsbotham relates here seven cases of rupture of the uterus,† which he has himself witnessed, and which occurred either at the full period, or near to it, of ordinary utero-gestation. The volume contains, besides, an account of a case where this accident occurred about the fourth month; and another, which has been communicated to the author. Every case which has occurred to the observation of Dr. Ramsbotham "has sooner or later proved fatal."—"Some women (he remarks) scarcely survive delivery; others bear up against the effects of the accident for several days." Some writers, as CRANTZ and LEVRET, have pretended that the occurrence of this accident might sometimes be foreseen, by certain signs; but these are extremely equivocal, in the opinion of Dr. Dewees, who has minutely examined them; and Dr. Ramsbotham says, "I know of no particular symptom threatening its approach, or indicating when it is about to happen, which would justly warrant a premature resort to delivery." We shall transcribe Dr. Ramsbotham's account of the signs of the actual occurrence and effects of this accident.

"Rupture of the uterus always takes place suddenly, and generally without any previous warning. While the labour appears to be going on naturally but slowly, the woman is seized in the middle of a strong expulsive effort, with an uncommon pain in some part of the

* Smellie, it is worthy of remark, seems to have felt no repugnance on finding his example followed by others; for he publishes an account received from a correspondent, who tells him, in a similar case, that, "according to his *prudent* advice, he spoke nothing of the matter." A disposition to follow this *prudent* advice has conduced, perhaps, to the opinion, which has not unfrequently been expressed, that rupture of the uterus is but a rare accident in parturition, since so few instances of it have been recorded, at least in English medical literature. But, when we consider that Dr. Ramsbotham has been called to nine cases, and that GREGOIRE of Paris witnessed sixteen in the course of thirty years' practice, it would appear to occur not very unfrequently. Though, on the other hand, it is stated that, amongst twenty-three thousand one hundred and fifty child-births, which occurred in a given time at the Hospice la Maternité at Paris, there was only one case of rupture of the uterus. It is not at all probable that the accident could have happened in that establishment, at least if death ensued from it, without the discovery of it. This evidence is worthy of being collated with the fact that the greater proportion, by far, of the recorded cases which have occurred to the observation of medical men, have affected women who have been attended in their labour by midwives alone, or by obviously unskilled practitioners, until the occurrence of the accident.

† Two other cases are also related by Dr. R. in the *London Medical and Physical Journal*, for October 1813 and September 1814.

belly: this pain is of a very different nature from those pains of labour under which she has hitherto suffered; she has never felt the like in any preceding confinement. The attack of this new pain usually occasions a shriek, and is accompanied with the sensation of something having given way within: it is commonly followed by a sense of weight and oppression, and sometimes by the feel of a rising of her *burthen*. The patient now involuntarily puts her hand to her belly, with a complaint of increased suffering, and utters frequent exclamations expressive of misery, with "Oh! this pain!" This new pain is referred to one point, on one or other side of the uterine tumor; and it is stated to be similar to that which would be occasioned by cutting or tearing the parts asunder, and sometimes it is likened to the cramp. After its attack, the regularity of the labour-pains is suspended: uterine action either ceases altogether, or is gradually diminished in energy and effect. By and by, the woman complains of faintness, which shortly approaches to syncope; the countenance becomes pallid, and is at the same time expressive of great anxiety; the eye rapidly loses its natural lustre; the pulse gradually gives way, and becomes quick and tremulous; difficulty in respiration is presently perceptible in a greater or less degree; and there is a general restlessness of body, with coldness of the extremities. In cases in which there has been no previous sanguineous discharge, a slight degree of external hemorrhage now makes its appearance. In those in which there has previously been some trifling show, it is suddenly increased in quantity. Vomiting of greenish or dark-coloured fluids, in some instances, almost immediately supervenes to the accident; in others, it comes on a short time before the death of the patient. There is an occasional return of uterine action, but in a slighter degree, which the woman unavailingly assists by the voluntary efforts of the diaphragm and abdominal muscles: she is at the same time perfectly aware that there is a material alteration in the kind of pain, from her inability to bear down as she has been accustomed to do."

"A rupture of the peritoneal coat of the uterus," Dr. R. adds, "sometimes happens without extending itself into the uterine structure. Under this occurrence, we observe all the symptoms of actual rupture of the uterine structure itself, in a diminished degree, except those connected with the escape of the child."

"A breach in the vaginal surface also occasionally occurs, which seems to be produced by the continued pressure of the head, impelled by powerful uterine action. If the breach be trifling, the accident may not be productive of much inconvenience: if it be extensive, and especially if an opening be made into the abdominal cavity, such a similarity of symptoms follows as induces a suspicion that the uterus has given way."

A very remarkable case of rupture of the peritoneal tunic alone, is related by Mr. C. M. CLARKE, in the third volume of the *Medico-Chirurgical Transactions*. This case terminated fatally half an hour after the accident, (the woman dying un-

delivered, under the care of a midwife,) having presented the ordinary symptoms of rupture of the uterus generally.

Dr. Ramsbotham says he has never met with rupture of the uterus in a first parturition.

"The accident has happened, in those cases which I have seen," he says, "in a subsequent labour, and sometimes after several difficult births, though living children have been expelled. I am thence led to suspect, either that the uterus has received some local mechanical injury from the violence of its own efforts, or from the previous effects of artificial assistance, by which its structure is at this point weakened; or that it is thinned at the part where it gives way, during the last months of gestation, by continued pressure against some prominent part of the pelvis.

"The breach of structure usually happens somewhere about the cervix, either anteriorly towards the symphysis pubis, or posteriorly towards the prominence of the sacrum. The rent is either transverse or is carried laterally upward. The fundus uteri rarely gives way, yet its body and sides occasionally do."

Dr. Dewees has arranged all the obvious causes of this accident under two heads: those which act directly, and those which act indirectly, on the uterus.

"The first, or direct, are mechanical violences, and may be external or internal. The external may be a blow, a fall, a kick, or violent pressure; the internal may be, attempts to turn, or to return a prolapsed limb, or the mal-adroit application of instruments, or the unequal surface the fetus itself may present.

"The second, or indirect, are those which impair the integrity of the substance of the uterus; such as all those causes which offer a mechanical impediment to the passage of the child, as a contracted pelvis, an unusual sharpness of the linea iliopectinea, and exostoses, tumors, scirrhus indurations, and ulcers."

In a case which occurred to the observation of the physician just mentioned, the influence of inordinate pressure during the course of utero-gestation, seems to be well proved: the head of the fetus measured one foot ten inches in the horizontal circumference of the superior part of the cranium, "and the lower part of the wound, (in the uterus,) where the rent began, was gangrenous." This gangrene cannot be supposed to have commenced subsequently to the rupture of the uterus, for it does not appear that the woman lived more than twelve hours after that accident; but, as it was not accompanied with any striking symptom, it was not known precisely when it happened, though it seems probable that it occurred only a few hours, at the utmost, previously to death.

Dr. Ramsbotham is disposed to think that a "*thinning*" of some part of the uterus, from undue pressure during pregnancy, may be a common cause: and Mrs. BOIVIN mentions that, in a

woman who died soon after delivery at the Maternité, from a pulmonary affection, whose pelvis measured only two inches and three quarters from pubis to sacrum; the uterus, at a few lines above its neck, in the situation corresponding with the sacro-vertebral angle, appeared to be very nearly *worn through* for the space of two-thirds of an inch, having in this part not above the eighth part of a line (96th part of an inch) in thickness. The fetus had been extracted by means of the perforator. The cases of DENMAN, BYE, and several others, show also the effect of undue pressure on some projecting point of the bones of the pelvis, in the production of this accident.

One of the cases related by Dr. Ramsbotham, in which the rupture took place about the fourth month of pregnancy, is particularly remarkable. Dr. R. says,

“About four *p. m.* on Friday, June 2d, 1820, I was called in a hurry to see a lady in Providence-row, Finsbury, who was stated to be dangerously ill, and who was said to be about four months advanced in pregnancy of her first child. I learnt that she had been suddenly seized with sickness and vomiting about eleven *a. m.* (after passing a good night,) which her friends attributed to some mackerel dressed with vinegar, of which she had freely eaten the preceding evening at supper. The family apothecary had been called, who ordered some medicine; but, as she seemed to be getting worse hourly, I was sent for. I found her under symptoms of the greatest danger: her pulse was scarcely to be felt; her countenance was pallid and depressed; her hands were clammy and cold; and she complained of pain in the belly. There had been no external flooding, yet the symptoms struck me as being indicative of internal loss of blood or of the effects of lead on the constitution. I ordered some opening medicine, and the frequent injection of clysters, with a promise that I would shortly see her again. In little more than an hour a message was sent to my house, requesting I would see her again immediately: on my arrival at the house, she was dead. Leave was obtained to inspect the body the next day. The uterus was found to be ruptured on its left side, and the ovum had escaped in its membranes entire into the cavity of the abdomen, in which was also a large quantity of coagulated blood, to the amount of several pounds. The uterus had a singular appearance: it seemed double, and to consist of two parts united longitudinally together; but the ruptured portion had no external opening,—that is, it had no *os uteri*. Each portion had an ovarium attached to it. This uterus and ovum are preserved.”

A case nearly similar to the foregoing is related by DIONIS: the remarkable difference between the two consisting in the communication of the cavity of the uterus in which the fetus was lodged with the vagina in this, whilst it was wanting in that of Dr. Ramsbotham. The subject of the case mentioned by Dionis was a *jemme de chambre* of a *dauphine* of France: she was seized, towards the sixth month of her pregnancy, with

violent pains in the belly, which continued for three or four hours; after this she, for a time, ceased to feel the motions of the fetus. Twelve days afterwards, she was taken, about eight o'clock in the evening, with pains not less severe than those in the former attack; she now and then experienced nausea and vomiting. Convulsions came on in the course of the night, with cold sweats, tumefaction of the belly, and great prostration of strength, which terminated in death. Dionis opened the body. He found the fetus amongst the intestines, immersed in a large quantity of blood. The navel-string was whole, and the placenta still adhered to the uterus. This organ was divided, towards its fundus, into two parts, each having a distinct cavity, both of which had a common opening into the vagina. Each body had a fallopian tube and ovary: the left one, which had contained the fetus, was ruptured; the right presented the produce of a more recent conception, which was of the size of a small egg.

The most extraordinary accident of this sort on record, is that related in one of the volumes of the *Edinburgh Medical Essays*, where not only the uterus, but the abdominal parietes also, burst, and exposed the fetus.

Although all the cases which have occurred to the observation of Dr. Ramsbotham have terminated fatally, he says, "Some cases are upon record, in which the woman has recovered. Notwithstanding my want of success, I have always thought it my duty to offer a chance of life to the mother, by the only practical expedient,—by as early a delivery after the accident as the case would allow. I cannot accede to the doctrine of allowing the woman to die undelivered."

The establishment of this rule of practice is the especial object of Dr. Dewees, in the dissertation to which we have already referred; and, after a minute, candid, and judicious examination of the histories of cases on record, he seems to have shown its propriety in the most satisfactory manner. The arguments used by Hunter and Denman, for the contrary practice, are discussed by him in an extensive manner, and he finally "challenges the advocates of Dr. Denman's opinions to prove that there was an instance of recovery, 'where no operation was performed;'" whilst many well-authenticated instances of recovery when delivery has been effected, are recorded.

"The first instance that is distinctly recorded," says Dr. Dewees, "is that mentioned by Heister,* on the authority of a surgeon named Rungius. In this case, the intestines were distinctly felt through the rupture of the uterus, and through which the fetus was extracted; yet

* *Instit. de Chir.* tom. ii. p. 137.

the woman recovered. Dr. Douglass, in his essay,* gives the history of Mrs. Manning, who also recovered. Dr. Hamilton† relates another instance of complete and entire recovery, although the intestines issued through the wound of the uterus, and were reduced by him after the delivery of the child. In this case, he declares "the recovery was nearly as good as if no extraordinary accident had happened." Dr. Ross relates the case of a Mrs. Granan, of Eppendorf, near Hamburgh, who suffered this accident in two consecutive labours, and yet recovered.‡ Mr. Kite gives a case of ruptured uterus, which terminated favourably.§

"In a copy of the MS. Lectures of Dr. J. Hamilton, the present Professor of Midwifery in Edinburgh, there are two cases related of recovery; one of which he himself attended, and says, it was 'one in which almost every circumstance was unfavourable;' for, 'in bringing the child through the lacerated part,' he 'felt the uterus tearing more: the woman lost three pounds of blood; yet she recovered, and afterwards had children.'" The other case occurred in Lancashire:—"A poor woman fell from a cart, in consequence of which the uterus was ruptured, and the child passed into the abdomen. The bones of the pelvis were so much injured by the fall, as not to allow of delivery,

* "Essay on the Rupture of the Uterus, p. 7.

† "Outlines, p. 344.

‡ "Annals of Medicine, vol. iii. p. 377.

§ "Mém. Med. Soc. of London, vol. iv. p. 233. Madame La Chapelle also; *Annuaire Medico-Chirurg.* tom. i. p. 542."—We have to remark respecting the case related by Mrs. La Chapelle, that it is not so precisely applicable to the question under consideration as Dr. Dewees intimates, by the context. It was not a well-determined case of rupture of the uterus, by which the fetus might have escaped into the abdominal cavity: it seems to have been rather a separation, to a certain extent, of the uterus from the vagina, by a transverse laceration, or such an accident as is noticed by Dr. Clarke, of Dublin, in the following paragraph, referring to the cases detailed in the Transactions of the Association of Physicians of Dublin.—"A survey of these eight cases, (Dr. Clarke says,) will show that the anterior part of the vagina, near to its connexion with the os tincæ, is the part most apt to give way on certain extraordinary efforts, whether of nature or art:" only that the separation took place at the posterior part of the vagina, in the case of Mrs. La Chapelle. We quote her account of it. "On the introduction of a finger into the vagina, I perceived that this canal was separated from the neck of the uterus at its posterior part. The state of the patient did not permit a more full examination; we only directed our efforts to the recovery of her strength."

Dr. Dewees says, (we may remark in this place,) in another part of his dissertation, "If the laceration happen to the neck of the uterus, or at its connexion with the vagina, it is much more frequent that the fetus, with the placenta, pass immediately into the abdomen." This inference is precisely opposite to that which we should be disposed to form, from reasoning on the mechanism of the parts concerned; and it is exactly opposite to the conclusions of Portal, who says, "Il paroît, d'après la lecture de diverses observations rapportées par les auteurs, sur la rupture de l'utérus pendant l'accouchement, que lorsqu'elles se sont faites dans son fond, ou dans son corps, l'enfant s'est frayé une route dans la cavité du bas-ventre, et que la femme a péri; mais que, lorsque c'est le col de la matrice qui s'est déchiré seulement, alors l'accouchement s'est fait par les voies naturelles, et la mère a pu vivre." Saviard and Chaussier make similar remarks. Rupture at the connexion of the uterus and vagina is, according to most authors, very frequently owing to improper or awkward introduction of the hand.

being much *mashed*: the Cæsarian operation was performed, and she recovered.'

"Mr. Thibault* relates a similar case to the one just recited: gastrotomy was performed with the most entire success to the woman, though too late for the preservation of the child.

"Baudelocque relates† that a M. Lambron, a surgeon of Orleans, performed gastrotomy twice on the same person, with the desired success to the woman, after the rupture of the uterus. This woman became pregnant a third time, and was delivered naturally of a healthy child.

"Mr. Hugo‡ relates a successful case also, and we could without difficulty increase the number; but these are sufficient to prove that success has attended the 'interposition of art.' The cases we have just cited were all fortunate to the woman, but the child uniformly perished: this was rather owing to the time at which art interfered, than to the mode it adopted. Of this we have sufficient proof in the case related by Burton.§ He says, 'I was called to the wife of a broker in the city of York, who had had several children: she fell into labour at the regular time; she had only a slow labour at first, but after some little respite her pains became more violent; during one of which she perceived something to crack within her, as she termed it; after which, she exchanged her pains for faintings, &c. with an intermitting pulse. On this account I was called in. Being told every thing that had happened, I was apprehensive of what indeed proved to be the case: wherefore I told the by-standers my opinion; and that, as the child was alive, it was proper the woman should be delivered as soon as possible; which was done directly. The child was small, but very healthful and lively. Immediately after the birth, I introduced my hand into the uterus, where I found one side of it burst so wide as to have admitted my hand to pass through the opening.' Mr. Haden relates a case that terminated with safety to both mother and child.||"

In addition to those, we are enabled to add the cases related by Dr. CLARKE, of Dublin;¶ Dr. LABATT;** Dr. FRIZELL;†† in the *Gazette de Medecine* for 1778, (where the child was turned and delivered by the feet;) and by Dr. Rossi, of Parma, noticed in a late volume of this Journal. In the last instance gastrotomy was performed. The *Leipzig Commentaries* also contain the history of a case, where the uterus was ruptured by a violent blow on the belly: the fetus was felt projecting at the

* "*Jour. de Med.* for 1768. † Heath's *Translations*, vol. iii. p. 430.

‡ "*Med. and Phys. Jour.* for March 1808.

§ "*System of Mid.* sec. xliii. p. 110.

|| "*Med. and Chirurg. Trans.* vol. ii. p. 118."

¶ *Trans. Assoc. Dub. Coll. Phys.* vol. i. We should however remark, that this case is not satisfactorily shown to have been an instance of rupture of the uterus, or that it was more than one of rupture of the vagina, or of the connexion between the vagina and uterus.

** *Dublin Medical and Physical Essays*, vol. i.

†† *Trans. Assoc. Colleg.* vol. ii.

left side of the abdomen; an incision was made here, and the fetus extracted. The woman afterwards bore children, in the ordinary way.

Dr. Dewees endeavours to show that the instances of supposed recovery from the effects of this accident, when the case has been "resigned to the natural efforts of the constitution," are not valid examples of such an event: for that it does not appear certain that the uterus was completely ruptured; or, in other words, he thinks some of them have been cases of original extra-uterine gestation, and others of rupture of the rest of the uterus whilst its peritoneal covering has remained entire, or that the fetus has not really passed into the cavity of the abdomen, but been enclosed in a membranous cyst formed by the peritoneum. We think it probable that Dr. Dewees is correct in supposing that some cases have been instances of extra-uterine gestation, in which the attendants have been deceived by the occurrence of the ordinary pains, like labour-pains, about the usual period of utero-gestation, which are known to occur in such cases, and continue for several hours, as we had occasion to remark in some late Numbers of this Journal.* Dr. Dewees notices particularly the cases said to have been instances of recovery when the fetus has escaped from the uterus into the cavity of the abdomen, and suffered to remain there, that have been related in the *Journal de Medecine* for 1780, and that by Dr. Sims.† That in the *Journal de Medecine* is selected, he says, as "one of the most favourable cases we could find upon record for the above opinion." We cannot transcribe his discussions of these points; they are necessarily too long for the limits of our Journal. In support of the opinion that the peritoneal coat of the uterus may have formed a cyst for the fetus in the cases alluded to, Dr. Dewees says, "We have Dr. Ross's third case, (*Annals of Medicine*, vol. iii. p. 306. 'On opening the abdomen of this cadavre,' says Dr. Ross, 'it was found under the ligamentum latum of that side, (the left,) an arm of the child could be felt, covered only by the peritoneum.' Here then is demonstration that the substance of the uterus can be torn without doing violence to its peritoneal covering."

Dr. Dewees will, probably, be thought to have formed too exclusive an opinion respecting the instances of recovery from the immediate effects of the accident, when the case has been left to the natural efforts of the constitution, and the fragments of the fetus have, at some remote subsequent period, been evacuated through the abdominal parietes. It must, however, be

* No. 248, p. 345; and vol. xli. p. 514.

† *Medical Facts*, &c. vol. viii.

very difficult to prove that, in any of these or similar cases, the fetus has been really in the abdominal cavity,—that is to say, not enveloped in a particular cyst; because the only evidence that is valid must be obtained by examination after death; and, on the patient surviving for any considerable period, there will, in all probability, in every instance, (as we observe in all analogous cases,) be something like a cyst formed around it, by means of the coagulable lymph effused during the inflammation which must ensue from the presence of such a foreign body in the cavity of the belly; or else so much alteration of the parts concerned will have taken place, that it will not be possible to decide whether or not a cyst had ever existed. Dr. Dewees, however, endeavours to prove that the cyst which was present in the case related by Dr. Sims, was not formed in this way, but was the natural peritoneum. However, admitting these cases to be instances of what is above denied, it does not appear that near so many of them can be cited, as there can of those of recovery when delivery has been effected by art. We have, besides, to take into consideration the wretched condition of those women who have survived the immediate effects of the rupture, and carried about with them a fetus undergoing decomposition in the abdomen. Many, indeed most, of those, on the other hand, who have been delivered, have subsequently enjoyed good health, and some of them have afterwards borne living children; as will be seen by referring to the cases noticed by Dr. Dewees, in a passage which we have already transcribed.

The remarks of Dr. Denman on this subject are absolutely surprising; he says, there was no instance of recovery within his knowledge, “except one, *which was doubtful*, of either of them (mother or child) being preserved,”* when delivery had been effected by art. This remark is made several years subsequently to the time when he had said,† “Besides some few others, of which I have been informed, or which are recorded, a case has occurred to my very worthy, able, and experienced friend, Dr. Andrew Douglass, in which the uterus was ruptured: he turned the child, the patient recovered, and had afterwards children. If no other case,” he continues, “had ever occurred, I apprehend this would be sufficient authority to render it in future the indispensable duty of every practitioner to act in a similar manner; and, bad as the chance is of the patient, to be strenuous in using all the means which art dictates to extricate her, if possible, from her danger, or to preserve the child.”

* *Essay on the Rupture of the Uterus.*

† In his *Introduction to Midwifery*, vol. ii. p. 117.

Dr. Denman may be readily excused for altering his opinion respecting the practice to be pursued in such cases, between the time of the publication of the two works referred to, and for being ignorant of some recorded cases of recovery, (if not in England, in other countries,) or for judging differently from most other men respecting their validity; but his attempt to throw suspicion on the case of his "*worthy, able, and experienced friend, Dr. Andrew Douglass,*" is not too harshly designated by Dr. Dewees, when he terms it "*disingenuous.*" Dr. Dewees also, very properly, says, "Why the case of Dr. Douglass should be doubtful in the year 1810, when it was recorded as an unequivocal instance of recovery in 1795, is extremely difficult to tell: it looks too much like a subterfuge to avoid the force which the fact brings with it."

Besides the preponderance, at least, of the instances of recovery when delivery has been effected by art, Dr. Dewees shows that the women who have been thus delivered have, on an average, lived longer after the accident than those who have died undelivered; and, probably, most practitioners may have been disposed to say, after thus using their best efforts, though unsuccessful, for their patients, with LAMOTTE, "*Quelqu' inutile que fût cet accouchement, nous fumes plus contents tous deux, elle d'être accouchée, parce qu'elle en mourut plus tranquillement, et moi de l'avoir exécuté.*"*

In a case of this accident, though delivery by art be determined on, the question, in what way delivery is to be effected, is still to be settled. The obvious means are, turning and delivery by the feet, delivery by the crotchet, and gastrotomy. The choice has but very rarely fell on the last method: but, in respect to this point, Dr. Ramsbotham remarks—

"As the number of women who have ultimately recovered from this accident is at the present so trifling, and as the occurrence is in itself almost necessarily fatal to the mother, it may be a question worthy the consideration of the profession, whether the Cæsarian section, offering a mode of freeing the mother from the child, with a chance of its life, ought not occasionally to be substituted for the perforation of the head. But, in determining on this tremendous expedient, which will place the chance of recovery to the mother in a still lower scale, we ought previously to ascertain, if not to a certainty, as far at least as probability will allow, that the child is still alive under the breach in the uterine structure. If this be the case, such a length of time ought not to be allowed to pass away in the interval as can be supposed to interfere with that life."

* The French practitioners, subsequently to the time of Lamotte, as well as those of the continent generally, it may be right to remark, make no doubt of the propriety of using the most prompt and efficacious measures for the delivery of the fetus.

We shall also transcribe the remarks of Dr. Dewees on this point, as he supports his arguments by precise references to facts; and to the successful cases of this operation which he notices, we may add that related by BAUDELOQUE (*Rech. sur l'op. Cæsar.* p. 58,) and that noticed in a late volume (xli. p. 514,) of this Journal, by Dr. Rossi.

"Gastrotomy and the Cæsarian section present horrors to the mind peculiarly their own; nor should we be able to overcome the appalling sensations they produce, if we were not influenced by paramount considerations. To save life is a strong motive to the operation; and to be snatched from death is a powerful inducement to submit to it. Where this is the only resource, the case should be fairly and candidly stated, that no after blame may attach; and, in all cases of such hazard, responsibility should be divided, by requesting the concurrence of a brother practitioner, where time too precious would not be lost in this compliance. It has been called a 'horrible expedient' by Dr. Douglass.* It is so confessedly; so are lithotomy and many other operations: but this is not to be the test. Its utility alone ought to determine whether it should be considered as a resource of our art, or be for ever proscribed: for we are by no means satisfied with Dr. Douglass's reasoning upon this subject. He asks, 'If a rupture of the uterus is of itself an injury so generally fatal, what is the patient likely to gain by combining the dangers of such an accident with those of a penetrating wound which will expose the abdominal viscera?'† Dr. D. appears to have forgotten that there was already 'a penetrating wound' which 'exposed' the abdominal viscera; and that an additional one through the integuments would scarcely enhance the risk, since we know that wounds of this kind are not necessarily mortal.‡

"Besides, what would Dr. D. have us do in those cases where there is no possible alternative (as where the pelvis is much deformed,) but this operation? for it is only in cases similar to these that the operation is recommended. The woman can but die after the operation; and this she certainly will do if it be not had recourse to: and what practitioner would not prefer an alternative that may succeed, though hazardous, to the abandonment of a patient to the unrestrained consequences of disease?

"That it has been successfully employed, we are not at liberty to doubt; nor is there any testimony that it has been either wantonly em-

* *Essay*, p. 51.

† *Ib.*

‡ "We find three highly-interesting cases of the extirpation of the ovaria, in which there was a speedy restoration to health, although the wounds through the teguments of the abdomen were extensive, and its cavity a long time exposed to the air. In neither of these cases did any upward symptom arise; though in the first case the tumor was so large as to contain fifteen pounds 'of a dirty glutinous-looking substance,' and the sac which contained it, after being extirpated, weighed seven pounds and one half. In the second, notwithstanding every care was taken to prevent it, a quart of blood was spread among the intestines; yet no unpleasant symptoms are said to have arisen. In the third, a diseased ovarium was taken out, which weighed six pounds; yet the patient recovered 'in two weeks.'—Dr. M'DOWELL's Cases, *Eclectic Rep.* vol. vii. p. 242.

ployed, or that it has added new sufferings or new dangers to the already almost certainly fatal disease for which it is proposed as a remedy. We shall add the evidence we are in possession of, that it has been successfully performed, and from it allow every one to draw his own conclusions as to its advantages. As regards ourselves, we have no hesitation in believing that it is exclusively indicated in several combinations of ruptured uterus. Mons. Thibaut des Bois, a surgeon of Mans, has given an account of this operation having been successfully performed on a woman several hours after the accident, though too late to benefit the child. He adds, 'that the woman suffered scarcely more than from the consequences of a common labour.*' M. Lassus† quotes a history of this operation having been twice performed with entire success on the same woman.'

Dr. Ramsbotham says, "In all cases there is a narrowness, if not an absolute deformity of the pelvis, so that perforation of the head becomes, too commonly, indispensably necessary to the delivery." This remark is certainly not universally applicable: it is probable it is not to the cases where women have borne living children subsequently, (at least, and perhaps to several of the others;) and it is certain that it is not to the case which occurred to the observation of Dr. Dewees, already noticed, where there was "neither 'deformity of pelvis,' 'exostosis,' nor unusual 'sharpness of the linia iliopectonea;' no 'tumor,' 'schirrous induration,' nor 'cartilaginous condition of the os uteri.'" In continuation from the passage last transcribed from the remarks of Dr. Ramsbotham, we find him say, "If the presenting part of the child have retreated from the situation which it had previously occupied, so that a considerable portion of the child has escaped into the abdominal cavity, delivery must be effected by the introduction of the hand, and extraction by the feet."

* *Journal de Med.* for 1768.

† *Pathologie Chirurgicale*, par M. LASSUS, tom. ii. p. 237.

CRITICAL ANALYSES

OF

RECENT PUBLICATIONS, IN THE DIFFERENT BRANCHES OF
MEDICINE AND SURGERY,

In the Literature of Foreign Nations.

Παρίδες ἄρα
Ἀνδρῶν, ἡ παλαιῆς ἀνδρῆς, ἀγαλλόμεθα.

Principes généraux de Physiologie-Pathologique, coordonnés d'après la Doctrine de M. Broussais. Par L. J. BÉGIN. 8vo. pp. 390. Mequignon-Marvis, à Paris, 1821.

[In continuation from page 432.]

WE have passed over the preceding chapter more rapidly than we should have done, probably, had we not been desirous to devote the extent of space we have yet to appropriate to our account of this work, to that on *the general treatment of irritations*; and we enter on this subject with the more zeal, from a considerable proportion of English medical men—judging from the works of the older writers, or the incorrect accounts of superficial observers, ignorant of the principles which direct their conduct,—having erroneous notions of the merit of the practice of the French physicians, which they are pleased to regard with ridicule or contempt. Let us see whether Mr. Bégin's account of the therapeutic principles of the more enlightened part of them does not present matters which might be considered, more advantageously, with somewhat different sentiments.

The first questions which present themselves when we endeavour to determine what should be the conduct of the physician in the treatment of diseases, are these:—Is it, in general, more advantageous than injurious to the patient to leave to nature the care of his cure? And, if this conduct be favourable in certain cases, and the contrary in others, what are the diseases which the physician should always combat; and in what circumstances should he abandon them to their natural course?

The partizans of the "*médecine expectante*" have adopted the principle that, in acute diseases, nature is disposed to a regular course, the issue of which is almost always more advantageous, in its results, than that of the violent methods of those physicians who would take into their hands the direction of the progress of the malady. Apoplexies, intermittent fevers of a very bad type, and some other very severe affections, have alone appeared to them to be beyond the restorative powers inherent with living bodies. Chronic diseases, on the contrary, although submitted to the same regulative principle of the organic movements, have almost always been considered to be above the efforts of this power; and it has been said that they, most frequently, claim the active interference of the physician.

It has been observed, by those same persons, that the termination

of violent disturbances of the functions is often accompanied with a sudden and immoderate re-establishment of the secretions, and the evacuation of a more or less considerable quantity of fluids. They have given to this remarkable phenomenon the name of crisis; and, generalizing the cases in which they have observed these crises to be followed by the prompt restoration of health, they have pretended that the physician should attempt to do no more than to combat symptoms of too great violence, to prepare and favour, or give rise to, the critical movements.

The suspension of the secretions, the burning heat and dryness of the skin, the thirst which torments the patient, the smallness in quantity and turbid state of the urine, are, in acute fevers, so many phenomena dependant on the irritation which is the source of the general disease. If this primary irritation suddenly ceases, either by the spontaneous actions of the economy, or by the effects of certain medicines, it is a natural consequence that the secondary phenomena which they have produced should cease at the same time. We observe then that the secretions are suddenly re-established, and with a degree of energy more considerable in proportion as they have been more completely arrested, and as the organs which are charged with their elaboration have been in a state of more intense sympathetic irritation. If we examine the patient at the time the crisis is about to take place, we find that the irritation of the organ primarily affected is becoming less severe, and that there is substituted an excitement of the secretory organs, the intensity of which increases in proportion as the other diminishes. It seems probable, then, that it is not because the "*crisis*" appears that the malady is alleviated; it is because the irritation of the parts primarily affected has subsided, that the secretions are re-established: the critical phenomena are the results, and not the causes, of the cessation of the disturbance excited by the inflammation of the viscera.

These different views have given rise to two different modes of medical treatment: that in which the critical efforts are regarded as the cause of the disappearance of the primary disease, has led to the adoption of the "*medécine expectante*;" that in which they are considered as results of its cessation, to the practice which will be described after we have shown somewhat more particularly the indications to which it is referable.

Many phenomena which authors have not considered as appertaining to crises, should however be allied with them, since they depend on the same vital laws, and lead to analogous effects, though the final results are sometimes different. We have seen, for example, that, when an organ is irritated, the parts more intimately related with it by sympathy participate with its irritation. Now it happens, very often, that this secondary irritation substitutes the primary, and thus terminates the original disease. It is in this way that cutaneous eruptions, boils, erysipelas, and many other affections, which succeed to gastritis, coincide with the disappearance of the latter disease after having been excited by it. It is by the same laws, however, that other diseases originate which are sometimes more severe and dangerous than

the original one. Thus, in children, the brain, sympathetically excited by irritation of the stomach and intestines, becomes in a few days the principal seat of disease, which will probably be more readily fatal than the gastric disorder. Sometimes the primary disease accompanies the secondary affection; and, in this case, the latter frequently seems to aggravate, rather than to alleviate, the former.

It has been generally observed, that the organ which predominates in the economy, and which characterizes the special idiosyncrasy of the subject, becomes almost always the seat of the secondary diseases which substitute the primary; or any other part which has been frequently much excited, and thus become the most irritable point in the body, will become the seat of the substituted affection. The physician may, then, in a great number of cases, be able to determine the organ in which the secondary disorder will appear.

Substitutions of irritation analogous to those we have seen take place from the internal organs to the external parts, will also be effected between the latter and the former. This phenomenon, although it occurs in an inverse direction to that above considered, is nevertheless a consequence of the same principle, and is effected by the same mechanism. It is thus that, on the skin being the seat of irritation, this affection may disappear suddenly, and the mucous membranes of the stomach and intestines, or of the lungs, will become the seat of more or less violent irritation. A man is attacked with irritation in one or more of his joints: the digestive organs are then sympathetically affected; the pain in this seat is soon, or immediately, followed by the cessation of the primary disease, and a gastro-enteritis succeeds to it. It is not then said that gastritis is the crisis of gout, and yet, for the sake of congruity, it should be said; since, in the case where gout succeeds to gastritis, it is pretended that the affection of the joints is the crisis of that of the stomach. In many instances, cutaneous diseases provoke internal inflammations, and disappear only on leaving behind them very serious maladies. Here the spontaneous efforts of the economy, far from being favourable to the patient, are evidently the contrary, and these effects of the *vis medicatrix naturæ* are even frequently fatal.

An important fact to be observed in the history of crises, is that, as the irritation is more acute, so is its disappearance more rapid: the more vigorous the patient, too, the more apparent are the critical phenomena, and generally more prompt in producing their proper results, whether they be for good or evil.

When irritation is developed, it is certain that, in submission to the organic movements, it tends to a termination after a longer or shorter period. Now, this termination may happen either only after a very long and almost indefinite period, or by the death of the patient, or with the re-establishment of health, which is operated with or without a crisis. Let the patient be cured, that is the object of the physician; and, in order that the medical art may be as beneficial as it is qualified to be, let the disease be removed as promptly as possible. Whilst it exists, the patient suffers, and he is submitted to all the hazards which accompany its presence, and the possibility of its exasperation. It is

perfectly indifferent to the individual who is cured of a severe disease, whether he has had a *crisis* or not. Besides this, the expected crisis, the precursors of which are watched with so much solicitude, is often wanting, when the disease is left to the spontaneous efforts of the system, or it is operated by a secondary irritation which is more dangerous than the primary; and, whilst we are observing the progress of the disease through its long course, unforeseen accidents may destroy the patient, or the irritation may become more obstinate and pass into a chronic form; or a secondary affection arises without the disappearance of the primary, and then we have two diseases to treat instead of one.

The foregoing remarks, (although they are but slight hints of what might be said on this subject), with the consideration that it is in the power of medicine to effect the desired object, seem to establish the axiom of the propriety of combatting irritations wherever they may be seated. This point being settled, it remains for us to consider the methods which are the most appropriate for the treatment of the diseases under consideration. The methods generally adopted consist in—1°. The application of general antiphlogistic measures and local blood-letting; 2°. The application of irritants to parts remote from the seat of the disease, in order to elicit the irritation from the latter; 3°. The topical application of stimulants to the part itself which is the seat of irritation. The second of these methods is founded on indications derived from the views of the spontaneous efforts of the economy that were above exposed: but let us examine each of the three in a particular manner, in order to determine its relative value to the rest, and the cases to which it is most properly applicable.

The first is, in general, the most efficacious, and that which is attended with the least inconvenience to the patient; and it is that which should be employed, for the most part, in the early stages of irritations. It must not, however, be supposed that it may be applied without discrimination, or without some modifications on particular occasions. When the irritation is very intense, and the pain attending it suspends, almost completely, the influence of the nervous system on other organs; and the heart, hardly able to contract, propels but a very small quantity of blood into the arteries, it would be imprudent to effect at once copious sanguineous depletion: such depletions have, not unfrequently, produced immediate death. The physician should then prescribe the application of leeches, and these in some instances should be only few in number: he will observe their effects, and, if the pulse become developed, he will then revert to this means in a less restricted manner. It is not a rare thing to see patients, thus roused from the most profound oppression and extreme debility, present symptoms of excitement sufficiently violent to indicate general blood-letting; whilst, a few hours previously, they would, probably, have borne hardly a dozen leeches. In the contrary case, that is to say, when local blood-letting, although moderate in extent, instead of leading to an increase of fulness and strength of the pulse, is followed by contrary circumstances, and the debility is increased, the indication is evident: sanguineous evacuations must then be abandoned, and

revulsives, that is, counter-irritants, should be resorted to, in order to elicit to the external parts the vital actions concentrated in the internal organs.

In subjects which present the phenomena of excitement of the sanguineous system to a high degree, it is easy to determine the conduct which it is proper to adopt: general and local blood-lettings, spare diet, and cool drinks, are indispensable, and their salutary effect is assured. It sometimes happens that a state of extreme plethora fetters the organic movements, and produces a state of debility that is merely apparent: some little acuteness is often necessary in order to discern this condition; and it requires, more imperiously than the preceding one, abundant emissions of blood, and the antiphlogistic diet to the most rigorous extent. The subjects of this condition present, generally, a very large chest, a deep-red colour of the surface of the body, especially in the face; the features of the countenance are unusually large; and there is a certain embarrassment in their voice and respiration which indicates that the chest is surcharged with blood.

In cases where irritations occur in subjects previously shaken by disease, exhausted by previous excitement, and emaciated by chagrin or a penurious and unwholesome diet, the state of the patient is often sufficiently embarrassing to perplex the most sagacious practitioner. We must take care here not to evacuate, by general blood-lettings, the small quantity of blood which is present in the external veins. By depriving remote parts of the fluid which excites them, we should augment the relative predominance of the irritated organ; for it would participate but very little in the effects of the depletion. Concentration of the vital actions is, moreover, operated with much greater facility in enfeebled subjects than in others; and it is more difficult to destroy their irritations by remote bleedings, because the irritated organs retain their undue proportion of blood with more tenacity. It is necessary, then, to apply our remedies as nearly as possible to the seat of the disease: it would even be desirable that we should immediately, without augmenting the irritation, subtract blood from the part affected; but this is not in our power in affections of the internal organs, and we must be content with operating on the part of the capillary system that is most intimately allied with them.

In cases of inflammation of the mucous membranes, it is the capillary system of the skin which is most intimately related with the parts in question, and it is, consequently, on this part that leeches should be applied; but, in acting thus, we might deprive the system of so considerable a quantity of blood, in regard to the condition of the patient, that death ensues as a consequence of it. When, then, the relief of the local disease is not equivalent to the general debility produced by the blood-letting, the effect of this measure is disadvantageous. In such cases, it is more proper to have recourse to irritants on exterior and remote parts.

The propriety of blood-letting, in general, is here the more insisted on, because the blood, although it is elicited to the part effected chiefly by the excitement of the nerves and vessels of this part, is

itself a powerful cause of excitement, which it is desirable to obviate. We know that the blood is more abundant in a given part as the vitality of this part is more active; and that, on compressing the arteries running to it, without totally arresting the circulation, we weaken the sensibility with which it is endowed; or, in other words, by diminishing the quantity of blood which flows through a certain texture, we render the vital actions of this texture less energetic. If we prevent, for some time, the afflux of blood to an irritated organ, the excitement of it ceases. It is thus that compression, methodically applied, opposes the inflammation of parts after strains, and removes inflammation of structures that can be duly submitted to its influence.

There are persons in whom irritations are so obstinate, and so tenaciously fixed in the parts affected, that it is almost impossible to destroy the morbid action, although we employ general and local blood-lettings, emollient applications, and the antiphlogistic regimen, to the utmost extent. Indeed, in proportion as we go on practising blood-letting in such cases, the patient becomes enfeebled, whilst the irritation seems to become more and more obstinate: sanguineous evacuations would produce death rather than the removal of the inflammation. If we persist in the use of them, the patient becomes emaciated, sinks with astonishing rapidity, and arrives at a state of extreme marasmus, without the inflammation having been dissipated. It is advisable, as soon as we perceive this disposition in the case, to refrain from further depletions to much extent. The first period of the disease, that in which it is possible to remove suddenly the irritation, being passed, the disease will run its course, and the functions will be for a long time disturbed. The strength of the patient should here be carefully managed; we should gain nothing by depressing it too much or too rapidly: we must give revulsive measures with the administration of sedatives internally, and have recourse to the application of leeches occasionally, as the intensity of the symptoms may render them necessary.

A condition necessary in all cases, in order that irritations may be successfully treated, is the absolute rest of the irritated part. Our physiology shows us that the exercise of the functions is a powerful cause of stimulation for the organs which execute them: the actions performed by a given part cannot take place without the blood being elicited into its capillary system, and the nervous powers being excited. It is incontestible that inflammation must be exasperated by a cause which is followed by such results. Irritations of the internal organs, the progress of which is the most rapid, and which lead to the most serious consequences, especially require a state of repose of the affected organs. It is hence that silence and inaction of the body are so necessary for the cure of pneumonia and pleurisy. It may be remarked that general bleedings act beneficially in the former of these diseases, not only by depletion of the vessels, but also by diminishing the mass of fluid the lungs have to elaborate; and thus they favour the repose we endeavour to give to those organs: and if very violent inflammation of the lungs constitutes, frequently, so obstinate a disease that it is not possible to remove it suddenly by means of blood-letting,

as some other inflammations may, it depends, probably, on our not being able to effect a complete suspension of the functions of the organ, the exercise of which constantly tends to preserve the excitement of its tissue. In irritations of the digestive organs, we can, by withholding the administration of alimentary substances, produce a suspension of their proper functions; and the success of our treatment will commonly be in proportion to the degree of strictness with which this privation of food is insisted on.

The second method, or that constituted of revulsive measures, may often be advantageously joined with the direct antiphlogistic means, when abstraction of blood has not been at once successful; but it is necessary to select with caution the period at which these measures are employed. If they are resorted to in a precipitate manner, they may give new activity to the original irritation, augment, in cases of gastro-enteritis, the arid heat of the skin, the frequency of the pulse, the dryness of the tongue, and the thirst, and render necessary further sanguineous evacuations.

It will be generally found that, whenever a remote irritation does not cause a cessation of the primary irritation, it increases, sympathetically, the violence of the latter; or, in the words of an English author, whose therapeutical disquisition constitutes one of the most interesting and valuable productions in medical literature, the irritation we produce in this case, instead of imitating the substituted, exemplifies the extended, disease. So that, whenever we judge it proper to have recourse to external irritants, we should use them in an energetic manner, and let their application coincide with that of antiphlogistic means; so that sedatives and refrigerants may be applied to the organ suffering the irritation, at the same time that the counter-irritants are placed on remote parts. By acting thus we may avoid the evils which are otherwise hazarded by the use of means of the revulsive kind.

The skin is the organ to which counter-irritants are most commonly applied: its exquisite sensibility, and the little danger attendant on irritations of it to a moderate extent, are the chief reasons for this choice; but it is not the only tissue on which those remedies may be successfully applied. They may, indeed, be administered internally, in cases where the intestinal canal is not the seat of irritation; and, when thus employed, they act beneficially in proportion to the sensibility of the mucous tissue on which they are placed.

Although it is almost indifferent what species of stimulant be applied externally,—the principal object of consideration being the diversity of the energy of the substance employed,—it is of the highest importance that a judicious choice should be made in the substances with which we irritate the intestinal tube. The membrane which lines this organ possesses a sensibility which develops such very different, and frequently opposite, results, according to diversities in the matters placed in contact with it, that the most dissimilar consequences often ensue from stimulation effected by bitter, aromatic, alcoholic, and purgative, substances. Another circumstance which should contribute to render the practitioner careful in his selection of means of this kind, is this, that the substances employed may be absorbed and

carried into the general circulation of the fluids, and thus cause various irritations in the system, more or less generally. Those substances should ordinarily be preferred that irritate the intestinal tube without acting with violence on the sanguineous system : amongst the matters of this class, purgatives hold the first rank. Besides the irritation they oppose to that we wish to remove, they excite, in the mucous membrane of the intestines, a very considerable organic movement, the object of which is the secretion of a greater or less quantity of mucous matter. This process requires the afflux of an inordinate quantity of blood ; it sets into vigorous action the nervous faculties of the abdominal organs ; and, when it is entertained for a considerable period, it is but rarely that nature does not abandon, sooner or later, the morbid operations it was carrying on in remote parts. There is also another advantage derived from the use of purgatives, when judiciously employed, which is the separation of a more or less considerable quantity of the fluids from the general mass ; an effect which produces a degree of debility often equally beneficial with that which follows evacuations by blood-letting, whilst its duration is less permanent. Some choice is necessary in the species of purgatives employed ; the milder sort being generally preferable for patients of the sanguineous temperament ; whilst the more active, and even drastic, are often most beneficial in persons of an indolent or phlegmatic constitution. Bitter medicines are preferable to purgatives in scrofulous subjects, and during chronic and indolent inflammations of the lymphatic system. Besides the irritative action they exert on the mucous membrane, they excite the sanguineous system, and restore to it the predominance it should preserve over the lymphatic vessels. We obtain here, then, a local revulsive effect, by the irritation of the mucous membrane of the stomach and small intestines ; and a general revulsive effect, which consists in eliciting the vital actions to another order of vessels than that which is the seat of disease.

In addition to what has been said respecting the use of counter-irritants on the skin, it should be remarked that it is not a matter of indifference whether they are placed near to, or remote from, the part which is the seat of the disease. Whilst the irritation is very acute, we should apply the irritating topics to distant parts : it is in conformity with this precept that vesicatories are successfully placed on the lower extremities in acute gastro-enteritis ; the feet, knees, and even the elbows, may be especially surrounded with rubefacient sinapisms, or vesicatories, in this case. Irritations about the large joints is preferred, because these parts are known to be related with the mucous membranes of the digestive organs by a particular sympathy, and that excitement of them exerts a more direct and powerful effect on those organs than excitement of most other parts. In proportion as the irritation has been prolonged, it is advantageous to approach the counter-irritation towards the seat of the former affection ; and, when this is of a chronic kind, the use of the remedial measure should not be suspended until the disease has entirely disappeared.

The revulsive measures are not constituted solely of the use of topics which excite irritation of surfaces ; there are various other

measures which produce somewhat analogous consequences. Thus, exercise of the limbs, in the greater part of chronic inflammations of the viscera, is a resource as simple as it is efficacious, and one which should never be neglected. Muscular exertions elicit the vital movements to the exterior parts of the body; and they produce a revulsive action which may be lessened or increased any instant, according to the existing indications. Their energy is very different from that of a blister, to which the economy becomes insensible after it has existed for a few days. During convalescence from chronic diseases of the viscera, this measure is almost indispensable, and is the most powerful means of obviating a relapse. The extreme rarity of deaths amongst soldiers,—even when by no means free from sickness in their ranks,—during long and difficult marches, is a fact which has been remarked in the most general manner.

The third method for the treatment of irritations,—that in which irritating substances are applied to the seat of the disease,—is, of the whole, the least advantageous; that which most readily leads to an exasperation of the malady, and which, consequently, submits the patient to the greatest number of unfavourable chances. The circumstances under which it may be convenient to employ it are, then, but rare; and well-cultivated experience and much sagacity are requisite in the practitioner, in order that they may be recognized. The cases to which it is applicable, are the following:—

1°. When the morbid irritation is intermittent, and when the intervals of the paroxysms are perfectly exempt from super-excitation. In this case, powerful stimulants, amongst which is the cinchona, are of the greatest efficacy; but the first and most important of the conditions on which the success of the treatment depends, is that the state of apyrexia be complete; and, in proportion as it is less perfect and of short duration, it becomes more difficult to effect the cure. When the excitement is sufficiently intense to keep up, constantly, a febrile commotion, but not sufficient to prevent a recurrence of rigor,—that is to say, when the fever is remittent,—the first indication is to alleviate the irritation; and if, after the diminution of this, the periodicity remains, leaving intervals of complete absence of fever, it will be proper to employ cinchona. The theory of the agency of this remedy is pointed out in former parts of this article.

2°. Another case in which stimulants applied to a part affected with irritation are frequently successful, is that of certain hemorrhages, and especially of hemorrhages seated externally. Stimulants seem to act here by causing the vessels to contract, or by temporarily rendering them torpid, after the first excitement from their application has subsided; but, when the vessels are intensely irritated, and when the patient is very vigorous, the re-action which follows their application often aggravates the accidents, and renders the hemorrhage more violent. Sometimes they seem to arrest the flow of blood only by substituting a real state of inflammation to the hemorrhagic irritation. Now, this conversion may take place when internal organs are the seat of the disease, and, such inflammation being often not less dan-

gerous than the lesion which has preceded them, the prudent physician will not use these means internally without the most careful circumspection.

3°. When the secretory vessels are especially affected with irritation, we may, in certain cases, substitute for this irritation a state of real inflammation, the course of which will be shorter than that of the former condition, whilst it terminates in a more assured cure. But remedies of this kind, though accompanied with but little danger when they are applied to the urethra, the conjunctiva, and several other surfaces, are attended with serious hazard when the digestive tube is the seat of the disease. Violent irritation of the mucous membrane of the urethra, or of the eye, may be borne without much peril, whilst that of the stomach and intestines would soon be mortal. There is a close analogy between hemorrhages and too abundant secretions, and therefore the indications for the use of remedies of the kind under consideration, are similar in both cases; as is, also, the theory of their mode of agency.

Such is the outline our limits will permit us to trace of the clinical practice of the more enlightened part of the French physicians: it does not present a single trait that is not copied from the picture drawn by Mr. Bégin; and those who may consider the representation it gives of its object importantly defective, (as an outline,) or devoid of keeping, are requested to refer to the original, and not to form their judgment before they have contemplated it when filled up and coloured by the hand of an able master.

Medical and Physical Intelligence.

Continuation of the Analysis of the Contents of the first Number of the Journal of Experimental Physiology of Dr. MAGENDIE.

"Note on the Introduction of Viscous Liquids into the Organs of the Circulation, and on the Formation of the Fat Liver of Birds." By Dr. MAGENDIE.

DR. MAGENDIE, it appears, in some experiments in which his object was to change the nature of the blood, had injected oil into the veins, thinking that "this innocent substance could circulate with the blood without inconvenience:" but this was not the case; "the animal subjected to the experiment died a few instants after the injection of an ounce of olive-oil into the jugular vein."—"On examining the organs after death, I saw (Dr. Magendie says,) that the olive-oil had plugged the extreme ramifications of the pulmonary artery, and that it had thus caused the circulation and respiration to cease, by preventing the arrival of the blood at the left side of the heart by the pulmonary veins. The oil had acted, then, as an inert, impalpable powder, suspended in water, which instantly produces death if it be injected into the jugular vein, because it obstructs the extreme divisions of the pulmonary artery." A somewhat-thick

solution of gum tragacanth “ produced exactly the same phenomena as the oil.”

Dr. Magendie had ascertained that substances injected into one of the branches of the vena porta, acted quite differently from what they did when introduced immediately into the general venous system. A few cubic inches of air thrown suddenly into the jugular or crural vein, kill almost instantly; whilst this fluid may be introduced, in any way whatever, into the branches of the vena porta, without “inconvenience.” Dr. Magendie wished to know, then, what would be the effects of oil passed into the vena porta. Two ounces of this fluid were thrown into one of the mesenteric branches of that vein: the dog, which was the subject of the experiment, continued for several days in a stage “approaching to death,”—lying on his side, breathing with difficulty, making no movements, “not even for the natural evacuations, which seemed to be effected involuntarily.” After the lapse of four days, “he began to come to himself; he took some food; and three days more were sufficient to restore him to health.” The injection was repeated after the lapse of eight days, with, however, three ounces of oil. “The same symptoms (Dr. Magendie says,) showed themselves, and the animal died in the night. I opened him the next morning. The vessels contained here and there some traces of oil; but the liver especially attracted my attention: this viscus was much larger than ordinarily; it was of a pinkish pale-yellow colour; some small irregular fissures were apparent on its surface, that did not appear to be of recent origin; its colour interiorly was similar to that of its surface: the organ, in a word, showed the closest analogy with the fat liver of birds.

“This last circumstance has appeared to me to be very curious. Indeed, the fat livers are produced by forcing the animals to digest a large quantity of aliments containing but little azote. These animals having no chylous system, (as I have shown in a Memoir expressly on this subject,) all that is absorbed by the intestinal surface passes immediately to the liver; whilst in the mammalia, the drinks, and probably a small part of the chyle, alone pass through the liver; the rest of the chyle passing through the thoracic duct.”

“*A Memoir on the Lymphatic Vessels of Birds,*” by Dr. MAGENDIE.

Until the year 1768, as every anatomist will remember, the existence of lymphatic vessels of the digestive organs in birds was never affirmed: it was in that year that HENSON announced his supposed discovery of them, in a letter to JOHN HUNTER, which was published in the 58th volume of the Philosophical Transactions. HENSON, on the same occasion, speaks of lymphatic vessels in the neck of birds; the discovery of which, he says, did not originate with him, but with John Hunter. Dr. Magendie says he was surprised,—on reading this paper of HENSON’s about three years since,—that so able an anatomist as John Hunter should have found such vessels in the neck, and have overlooked them in the abdomen, had they really existed. This reflection, notwithstanding the apology of HENSON, induced Dr. Magendie to make some researches on this subject. He chose the goose for the

subject of his examinations, because it was in this bird that Hewson states he had discovered them. He readily found them, on each side, in the neck; but "he was not a little surprised (he says) at not perceiving any trace of lymphatic vessels in the mesentery, notwithstanding the care and attention with which the examination was made." He sought for the *considerable plexus* which, according to Hewson, *embraces the superior mesenteric artery*; but he was not more successful: and it was in vain that he endeavoured to find the double thoracic duct. He repeated his examination a second and a third time, increasing his care and precautions; but it was useless: he always readily found the vessels in the neck, but never any trace of "chyliferous vessels, nor of the thoracic duct." He repeated his researches during digestion, but they were equally fruitless. He has since dissected above fifty birds, "of all kinds," carnivorous and others, and examined them whilst digestion was going on. The result of the whole of his observations has led him to infer that "the chyliferous vessels and the thoracic ducts do not exist in birds, and that the only traces of lymphatic vessels are seen in the neck."—"What anatomical circumstances, then, (Dr. Magendie says,) can have deceived Hewson, and led him into so serious an error as that into which he has fallen?" If it related only to the chyliferous vessels, he adds, the circumstance might be explained by supposing that Hewson had mistaken nerves for them; but this will not account for what he says about the thoracic ducts, which he states that he had even injected. There is no vessel running from the abdomen to the subclavian veins: even the azygos is absent in birds. The only structures which he thinks can have been mistaken for the thoracic ducts by Hewson, are the arterial tubes which sometimes "run from the middle part of the aorta to the pulmonary arteries, which are connected with the subclavian veins; but, as these vessels are entirely *obliterated* a few days after birth, it still remains to be known how Hewson could have injected them."

The foregoing remarks were comprised in a Memoir read to the Academy of Sciences in 1819: since that time Dr. Magendie has dissected a great number of birds, and has not only confirmed his former observations, but has also ascertained that the swan and the goose are the only birds which have a lymphatic apparatus on each side of the neck, terminated by a sort of lymphatic gland which communicates in some cases immediately with the subclavian vein.

Dr. Magendie has commenced some researches relative to the lymphatic vessels of reptiles and fish, described by Hewson and Monro. Hitherto his observations have led him to infer that these animals are entirely devoid of lymphatic vessels, and that the organs described under this name are only "sanguiferous veins." The sea-turtle, he adds, is the only one of those animals which furnishes objections to the foregoing as a general inference. He has himself injected vessels in the mesentery of this animal, which had the disposition of lymphatic vessels; but "as the mesentery was detached, it was impossible for him to follow them towards the vertebral column, and, consequently, to assure himself that they were really lymphatic vessels."

Curious Phenomena resulting from Blood-letting in a Horse.—Mr. BOULEY, a very able veterinary surgeon of Paris, bled a horse, having pneumonia, in the neck, with the phlebotomy, in the usual way. Nothing particular occurred during the early part of the operation; but, as the vessel into which the blood was received was not large enough to contain the quantity which Mr. Bouley wished to take, he, on this vessel being full, suspended the compression on the vein below the puncture, whilst the vessel was emptied. At the instant when the compression ceased, he heard a remarkable noise, which he had several times noticed in the course of his practice, without any ill consequence following the event, and to which he now, therefore, paid but little attention. The bleeding was completed, and the animal led into his stable. He had but just arrived there when he was affected with a general trembling; his breathing became laborious and plaintive; his pulse small, irregular, and much accelerated; and, finally, he uttered some deep groans, and fell down in his stall “as if stricken by lightning.” On reflecting on the whole of the circumstances of the case, Mr. Bouley believed that the noise he heard, above alluded to, arose from the rushing of air into the vein; and he instantly determined to draw more blood from the animal. As the blood flowed, the horse “appeared to assume a new life;” he made some efforts to get on his legs, but did not succeed until the lapse of five or six minutes from the last bleeding. When up, his pulse became sensibly developed, lost its rapidity; his breathing became deeper; and, in half an hour from the time of the accident, he seemed to be in “the same state as before the first bleeding.” Some new phenomena were now observed. The horse experienced, during the whole of the afternoon of the same day, “an extreme degree of sensibility of the whole of the right side of the body, (the side opposite to that in which the venesection was practised,) accompanied with very intense pruritus: he laid down and rolled himself about on this side, to rub himself against any objects that offered resistance.”

The pneumonia run its ordinary course, and terminated favourably. Thirty days after the accident the horse was put to his ordinary work, and has not since shown any sign of disease.

This case is related in the 2d Number of Dr. MAGENDIE's Journal, and Dr. M. remarks that Professor DURUY, of Alfort, has mentioned to him that he had witnessed a similar accident, in which a second bleeding was also immediately effected. This case terminated favourably. Dr. Magendie doubts whether sufficient air was introduced to have proved mortal if the second blood-letting had not been resorted to. He injected some air (he does not say how much,) into a vein of a dog, and then bled him; but the animal died as soon as if he had not been bled immediately after the introduction of the air.

Mr. CARPUE will commence his Summer Course of Lectures on the 11th of June.

MONTHLY CATALOGUE OF MEDICAL BOOKS.

Address to the Public, relative to some supposed Failures of the Cow-pox, at Repton and its Neighbourhood; with Observations on the Efficacy and general Expediency of Vaccination, and on the Injurious Consequences of Inoculation for the Small-pox. By B. Granger, Surgeon. 8vo.

A Treatise on Gonorrhœa and Syphilis. 12mo. 4s. 6d.

A Manual of Chemistry. By W. T. Brande. 3 vols.

A Treatise on Gun-shot Wounds, on Injuries of the Nerves, and on the Wounds of the Extremities. With five explanatory Plates. By G. J. Guthrie.

A Treatise on Indigestion and its Consequences, called Nervous and Bilious Complaints; with Observations on the Organic Diseases in which they sometimes terminate. By A. P. W. Phillip, M.D. F.R.S. Edin.

[HIGHLEY AND SON, FLEET-STREET.]

METEOROLOGICAL JOURNAL.

By Messrs. WILLIAM HARRIS and Co. 50, Holborn, London.

From April 20 to May 19, inclusive.

Day of Month.	Moon.	Rain gauge	THERM.			BAROM.		DeLac's Hygro.	WIND.		ATMOSPHERIC VARIATION.		
Apr													
20		•19	45 61 40	29°57	29°66	60 65	SSW	E	Rain		Th. &c.		
21			44 60 39	29°75	29°87	55 56	NNE	E	Fine		Cloud.		
22			42 58 42	29°96	29°81	59 60	E	ENE	Fine				
23		•09	47 65 48	29°70	29°42	62 60	E	E	Fine		Rain		
24			50 67 50	29°53	29°67	60 60	SSW	SSW	Fine				
25	☾		55 69 58	29°70	29°77	57 60	E	ESE	Fine				
26		•07	63 71 57	29°80	29°70	58 56	E	SE	Fine		Light.		
27			60 65 58	29°72	29°84	58 56	WSW	WSW	Cloudy	Fine			
28			60 68 55	29°86	29°84	56 56	ESE	E	Fine				
29			57 59 49	29°87	30°00	56 56	ENE	NNE	Fine		Cloud.		
30		•02	52 56 48	30°05	30°08	56 57	ENE	ENE	Cloudy				
May													
1	●		50 58 49	30°03	29°94	56 57	NNE	NE	Cloudy		Fine		
2			51 60 53	29°86	29°81	56 57	NE	S	Fine		Cloud.		
3			56 62 54	29°75	29°70	57 58	SSW	E	Cloudy	Fine	Cloud.		
4			57 65 55	29°70	29°73	58 58	ESE	SW	Fine				
5			59 68 50	29°64	29°51	58 57	SW	SE	Cloudy	Fine			
6		•15	53 62 47	29°60	29°64	57 57	SW	SSW	Fine		Rain		
7		•09	50 60 43	29°81	29°93	57 58	SW	SSW	Fine	Sho'ry			
8	☾	•26	50 63 48	29°99	30°11	57 57	WSW	WSW	Rain				
9			55 68 50	30°17	30°19	55 56	SW	SW	Fine				
10			58 63 55	30°25	30°18	53 55	W	SW	Fine				
11			59 65 49	30°02	29°90	57 60	WSW	W	Cloudy	Fine			
12			51 64 45	29°87	29°51	57 59	WNW	W	Fine				
13		•05	47 59 43	29°43	29°40	60 60	W	W	Showery				
14		•03	48 57 41	29°36	29°33	59 57	W	S	Fine		Sho'ry		
15		•15	49 56 42	29°41	29°52	57 55	W	W	Cloudy	Rain			
16			50 53 40	29°76	29°98	55 60	SW	SW	Fine	Sho'ry	Fine		
17	○	•29	51 57 41	29°99	30°11	60 59	W	NW	Rain				
18		•13	51 59 42	30°00	30°05	62 60	NW	NNE	Fine	Sho'ry	Rain		
19		•02	49 52 42	30°13	30°20	60 57	NW	NNE	Fine		Sho'ry		

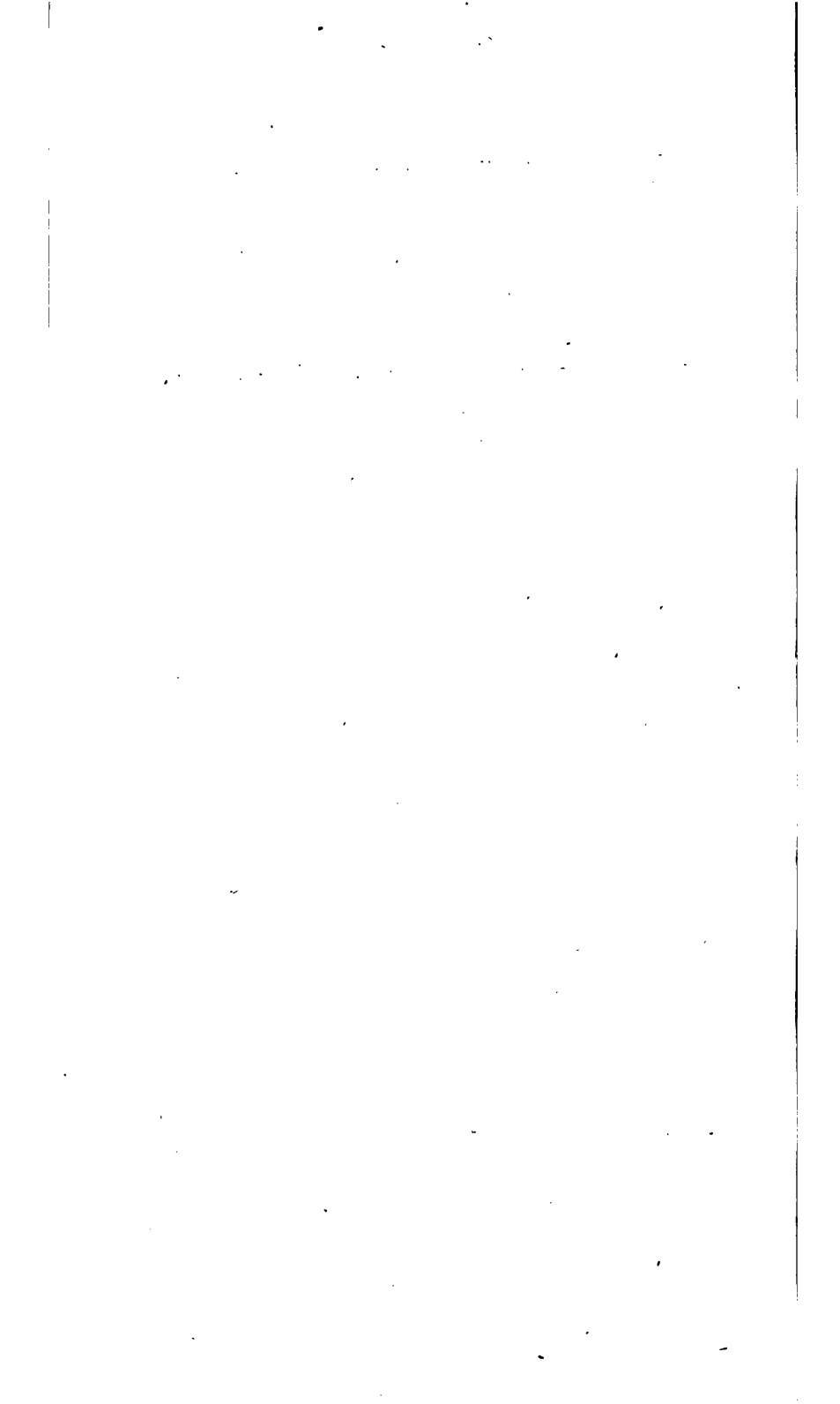
The quantity of rain fallen in the month of April,
is 1 inch and 67-100ths.

A
P R O È M I U M
TO THE FORTY-FIFTH VOLUME
OF
The London Medical and Physical Journal;
COMPRISING
AN HISTORICAL SKETCH
OF
THE PROGRESS OF MEDICINE
AND OF ITS AUXILIARY SCIENCES,
DURING THE SECOND SEMI-ANNUAL PERIOD OF THE YEAR 1820.



By WILLIAM HUTCHINSON, M.D.
MEMBER OF THE SOCIETY OF THE COLLEGE OF PHYSICIANS OF PARIS;
FELLOW OF THE LINNEAN SOCIETY;
MEMBER OF THE MEDICAL AND CHIRURGICAL SOCIETY OF LONDON;
AND ONE OF THE PHYSICIANS TO THE ROYAL METROPOLITAN
INFIRMARY FOR SICK CHILDREN.

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HISTORICAL SKETCH

OF THE

PROGRESS OF MEDICINE, &c.

THE history of the progress of the anatomy of the human body has become almost a blank, and even researches respecting the comparative structure of animals have ceased to present any original facts of much apparent importance to physiology. Men of the most eminent talents, who cultivate this part of science, have, indeed, for the most part, directed their efforts to the systematizing of what is known, and especially to the development of the relations of structure and functions in different animals, rather than towards the acquisition of further information of individual facts. This is a subject which furnishes a solid basis for a multitude of highly interesting and useful inferences; for, as far as the mechanism of the animal economy is concerned, there is a progressive analogy between the motive powers of the *polypus*, which swallows its own tentacula; of the *rotiferus*, which introduces food into its stomach by means of the vortex it produces by its circulatory movement; or even of the *tremella*, which, through the oscillation of its cylindrical and annular filaments, creeps on the surface of the ground, like a serpent, with a progression which* would attain the extent of a league in thirty-seven years;—there is a continuous analogy, as far as relates to mechanism, extending from the motive powers of those organized structures, up to those of the human body, through the whole of the series of animal beings. But, even on this subject, interesting as it is, nothing original of much importance has been promulgated in the period embraced in this history.

Some discoveries in human anatomy continue to be made from time to time, but there are very few of them which are of a kind qualified to improve our physiology. They chiefly relate to minute points of structure, that admit of no inferences which we can apply in a useful manner.

Of acquisitions of this kind, we have to notice those of Dr. ALBERT MECKEL respecting the *VILLOSITIES of the mucous membrane of the intestines of man and some inferior animals*;† a mode of structure

* According to the observations of SAUSSURE.

† *Ueber die Villösen des Menschen und einigen Thiere*. Von A. MECKEL (Professor zu Halle), Deutsches Archiv für die Physiologie. Funfter band, zweites heft.

which, as well as that of folds, 'grooves, and follicles, is the mean which effects here what cellules produce in the lungs,—*an increase of the extent of surface exposed to the influence of external agents.* Villosities are, indeed, only more minute forms of membranous folds, differing somewhat from the latter in the greater proportion of the extent of their surface to that of their base, and thus presenting where they exist—which is chiefly in the small intestines—the means by which the above-mentioned object may be most perfectly fulfilled.

Prof. Meckel has described the various forms of the villosities of the intestines of man; but details of his observations do not come within the scope of this Essay, for we can perceive in them no indications likely to be of utility: at least, we are unable to draw from them any inferences applicable to either physiology or pathology. The intimate structure of each villosity has not been satisfactorily ascertained. Prof. Meckel doubts whether they are constituted of minute capillary vessels, properly speaking, or of a continuous series of cells: though, from analogy with certain parts of some vegetables, he is inclined to believe the latter to be the case.

Some researches by Dr. BECLARD on *ossification* were noticed in a former Number of this series of Historical Sketches; and, as it was then hinted, they present the means of explaining some interesting points in pathology: he has recently published* some supplementary observations, which it is proper to notice in this place, though they do not seem to be fertile in useful indications. In his present additions to his account of the progress of the formation of the *spine*, Dr. Beclard supports his former assertion that the body of each vertebra becomes ossified from the extension of one central point only, and not from two lateral ones, as some other anatomists have supposed; and, in his references to comparative anatomy in support of his statement, he observes that the error of those who have maintained the latter opinion has arisen from their having examined too young subjects, and having mistaken for the commencement of the body of a vertebra the pedicle of each of the apophysarial masses. Observations on brute animals are especially calculated to furnish this incorrect view; for, in animals whose station is horizontal, the body of the vertebra, being the part of least importance, is developed the last, and by a comparatively very small point of ossification. In man, on the contrary, the body of the bone, being of eminent importance in maintaining the erect station, is first ossified; and this law is particularly manifest in the formation of the lumbar, sacral, and lower dorsal, vertebrae.

Some specimens of the *sternum*, of persons about 35 years of age, are in the possession of Dr. Beclard, in which there are two pisiform bony points, situate one on each side, at the upper end of the bone, and which are, perhaps, he thinks, the rudiments of the furcular clavicle of certain animals.

The bones of the *pelvis*, especially in women, he has sometimes found to possess an epiphysis, ordinarily pisiform or lenticular in

* *Note supplémentaire au Mémoire sur l'Ostéose, ou l'Ossification.* Nouveau Journal de Médecine, tome viii.

shape, forming the spine of the pubis, and which is commonly firmly attached to it: Dr. Beclard regards it as the rudiment of the *mammiæ* bone of some animals.

The spinous process which terminates the upper part of the *tibia* is sometimes formed by a small benticular epiphysis, which remains for a short time movable.

Some original observations follow, of more importance than those just related, since they serve (if the facts they indicate are as general as they may be very well imagined,) to explain the apparently opposing results of the experiments which have been made, especially by HUNTER and MAGENDIE, to ascertain the vessels concerned in the function of absorption: and it is somewhat remarkable that the discovery of the facts alluded to had been, in a manner, anticipated by Professor HALLER,* from his considering that they alone could explain the results above designated.

Dr. FOHMANN (prosector at the Anatomical Theatre at Heidelberg), during his researches, last winter, respecting the intimate structure of the intestines of man and some inferior animals, found, by accident, in the large intestines of a lion, a net-work of lymphatic vessels which had not been previously remarked in any animal; and which, on being injected with mercury, appeared to him to *inoculate directly with veins*. This discovery led him to make new researches in other animals, and he found similar lymphatic vessels in other *carnivores*,—namely, the *procyon lotor* and the dog; but here he could not clearly ascertain the passage of the mercury into the veins. He repeated the injection of the same lymphatics in the *phoca vitulina*; and what he could not effect by means of the lymphatics of the large intestine, he effected, perfectly, by those of the small intestine: then, after having filled all those of this intestine, and having pursued them to the *pancreas aselli*, he found some small *vasa efferentia*, by which the quick-silver had passed into the mesenteric vein. He repeated the injection in a second *phoca*, and found the metal pass into the same vein as in the foregoing experiment. On this occasion, a ligature was put on all the mesenteric vessels, at some distance beyond the large mesenteric glands; and it was ascertained that those glands, as well as the veins, were filled with the mercury. Dr. Fohmann has witnessed similar circumstances in the dog, cow, and horse; but, on publishing these observations, he had not yet had an opportunity of examining the state of the parts in man.†

A discovery of a nervous anastomosis, which, it seems probable, relates to some highly interesting law in physiology, but of which we at present have no conception, has lately been made by Dr. JACOBSEN.‡ It is one which connects the internal part of the ear with the tongue, and has, consequently, some analogy with the *chorda tympani*, which runs from the tympanum to the face. It is never wanting; presents hardly any variety; and is present in all the *mam-*

* In his *Ueber und Gegen den Neueren Empirismus in der Physiologie und Medicin*.

† *Salzburgh Medicinisch-Chirurgische Zeitung*, No. 46. 1820.

‡ *Deutsches Archiv für die Physiologie*. Fünfter band, zweites heft.

malia. In order that the precise distribution of this nerve may be clearly understood, it is necessary to adduce the following preliminary observations on the structure of some parts of the ear itself.

In the groove through which the chorda tympani runs in its course to the facial nerve, there are the openings of two canals, one of which is situate above the other. The inferior one is short, and terminates in the semi-canal destined for the internal muscle of the malleus. The superior is somewhat longer; traverses, in an oblique direction, the substance of the petrous portion of the temporal bone, and opens under the inferior extremity of the canal which WINSLOW termed the cochleiform apophysis. From this opening there extends a groove which in its commencement follows one of the margins of the foramen ovale, then passes over the promontory, arrives at the anterior margin of the foramen rotundum, and there is converted into a canal, which descends into the furrow by which the carotidean canal is separated from the fossa of the jugular vein, and it terminates by a small foveolous concavity between the two canals just mentioned. This little fovea seems to have been first discovered by COTUNNI, and ANDERSCH (who had observed, though imperfectly, the nervous anastomosis about to be described,) termed it the *receptaculum ganglium petrosus*.

This nervous anastomosis, now fully developed by Dr. Jacobsen, and which connects nerves so remote as the fifth pair, the glossopharyngeal, and the great sympathetic, is a ramification of the chorda tympani. According to the greater part of anatomists, this nerve is a single branch, which runs to join the facial nerve; but it is really composed of several filaments, the superior and largest of which is that generally known as the chorda tympani; whilst the middle and inferior ones appertain to the anastomosis under consideration. The middle one runs, through the superior of the two canals described above, into the cavity of the tympanum, where it is lodged in the groove of the promontory; there it receives the inferior filament, which arrives at the promontory by traversing a canal situate behind the inferior extremity of the Eustachian tube. Below this branch there is another nervous filament, which has originated from a branch of the sympathetic, and which penetrates the cavity of the tympanum by running along the membrane which lines the carotidean canal and that of the tympanum, and then, in the region of the promontory, unites into one trunk with the nerve previously described. The trunk thus formed, accompanied with a small artery, and covered by the membrane of the tympanum, descends into the promontory, giving off in its course two branches, the superior of which goes to the membrane about the foramen ovale, and the other appears to be distributed to the inner layer of the membrane of the tympanum. The trunk runs on, in the canal described above, to the fovea between the canals of the carotid artery and jugular vein, where it unites with the ganglion of the glossopharyngeal nerve.

Differences of opinion have been entertained, and still exist, respecting the vascularity of the crown of teeth; and some anatomists

have even described vessels shooting from the central pulp into the bony part of the tooth. JOHN HUNTER denied that such vessels had ever been seen; and Mr. WILSON says, that he "has injected the teeth of growing, and even of adult, animals frequently, so as to show the pulp to be very vascular; but never could trace vessels passing from it into the bone."* CUVIER, from observations on the germs of the teeth of elephants, says† that he is assured that vessels do not penetrate the bony substance; and he considers that this substance is formed by a sort of transudation of the ossific matter, which solidifies in successive layers, after the manner of the construction of shells, rather than by the ordinary mode of ossification. Balls have been found lodged and completely enveloped in the tusks of the elephant; but here Cuvier supposes that the balls had penetrated to the central pulp originally.

At the commencement of the last century, MONTESQUIEU lamented‡ the fate of the men of science of his age, in their existing at a period when it would appear that Nature presented no phenomena which had not been already observed, especially by those philosophers who lived in the preceding epoch, when so many brilliant discoveries were attained, that science may have been said to have then advanced at once from infancy to maturity: so that the few acquisitions in knowledge made by men in his time appeared to be merely indications of homage rendered to their predecessors, and such as might cause them to lament, with Alexander, that their fathers had effected every thing, and excluded them from the means of attaining any pretensions to glory. Yet the age of Montesquieu is now regarded as one in which science and philosophy made no small or unimportant advancement. But such have been, in all times, the reflections of men of eminent talents, who have had sufficient acuteness and severity of judgment to permit them to discern amongst the multitude of futilities which have been promulgated, the few facts which may present the bases of useful inferences, and which are qualified to become centres of new and important principles: men whose talents have enabled them to separate the host of hypotheses, in which every age is abundantly fertile, from the few theories which are really inductions from facts and the general expression of a multitude of individual phenomena. They have been desolated on perceiving the ascendancy of the former over the latter; and then, again, on comparing these—unappreciated as such acquisitions ordinarily are, for a very long period,—with the whole series of attainments of a preceding age, applied, as they have been, to many useful purposes, and contemplated with the distinctness peculiar to objects viewed at a certain degree of remoteness; they have been disposed to think, like Montesquieu, that the acquirements

* *Lectures on the Structure and Physiology of the Parts composing the Skeleton, &c. delivered before the Royal College of Surgeons of London in the year 1820.* By JAMES WILSON, F.R.S. Professor of Anatomy and Surgery to the College; Lecturer of Anatomy and Surgery at the Hunterian School in Great Windmill-street, &c. Published by Burgess and Hill, 1820.

† *Leçons d'Anatomie Comparée*, tome iii. p. 116.

‡ In his *Discours prononcée a la rentrée de l'Academie de Bourdeaux*.

of their contemporaries, compared with those of men of former times, were merely as rivers are to the ocean, which, though they bear along their contributions, do not perceptibly add to its magnitude; and which, indeed, often flow from sources not their own, but derived from the vast collection of ages which they would vainly pretend to augment. The greater number of the existing systems of science are, indeed, founded on but a very few elementary facts, which have, in many instances, appeared of but little importance until they have been well reflected on by men living in ages subsequent to that in which those facts were discovered; and this is a circumstance which will also contribute to explain why the attainments of our contemporaries often seem to be so devoid of interest. KEPLER had ascertained the relations between the revolutions of the celestial bodies and the distances of these from the centres of their revolutions: it had been inferred that some force prevented those bodies from pursuing longer than an instant a movement in a right line, and urged them towards the centres round which they revolve. It had also been inferred, that the force above alluded to, the *centripetal* (as it is termed), must have on the same body a diversity of influence according to the diversities of its distance from its centre, and *that* in the inverse ratio of the squares of those distances. All these things had been stated; but it was not until NEWTON made them the basis of his system of the universe that their whole importance was discerned. It had been remarked, also, that when the planets Jupiter and Saturn are in their greatest degree of relative proximity, their movements have not the same regularity as those evinced in the rest of their course; but this fact was merely a barren wonder until it was explained by the principles of the same philosopher; on which it, in return, reflects a ray that is regarded as one of the most brilliant of those by which the same principles are illustrated.

Those things were not, perhaps, sufficiently regarded by one of the most erudite and able physicians of the present age, when he contemplated the work* in which he endeavours to prove that physiology, pathology, and therapeutics,† have made no progress during the last century. All the new and eulogized hypotheses and proffered theories,—especially those relating to the nature and laws of irritability; the nervous influence, and its identity with galvanism; animal heat; chemical physiology; and absorption,—are examined by the author of the above proposition, and the errors and bad reasonings they involve are pointed out. He has attempted to show that what is good in the modern doctrines was known in former times; and the truth of his statements is combatted for with such acute and profound arguments, illustrated by choice results of a rare extent of erudition, that they, in the first instance, shake our confidence in the reality of the recent

* *Ueber und Gegen den Neueren Empirismus in der Physiologie und Medicin.* Von Dr. CHRIST. FRIEDR. HARLESS, Ritter, Königl. Geheimer Rath, Professor zu Bonn, u. w. s. 8vo. Bonn, 1820.

† Not including *chirurgery*, "which," he says, "beyond the influence of sectarianism and the spirit of the schools, pursues its course steadily and uninterruptedly towards the perfection of the art."

progress and improvement of those sciences. But, on a little reflection, most men will, probably, think that the proposition of Professor Harless is not valid in its full extent; for, that, although no new and satisfactory *general principles or theory*, of the several subjects above enumerated, may have been produced, many very important original observations have been made within the period above designated, and several new inferences have been drawn from facts already known, which may serve to contribute, in future times, to the construction of theories. Professor Harless, it should also be remarked, directs his attention principally to the productions of modern *experimentalists* in physiology, who, for the most part, have been just the men the least qualified to advance the progress of science, from their wanting the intellectual talents requisite to enable them to reason well on what they may observe, even if experiments undertaken without any clear views, and prosecuted without sufficient accuracy, should happen to furnish useful and original results. It is certain, at least, that many errors, some almost primordial in physiology and pathology, others of later introduction (the refutation of which is hardly of less importance than the original discovery of truths), have been obviated by late observations and reasonings: and, though most of the remedies for disease now employed may have been used in ancient times, it can hardly be doubted that the degree of precision with which they are applied is daily increasing. Most of the diseases arising from the mineral and vegetable poisons, as one instance, were but ill understood and worse treated, until a very late period. No slight advantages have, also, resulted from the knowledge which has of late been acquired (by means of pathological anatomy) respecting the organic lesions consequent on morbid actions, and the reciprocal influence of such lesions on the rest of the animal economy. A particular consideration of the disquisition of Dr. Harless does not, however, come within the views which are embraced by this Sketch; though it would not have been right to pass it by without some degree of notice.

Perhaps a more striking exemplification of the difficulties which attend the acquisition of physiological knowledge cannot be adduced, than that presented by the existing information respecting the mechanism of the circulation of the blood. The course taken by the principal part of that fluid is known; but the means by which it is propelled are by no means accurately understood. Indeed, with the exception of Dr. CARSON,* (who possesses, with the knowledge of the more essential parts of medicine, that of a science—geometry—which no physician should be deficient in, and which is especially necessary in the investigation of this subject,) no one has fully and earnestly engaged in the inquiry. Other physiologists have either only perceived, or have chosen to confine their researches to, a few of the more obvious difficulties of the subject,—as the share of motive influence performed by the arteries, and the question of the automatic dilatation of the ventricles of the heart;—and even these points have not been settled in a satisfactory manner. Dr. Carson continues to

* In his *Inquiry into the Causes of the Motion of the Blood*. 1815.

pursue the investigation of this subject, and has lately published a memoir* in which he adduces some further arguments in favour of his doctrine of the influence of elasticity of the lungs on some phenomena of the circulation. It is necessary to transcribe his statements in detail, in order that they may be understood.

“ The Harveian doctrine of the circulation of the blood may, I think, be divided into two parts. The first is the course of the blood; the second, the explanation of the causes by which it is moved in that course. The arguments advanced by Dr. Harvey on the first of these points, the course of the blood, must, I think, convince every candid inquirer that the blood is conveyed from the heart by the arteries, and returned to it again by the veins. But the illustrious discoverer has not been so fortunate in the second part of his great undertaking. In maintaining that the projectile power of the ventricles of the heart propels the blood through the whole of the arterial and venous canals, and, after having discharged this office, opens the auricular chambers by means of the returned blood, he lays claim to effects which are not warranted from the supposed causes, and which are inconsistent with the established laws of hydrostatics,—laws by which the blood, as well as every other fluid, must be governed. But this part of the doctrine of Harvey, admitting it to be philosophically correct, must be rejected in its present application, as affording no satisfactory solution of the phenomena. The followers of Harvey, adopting as the foundation of their arguments his doctrine of a *vis a tergo*, have enlisted the arteries into the aid of the heart, and contend that the blood is circulated by the combined agency of these powers: but the difficulty is not removed by the supposition; it is only shifted to another part of the system, and the phenomena are not better explained.

“ It has often created surprise, that a doctrine so simple in appearance as the circulation of the blood, and pointed out so plainly as we now suppose by facts of daily occurrence, should have been reserved for the discovery of modern times. The knowledge of the circulation seems to have been retarded by one remarkable phenomenon. The arteries, which are now known to constitute the channel of the blood for one-half of its course, were uniformly found to be devoid of that fluid after death. That vessels in which no blood was to be found by the most careful examination after death, should be the constant receptacles of it during life, is a supposition that would scarcely suggest itself to the anatomist; and, if suggested, would soon be rejected from the list of probable conjectures. The arteries were supposed to be the recipients of a vital aerial fluid. One fact, indeed, of frequent occurrence, seemed to be at variance with this belief; and, if the effect of it had not been defeated by an hypothesis, it must, we would suppose, have led to the truth. An artery, when wounded, was constantly observed to discharge blood from the living frame. But the ancient physiologists, unwilling perhaps to degrade the arteries from what they conceived to be their more refined office, and conceiving it impossible that, if these vessels contained blood during life, they

* In the xith vol. of the *Medico-Chirurgical Transactions*.

could be deprived of it by death, contended that the discharge of blood from a wounded artery was no proof that the vessel contained any blood before it was wounded, but that the injury and pain given by the wound drew blood from other quarters into vessels which contained none before; and that the impetuosity and obstinacy of the discharge arose from the conflict between this foreign intruder and the native aerial spirit. What gave greater plausibility to this supposition, was that the blood shed by the arteries, from the veins, seemed to be not the natural product of the body, but the factitious result of this imagined conflict.

“ The condition of the arteries after death was urged confidently by the opposers of Harvey as an insuperable objection to the doctrine of the circulation, upon its first promulgation; and was unquestionably one of the greatest obstacles found in the path of the discoverer: and, after all, the explanation given by this celebrated man of the powers by the operation of which the arteries are found empty after death, is most unsatisfactory. He says, that the left ventricle, in the last struggles of life, continues to propel after it has ceased to receive blood, and that, by these final propulsions, the blood at the time in the arteries is driven into the veins; and he further asserts, in support of this explanation, that the arteries of animals, who have been killed by submersion in cold water or by mephitic air, will be found to contain blood after death as well as the veins. But there can be nothing more evident than that the heart, by these abortive impulses, could only drive blood through the more remote portions of the arterial system by some impinged medium; and that this medium, which, upon the hypothesis of Dr. Harvey, could only be blood, must still remain in the arteries. But, defective as this explanation is, it has the further imperfection of being built upon an hypothesis that is altogether destitute of proof. This hypothesis is, that the heart continues to propel after it has ceased to receive blood. The heart, on the contrary, is generally found full of blood after death. The converse, therefore, of what is maintained by Dr. Harvey would appear to be the truth,—that the heart retained the capacity of receiving blood after it had lost the power of discharging it. The statement, also, by which Dr. Harvey has supported his opinion, has not been confirmed by observation. The arteries of animals which have been killed suddenly by submersion in cold water, or by any of the ways enumerated by Dr. Harvey, have been found equally empty of blood with those of animals killed by lingering disease. Into such a labyrinth of error and evasion will men of the most powerful minds be led, when they attempt to shape nature to a conformity with their opinions.

“ No new light has been thrown upon this subject by the followers of Harvey; and in general it may be observed, that, after the lapse of two centuries, and though a thousand volumes have been written, and thousands of animals slaughtered to elucidate the subject, the doctrine of the circulation has descended to our times nearly in the same state in which it came from the hands of the original discoverer.

“ Mr. George Ker, an ingenious and learned surgeon in Aberdeen, struck with the defectiveness and inapplicability of the explanation

given by Dr. Harvey and his followers of this remarkable phenomenon, as well as of many others, has, in a late publication, boldly denied the doctrine of the circulation of the blood altogether; and become the acute, strenuous, and most confident advocate of the opinions of the ancient physiologists respecting the condition of the blood in the living system, and the uses of the arteries.

“All the objections urged by Mr. Ker against the circulation had been stated by myself as objections, not against the doctrine of the circulation itself, which I believe to be founded on a basis never to be shaken, but against the causes assigned for the accomplishment of that effect, at least two years before the appearance of Mr. Ker's work, but unquestionably without the knowledge of that gentleman. The causes which I have ventured to assign to the motion of the blood, will, in process of time, I trust, be found to have a real existence in nature,—to be adequate to the effects assigned to them,—to have been fitly applied,—to afford a plain and satisfactory explication of the various phenomena,—to answer fully all Mr. Ker's objections,—and, in a word, to vindicate a theory, which does so much honour to our country, from all future opposition.

“The objection principally dwelt upon by Mr. Ker,—the emptiness of the arteries after death, did not pass unnoticed, as may be seen on reference to the inquiry into the causes of the motion of the blood: but, as I had not then had an opportunity of submitting my opinions to the test of experiment, I did not state them with that confidence which I even then felt in their truth. I have lately had that opportunity, and I now propose to state the result.

“The chief, if not the whole, of the movements of the animal machine, seem to be the effect of two powers acting either conjunctly or separately. These are elasticity and irritability. The elasticity of the parts which possess this property is inherent in the structure, and is independent of life. Irritability, which is the property of the muscular substance, is the concomitant of life, and ceases with it. The movements, which are the usual result of a combination of those powers, will not wholly cease at death. The elasticity will still continue to operate; and the result will be different, either from that which would be produced by their combined agency, or from that which would arise from their synchronous destruction.

“The motion of the blood seems to be the result of the contractions arising from the irritability of the heart and arteries, and of the resilience arising from the elasticity of the arteries and of the lungs. One class only of these powers is destroyed by death. The resilience of the lungs and of the coats of the arteries possess, then, an uncontrolled operation.

“The resilience of the lungs removes a part of the pressure of the atmosphere from the internal surface of the chest, and perhaps from the internal surface of the vessels by which they are penetrated. To restore to the parts within the chest an equality of pressure with that of the substances without it, the adjoining liquid and less-fixed parts of the body will be pressed through every channel that offers into the chest. What is called a vacuum will, in effect, be made in the chest

by the elasticity of the lungs. There will therefore be a draining from all parts of the body towards the chest, to fill up this vacuum. As thus the causes which return the blood to the heart continue to operate after the heart, the great engine by which it is discharged, has terminated its labours, a greater quantity of blood will necessarily be collected in the neighbourhood of the heart after death than existed there before it.

“ Various circumstances may intervene to fix the channels in which the blood will flow in its course towards the heart after death. The arteries are powerfully elastic, and, when their coats are relieved from the distending force of the heart, become of a diminished calibre. Valves stationed at the roots of the arteries prevent the return of blood from these vessels into the chest. After the small part of the aortic system intervening between the heart and the confines of the chest shall have been, as it usually is found to be, filled with blood, the blood in the rest of the system will sustain no diminution of pressure on the side of the heart.

“ No obstacle exists in the way of the blood in its course to the chest through the veins. No valves are stationed at the roots of those vessels, and the blood finds an unobstructed course from the roots of the cava into the auricle; from that, possibly, into the right ventricle and into the pulmonary arteries, and thence into the pulmonary veins. The heart, particularly the auricles and the large venous trunks, the coats of which being inelastic and easily dilatable, being all placed within what may be called the vacuum of the chest, will be distended to their utmost capacity. The additional blood requisite for this purpose can be drawn only from the veins. The place of the blood taken from one part of the venous system, will be supplied by that from another. The termination of this process will be the emptying of the arteries into the veins.

“ If the preceding argument be correct and founded upon true principles, it would follow that—were the elastic powers employed in the motion of the blood disengaged before the muscular powers had ceased to act, or synchronously with that event,—a distribution of the blood would be found to exist after death different from that which is now usually observed. The blood would not be found so extraordinarily accumulated in the right auricle, and in all the veins belonging to the system of the cava within the chest and at the approaches to it; and the arteries and capillary vessels would contain the proportion which, upon the supposition of the Harveian theory, must have flowed in them before death. I have not been able to devise any method of annihilating, before death, the elastic influence of the arteries, and therefore some allowance must be made in the phenomena which are to be brought into view for that cause; but I have been successful in removing from all influence after death the elastic power of the lungs, by far the most efficient, by the manner in which the animals were killed in the following experiments. Death was in these cases effected by inducing a previous collapse of the lungs, which was done by making openings into the chest of the living animal, and exposing the external surface of the lungs to the free access of the air.

“ In the first experiment made with this intention, an opening was made about an inch in length between a pair of the ribs on each side. I expected that sudden death would be the effect of these openings ; but in this respect I was disappointed, and at first not a little perplexed. This disappointment I experienced particularly in the case of a large dog. This animal, as I supposed, after the collapse of the lungs, by pressing up the diaphragm by means of the abdominal muscles as far as possible, and then, by a rapid and forcible contraction of the intercostal muscles, accompanied by a rapid and forcible contraction of the diaphragm, was enabled to rarefy the air contained between the external surface of the lungs and the chest to such a degree as to occasion a partial dilatation of the lungs, and an imperfect expansion of the heart. Thus life was painfully prolonged for nearly twenty minutes. The sufferings sustained by this animal for so long a period prevented a repetition of the experiment on any other animal in the same manner. The result was in other respects satisfactory. Though the death was tedious, it was ultimately produced by the collapse of the lungs. I had previously performed the same experiment upon a rabbit and a cat. In these the death, though not sudden, was neither so tedious, nor to appearance so distressing, as in the case of the dog.

“ The same appearances were, on dissection, exhibited by all. The muscles were remarkably red ; and, when an incision was made into them, they poured out blood. The membranous parts exhibited the blood-vessels as if they had been fully and nicely injected ; forming anastomoses, which appeared like a netting made of red threads, I was particularly struck with the coats of the intestines : instead of exhibiting the usual pale smooth surface without the vestige almost of a single blood-vessel, they appeared to be composed of a red-coloured netting, the meshes of which varied greatly both in dimension and in form. The liver was like red morocco. The flesh of the rabbit, which is usually white, was in this case of a reddish colour ; and all the dissected parts became wet with effused blood. The heart contained little blood. When the chest was opened, and the large vessels it contains were divided, a small quantity of blood only was effused ; not much more, indeed, than from the other parts of the body. The aorta and large arteries, in all the instances, were pale externally ; while the accompanying veins were of a blue colour. A part of the descending aorta, above the bifurcation of the iliacs, after its extremities had been secured by ligatures, was cut out, and was found to contain a small cylinder of blood generally coagulated. So it appears that the white colour of the arteries did not arise from this being empty of blood, but from the want of transparency of their coats. With respect to the vessels which the stomach, the intestines, and the membranous parts, exhibited in so beautiful a manner, I do not pretend to say what part of them may have been arteries. Supposing, however, that these carcasses exhibited the distribution of the blood as it really existed in life, it is very evident that the blood, not only of the larger arteries, but of the smaller vessels, whether they be arteries or veins, must, in consequence of death produced in the usual way, be emptied into the large veins. I think it probable,

however, that what are called the capillary vessels may, in consequence of this mode of killing the animal, be found to contain more blood than the share that belonged to them during life; for the elasticity of the coats of the arteries, the effect of which, as I before stated, I had not been able to devise any plan of counteracting, by contracting the bore of these vessels, would propel a part of the blood that was flowing in them at the moment of death into the vessels, the coats of which were inelastic and dilatable.

“ For the purpose of comparing the appearances of two animals of the same kind, killed in different ways, two rabbits were killed on the 20th of September, 1819; one of them by causing the lungs to collapse before death, the other after a different manner. In the case of the first of these rabbits, the belly was opened freely from the scrobiculus cordis nearly to the pelvis, and the lower surface of the diaphragm exposed to view. An opening, fit to admit my two fingers, was made through the muscular part of the diaphragm on each side. The sound of air rushing through the orifices announced the collapse of the lungs. As the animal possessed no power of contracting the openings made in the diaphragm; as its struggles would probably tend to widen them still more; and as, therefore, the capacity of dilating the lungs, even in the smallest degree, no longer remained, the animal instantly died. The appearances exhibited by the dissection of this rabbit were precisely similar to those already described of the bodies of animals killed by the previous collapse of the lungs. The vessels, particularly of the intestines, stomach, and mesentery, were very distinct and full of blood, forming frequent anastomoses with each other, in the way already described. The flesh was reddish, and, when cut into, bled. The heart, and vessels about it, contained only a moderate quantity of blood; for scarcely any blood was found, after the division of the vessels, to have been effused into the shell of the chest. The other rabbit was killed by thrusting a sharp instrument between the vertebrae of the neck: it died instantly, and was immediately opened. Scarcely was the vestige of a blood-vessel to be observed on the surface of the intestines or stomach, which had a pale appearance, excepting where they were tinged by the colour of their contents. The membranes scarcely exhibited any traces of vascularity. The flesh was white, and, when cut into, appeared to be dry, discharging at some parts a drop or two of blood. The liver was of a dusky-brown colour. The trunks of the veins were swollen and rounded: whereas in the other rabbit they appeared flat, and to contain a thin layer of blood. A considerable quantity of blood was found in the shell of the chest, after it had been opened and the large vessels it contains divided.

“ A few days after, a sheep was killed in the same manner as the first of the above-mentioned rabbits. When the openings were made through the diaphragm, the sound of air rushing into the chest announced still more plainly the fatal collapse of the lungs, and the last expiration. The animal, after making a few heavings with the chest, became lifeless. Several other sheep had been killed at the same place at that time, and there was an opportunity of comparing the carcase of this animal with that of the others. Scarcely any traces of the

smaller vessels were observable in the stomach, intestines, peritoneum, or mesentery, of the other sheep; while, in the same parts of this animal, they appeared in great abundance, and as injected with red wax. The appearance was so remarkable as to strike the butchers, and the persons whose attention I directed to it, with surprise. The colour of the fat was browner than usual. The muscles of this animal, being at all times red, did not exhibit so marked a difference as in the case of the rabbits, but, when cut into, they discharged blood. The larger arteries, where I had an opportunity of seeing them, contained a small cylinder of coagulated blood. The flat and tape-like appearance of the small veins, which I had observed in the rabbit killed in the same manner, was in this case very remarkable.

"The results of these experiments, I think, fully warrant the conclusion, that the difference of the distribution of the blood after death from that in which, according to the Harveian theory, it must exist in the living system, arises chiefly from the elastic power of the lungs; and that the emptiness of the arteries and of the smaller vessels, observed after death, admits of a satisfactory explanation from the supposed operation of this cause, combined with that of the elasticity of the arterial canals."

Some interesting additions have been made to our knowledge of the influence of respiration on the circulation of the blood, by a young French student, Mr. BOURDON.* The object of his enquiries was to determine the cause of the redness of the face; and other signs of distention of the capillary vessels, during expiration. The results of Mr. Bourdon's experiments are congruous with the views of Dr. Carson already designated. It may be right to state that HALLER explained the phenomenon above mentioned, by the supposition that the vessels of the lungs are, in a manner, folded-up during expiration. GOODWIN, from considering that the lungs are always considerably distended with air, would not admit that any obstruction to the circulation existed in this state: and BICHAT went further, and said that, even though the lungs contained no air, there would be no such obstruction in them.

Mr. Bourdon, after stating, what cannot be disputed, that this redness of the face, &c. happens during expiration, proceeds to show that a temporary obstruction to the passage of the blood through the lungs takes place during violent efforts; which obstruction, he thinks, depends on the pressure suffered by the pulmonary vessels on the one side by means of the retained air, and on the other by the abdominal muscles. He remarks, that the jet of blood from an opening in a vein is increased in force and rapidity during coughing, vomiting, &c. The vena cava of a dog laid bare, was distended during the cries and struggles of the animal. Cases of rupture of the vena cava have happened during violent efforts; and an instance was related a few months ago, in the *Journal Complémentaire*, &c. of this rupture taking place

* *Récherches sur le Mécanisme de la Respiration et sur la Circulation du Sang.* 8vo. Paris, 1820.

during a violent cough, caused by the presence of a foreign body in the trachea. We commonly observe the jugular and other veins distended under similar circumstances. The arterial system suffers, of course, differently from the venous in the condition under consideration. During the first instants of the efforts, more blood than ordinary flows to the left side of the heart, from its being suddenly driven from the lungs with unusual violence; and hence rupture of arteries has occurred during the first moments of violent efforts: but after this period the large arteries have less blood than ordinary flow through them, in proportion as it is accumulated in the venous system. It is hardly necessary to state, that the capillary arteries will become distended in efforts of a certain duration: this must necessarily ensue from the obstruction in the venous system.

Some other experiments were made by Mr. Bourdon, with the object of ascertaining whether or not he could, if he chose, voluntarily produce death, by the action of his respiratory organs. That he might not give occasion to a renewal of the story of the golden tooth, he takes care to show, by references to history, that it is probable that death has been produced in this way. He first made a long expiration; he then employed all his efforts to resist the call for inspiration. At the end of thirty seconds this call had become very urgent, and at the end of fifty seconds it was irresistible.

He tried another way. He closed the glottis after a slight inspiration, and without contracting the abdominal muscles: he then tried to prevent the escape of the inspired air, and to avoid the introduction of more into the lungs. At the end of a minute the anxiety was extreme; and shortly after this the glottis opened in spite of him, the air contained in the lungs was expelled, and new inspirations then executed.

There remained another mode of effecting the same object. He made a deep inspiration, then closed the glottis, and contracted the abdominal muscles, using considerable effort. He carefully avoided temporary relaxation of the muscles. He had some of his friends near him, who were to stop him when they judged it prudent. At the end of six seconds, his face was red and tumid; after twelve seconds, he experienced slight vertigo; after fifteen seconds, the vertigo had increased, and his face was of a violet colour: he saw objects as it were through a cloud; he heard the words addressed to him only in a confused manner. His friends pinched him violently, to make him stop, but he hardly felt any pain: he was on the point of losing his consciousness, when his effort ceased. He doubts whether a stronger person may not be able to produce death in this way; though he thinks that no person can properly asphyxiate themselves (that is, produce death) by either of the two former methods: and he makes a question of the manner in which death would take place in the case of its being effected by means of the last experiment,—whether by apoplexy, syncope, or a sort of asphyxia.

The exact state of the respiratory organs during violent muscular efforts of the body, more or less generally, does not appear to have

been correctly understood previously to the light thrown on the subject by some other experiments of the physiologist just mentioned. It has generally been supposed that the diaphragm is in a state of contraction during great voluntary efforts; a notion, of which the truth might have been readily doubted, if it had been considered that the glottis is closed on such occasions. This fact, however,—that is to say, the closure of the glottis,—has not been generally known; though SENAC had stated this connection of action to take place when a person supports a great burthen on the chest; BARTHEZ, in the act of swimming; and BICHAT, in lifting or moving heavy masses. HALLER, we may add, went further, and showed, by an experiment, that the effort evaporated as it were with the escape of air from the glottis. GALEN, also, whose writings contain so many admirable physiological observations, several of which have been brought forth in our own time as original, evidently remarked the relation in question; and he has clearly pointed out its influence on several muscular actions. (See his *Treatise de Utilitate Respirationis*.) But, we must acknowledge with Mr. Bourdon, that the isolated facts of this kind noticed by former authors have been but obscurely seen, and have remained fruitless up to the present day. That the glottis is capable of being closed, both by the falling of the epiglottis on it, and by the coaptation of the vocal cords and the arytenoid cartilages, has been proved by MAGENDIE; and this physiologist has also shown that a certain degree of contraction of it in the latter way, takes place when an animal utters acute cries: it is the object of Mr. Bourdon to designate the relations of this contraction with various other phenomena besides those already mentioned. He first endeavours to account for the neglect of the fact in question, or the want of any important inferences from it, from the confusion that has been thrown around it by the erroneous notions which have hitherto been entertained respecting the action of the diaphragm during certain violent muscular exertions. It has been supposed that the diaphragm contracts at the same time with the abdominal muscles during such efforts. Here contradictions seem to be involved; for the result of contraction of the diaphragm is *inspiration*, and how can inspiration take place if the glottis be closed? There is, besides, an evident want of object in the contraction of the diaphragm in this case; for, the glottis being closed, the air is retained in the chest, which is thereby rendered immovable, and its firmness cannot be increased by the contraction of the diaphragm. Mr. Bourdon has shown, in a satisfactory manner, that the diaphragm is in a state of relaxation at the time the abdominal muscles are contracted in making great muscular exertions: but we shall follow him in the order in which he has stated his observations; and advance, as he has done, the proofs of this assertion hereafter.

Mr. Bourdon was struck, in the first instance, with the prompt and easy manner in which respiration is suspended on making any vigorous muscular effort: that is to say, whenever the action of the abdominal muscles is about to be employed for effecting other results than the expulsion of air from the chest; whether in vigorous exercises of the body, such as leaping, running, wrestling, swimming, &c. or in the

various expulsive efforts, as vomiting, parturition, the evacuation of the feces, &c. On examining, individually, the organs of respiration, he felt assured, that the diaphragm, by its contraction,—the glottis, by its closure,—and the veil of the palate, by its elevation,—could alone be capable of retaining the air in the chest, and of suspending, in consequence, the succession of the phenomena of respiration; and he has satisfactorily proved that it is by the closure of the glottis that this is effected.

Reasoning from its organization, he says, we should conclude that the glottis is susceptible of being exactly closed. During a great muscular effort, the larynx is raised in a certain degree; and when we at this time, alternately, in quick succession, admit the air into the lungs and partially expel it, a sound, somewhat like a *low cough*, is distinctly heard, and which evidently comes from the situation of the glottis. After making these and some other analogous observations, he resorted to some experiments on dogs, with a view to attain further evidence. The act of vomiting is that which is most readily produced in these animals, and this is excited without any cries, which are always accompanied with some degree of contraction of the glottis, and therefore would render any observations devoid of precision, with the view in question, if they occurred. Having exposed the glottis of a dog, and administered a strong emetic potion, he observed, (shortly after the appearance of the signs of nausea, as spasmodic motions of the lips, the tongue, jaws, and pharynx,) in the most distinct manner, the glottis become exactly closed at the instant when the abdominal muscles were forcibly contracted, as vomiting took place. He was assured that the epiglottis took no part in this closure. He proved the same thing in himself: after having attained the power of passing his forefinger down to the glottis, without producing the efforts to vomit which such an attempt causes in persons in whom those parts have not been habituated to the contact of such a foreign body. Mr. Bourdon says he can now pass his finger into his glottis, so as to distinguish easily the summits of the arytenoid cartilages and the folds of the mucous membrane of the larynx. When he had practised the attempts to effect this for sixty-five days, he once, on having introduced his finger more incautiously than usual, arrived suddenly at the glottis. He immediately experienced intense nausea; at the same instant, the abdominal muscles contracted, and the glottis closed: in a word, there was a real effort to vomit, though this effect did not take place. The passing of his finger to the glottis causes now only slight nausea, which can be easily withstood: he has profited by this disposition to ascertain the state of the glottis in the muscular efforts we are in the habit of executing, either for the more extensive movements of the body, or the expulsion of the feces; and he has become assured that the mechanism is similar to that of vomiting: in all the glottis is closed at the same time that the abdominal muscles enter into contraction. He observed, further, that the glottis was contracted by means of the meeting of the vocal cords and the arytenoid cartilages; since he distinctly felt the latter approach each other under his finger. He was certain that the epiglottis had nothing to do with the closing of the

glottis, because he could keep it pressed down on the base of the tongue, without preventing that effect from taking place.

This *consensus* or *synergie*, (as Mr. Bourdon calls this relation of action, after Barthez; and the term is certainly, in this case, more appropriate than *sympathy* :) between the glottis and the abdominal muscles, is observed in many familiar actions of the human body; and its importance in respect to those actions will be obvious on a little consideration.

In a man who has no glottis, or whose trachea has an opening in it, powerful muscular motions, various habitual efforts, and especially the expulsion of the excrements when requiring much exertion, cannot be effected, unless the preternatural opening be closed on the occasion.* Several species of animals furnish evidence in support of the views just exposed. Wherever the glottis is wanting, the expulsion of matters from the body is rendered comparatively easy by some mechanism or other: either by the liquidity of the matters to be expelled, or by the absence or weakness of the sphincter muscles, or by the presence of a cloaca in which the liquid excrements are mingled with the solid. Thus, the resistances are naturally weakened where it is not possible for any great expulsive power to be developed. In those animals which possess neither lungs nor air-cells, and consequently no glottis, all the expulsions are easily made, because all the sphincters are weak, and the matters are more or less liquid. Reptiles, which have a glottis, but an imperfect one, and birds, in whom this organ is more perfect, and whose principal sphincter is also more strongly developed, form, in these respects, as well as in many others, the natural link between fish and the mammalia. Amongst the latter there are several species, as the marsupia, and especially the kangaroo, which have only rudiments of the glottis, and this disposition is accompanied with a singular structure of their principal expulsatory organs. Their uterus, bifurcated and folded, has no neck, like that of the greater part of the mammalia. From this reason, the product of conception cannot remain in the uterus during the time necessary for the full growth of the fetus, and it is expelled at an earlier epoch, and received into a pouch situate under the belly, which envelops the mammæ, to which the young one, still imperfectly formed, attaches itself. Although the neck of the uterus does not exist where there is no glottis, the inverse is not true, that the glottis does not exist where there is no neck of the uterus; for the glottis has other offices to perform than that of contributing to parturition.

In order to render the facts above stated more clear, and the conclusions that may be drawn from them of a more positive and general character, Mr. Bourdon relates the results of some experiments which he made in conformity with the indications the observations above detailed presented to him. He passed into his larynx a small canula of elastic gum: he now found it not only impossible for him to produce any voice, cry out, or cough, but he also was unable to make any expul-

* See a case of tracheal fistula related by J. L. PETIT, in the *Mémoires de l'Académie de Chirurgie*.

sive efforts. He tried in vain, several times, to suspend his respiration and make those efforts: as soon as the abdominal muscles contracted, the ribs were depressed, the air was evacuated from the lungs, and the viscera were hardly at all compressed: the whole effort of the abdominal muscles, and of the extensors of the trunk, vanished in producing expiration. He then stopped the canula after having made a deep inspiration, and he could, as ordinarily, suspend his respiration, compress the viscera, cause redness of his face, and, in a word, produce all the phenomena of the efforts under consideration. He again left the canula open, and again he was unable to produce them. He thinks, then, that the following conclusions may be drawn from the premises:

"1. That it is the glottis which suspends respiration during considerable efforts, in opposing, by its closure, the escape of the air contained in the lungs.

"2. Without the glottis, the action of the abdominal muscles would be constantly employed in producing expiration: neither compression of the viscera nor flexure of the trunk could be produced.

"3. There exists a real *consensus* of action between the glottis and the abdominal muscles; and, through this medium, between the glottis and the different reservoirs,—the bladder, the rectum, the stomach, the uterus.

"4. The glottis does not confine its office to the production of the voice; but, by the aid of the sympathetic connections which unite it with the abdominal muscles, charged to concur in, if not to preside over, important functions, it exerts the greatest influence on those functions themselves.

"5. Lastly, in the different efforts there is a tendency to expiration, to the production of which the closure of the glottis is an obstacle.

"Now, to establish that there is a tendency to expiration during efforts, and that respiration can be suspended only by the aid of the closure of the glottis, is it not doubly to prove that the diaphragm is not then in action?"

Although, Mr. Bourdon remarks, the facts already stated may, perhaps, be considered sufficient to prove this passive state of the diaphragm, yet, as the opposite opinion has hitherto been entertained by physiologists, it may be prudent to enter into some further considerations, in order to establish this proposition.

"The certainty," he says, "we have that the abdominal muscles act during efforts, furnishes one proof of the passive state of the diaphragm in these circumstances; for, to admit as possible the combined action of these organs, is to conceive that inspiration and expiration may be simultaneous, which is evidently impossible.

"2. If the diaphragm were active during efforts, it is during inspiration, as Bichat believed, that these efforts would be produced: now this is not what takes place. There is always, during efforts, either incomplete expiration, or only a tendency to expiration, conjunctures where the diaphragm is necessarily passive.

"3. When the diaphragm contracts, it descends towards the abdomen, determining the expansion of the lungs, into which the external air is transmitted. I shall, then, have proved that this organ is passive

in these efforts, if I establish that the lungs are then compressed, and that the air attempts to escape from them."

To establish this last proposition, he states the facts that hernia of the lungs form and increase during efforts; that, when there is fluid in the cavity of the pleura, and which opens externally, the fluid escapes from this cavity with greatest rapidity during coughing, and when respiration is suspended. It is under the same conditions, too, that rupture of the lungs and hemoptysis, and emphysema in the external cellular tissue, are produced; that the necks of animals, as frogs and some apes, which have here extensive membraniform productions from the larynx, are tumefied; and, lastly, respiration cannot be suspended when there is a fistula in the larynx or trachea, because, as soon as the abdominal muscles contract, the air passes out of the fistulous opening. Now this, and the other facts just stated, could not take place if the diaphragm were then contracted.

There is a case, however, in which the diaphragm may actively participate in efforts, or when these efforts may be made during inspiration: it is when the abdominal muscles contract at the very instant when inspiration begins. But this never takes place under ordinary circumstances; it is only produced in experiments: and it must have been from such cases that ARALDI of Milan drew his conclusions when he asserted that expulsions were always effected during inspiration.

Having thus advanced his propositions, and, as he believes, proved them, Mr. Bourdon enters into a regular discussion of the phenomena which take place in efforts in general, for the purpose of showing, in a more lucid and connected point of view, the parts performed by the abdominal muscles, the glottis, and the diaphragm, in those efforts. Our limits will not let us go into these details; and we trust that our abstract is sufficiently perspicuous to enable the reader to perceive, on a little reflection, all the most important of the circumstances just alluded to.

We may remark here, that though the glottis is stated above to be the *point d'appui* for the abdominal muscles during efforts, yet that this *point d'appui* may be, and really is, commonly transferred to a more remote extremity of the aërial tube in some animals, whose glottis is but imperfectly formed for the purpose in question. It is sometimes transferred to the mouth and the nostrils. This is done by several swimming mammalia,—as, for example, by the *phoca*: his cheeks are pushed out and distended as he strikes his limbs. A man who swims but indifferently, or who is unpractised in the art, does the same thing: he draws up the *velum palati*, which closes the posterior passage to the nostrils, closes his mouth firmly, and then his cheeks are pushed out as he strikes his limbs. Many bats have the power of closing the *alæ nasi* firmly, and form, when flying, the necessary *point d'appui* in the same way.

Some of the experiments made by Mr. Bourdon, with the view to determine precisely the importance of the closure of the glottis in several efforts, are too interesting, however, to permit us to neglect to give some account of them. He made an opening into the trachea of a dog, and fixed in it a firm canula: an emetic was then administered. The

animal made twelve or fifteen efforts to vomit, but without success: it was in vain that he attempted to retain his breath; the air was always rapidly expelled through the canula as soon as inspiration had terminated. The efforts to vomit seemed to be violent, and the anxiety of the animal extreme; but at length the efforts became extremely violent, respiration was short and suddenly interrupted, the whole body was in a state of tremor, and he evacuated, at repeated times, the contents of the stomach, always during expiration. This experiment was repeated several times on other dogs, and always with the same results. Mr. Bourdon then concludes, "that the abdominal muscles powerfully contribute to produce the sort of vomiting which is effected in a rapid manner; but that, deprived of this aid, the stomach seems capable of evacuating, by its own action, the whole of what is contained in its cavity." By some other observations, he has been led to believe that the action of the abdominal muscles, without contraction of the stomach itself, produces rapid vomiting, but never completely empties the stomach.

Mr. Bourdon took a dog who could swim well, and introduced a very long canula into an opening made in his trachea; the end of the canula fixed here was provided with a piece of leather, so as to prevent any water passing in by the wound. He put the animal into the middle of a large pond of water: the dog went to the bottom, notwithstanding all the efforts he made to swim; and he was obliged to walk along the bottom of the pond to get to the shore. The canula was sufficiently long to reach above the water,

He got a dog who could easily leap across a ditch about four feet wide. He laid bare the trachea, without making an opening into it; the dog sprang across the ditch as lightly as before the operation, to obey his master, who called him from the opposite side. Mr. Bourdon then opened the trachea, and placed an open canula in the wound: he then left the animal to himself; and, in his impatience to obey the voice of his master, still on the opposite side of the ditch, he exerted his efforts to leap it, as usual; but this time he fell into the middle of it, instead of clearing it, as he had always easily done before.

Although an interesting point in the physiology of the eye, it had not, hitherto, been ascertained whether or not the pupil is enlarged or contracted in its diameter, in relation to a greater or less degree of remoteness of the object contemplated, independent of any diversity in the intensity of the light reflected from the object on the eye. The following experiment, lately made by Dr. DUNGLISSON, presents, however, results which seem to admit of some inferences qualified to explain the question above indicated. Some extract of belladonna was introduced between the eye-lids. At the end of about twenty minutes, the iris had entirely disappeared. From the instant when the pupil had attained about thrice its ordinary size, objects were seen by the eye as it were through a cloud, the obscurity of which soon increased to such a degree that it became impossible to discern small objects within a short distance; for example, the letters of a book held in the hand. It was now ascertained, by means of a doubly-convex glass,

that the focus of the eye was at twice the distance from the object of that of the eye in the healthy state.

The iris, however, still acted, so as to lessen the extent of the pupil, on the sudden approach of a vivid light. The pupil recovered its ordinary extent but very slowly; so that, at the end of six days, it was still as large as on the day after the application of the belladonna. The sight became more clear, and the focus of vision shortened, in proportion to the progress towards the restoration of the ordinary extent of the pupil. In the open air, all objects could be seen pretty well, excepting those situate near to the eyes; but, on entering a room, every thing appeared enveloped in a cloud.

This experiment seems to prove that the iris performs an important part in the phenomena of distinct vision at different distances; and it seems evident from it that dilatation of the pupil accompanies the extension of the distance of the object contemplated, independently of the diversity of the intensity of the light reflected on the eye. When the pupil, in the above experiment, became dilated to a certain extent, the sight became affected as it is with aged persons and in subjects of *presbyopia*; and it required the same kind of glass to remedy the affection as that which is proper for the correction of the latter infirmity. The promptness with which this change in the focus of distinct vision took place may serve as an argument against this change being attributed to a diminution of the convexity of the pellucid cornea from abstraction of a portion of the aqueous humour. It may, perhaps, be accounted for by admitting that the enlarged pupil permits too great a number of luminous rays to fall on the capsule of the crystalline; the greater part of which rays, when coming from objects at a certain distance, are so refracted by the lens as to form a focus not immediately in the situation of the retina, but at some distance beyond it. According to this explanation, the use of the iris would be to prevent more than a certain quantity of light, from objects within a certain distance, falling on the crystalline; and the obscurity of vision which takes place when the pupil is immoderately dilated, would seem to depend on the too great divergency of the rays which it transmits when such a dilatation exists. The pupil is generally very large in *myopes*; a circumstance which, in conformity with the foregoing notions, may be considered as the result of an effort to diminish the inconveniences which result from the excessive degree of convexity of the cornea.

These observations will, perhaps, contribute somewhat towards the explanation of the difficulties attending the phenomena of distinct vision at different distances, in animals not possessed of a muscular structure qualified to alter the degree of convexity of the cornea. KEPLER, it must be generally known, supposed that the recti muscles effect this in man; but there are too many obvious and valid objections to this supposition to permit us to reason on it with any degree of confidence, as sufficient to account alone for the phenomena in question: neither is the power he supposed to exist in the ciliary processes—that they may, when in action, draw the crystalline towards the cornea—at all more probable; and that of the crystalline itself possessing fibres capable of altering its convexity, seems to be still less so;

notwithstanding the ingenuity with which they have been argued for, and the manner in which they seem to be supported by mathematical demonstration. That the conditions of the eye just designated perform some part in the phenomena of vision under consideration, seems to be very probable when we take an extensive view of comparative anatomy, and especially when we consider the structure of the eyes of birds, (which see with the greatest accuracy at very great diversities of distance,) and, especially, the firm manner in which the ciliary processes adhere to the crystalline in these animals: but these circumstances do not seem to be sufficient to solve the problem in its general relations to man and other animals.

HUNTER, on the other hand, in opposition to HALLER, considered that the alteration of the size of the pupil is not alone sufficient for the explanation. The hypothesis of Mr. LITTLETON,* of the power of distinct vision at different distances, (within a certain range of space,) evinces much ingenuity in the author; but the principles of it—that the retina, being of a certain thickness and pellucid, is susceptible of the impression which constitutes vision throughout its whole thickness, not merely on its *surface*; and that the focus of vision may be thus formed in any part of its substance,—are purely conjectural, and devoid of sufficient evidence to permit the generality of physiologists, perhaps, to regard it as any thing else than an ingenious hypothesis: it certainly wants that degree of probability which is required in a theory on which we can rest with a sufficient degree of confidence to satisfy us, if we are at all zealous in the research of truth. If these supposed qualities in the retina be admitted to exist, it must then, it is true, be acknowledged that distinct vision at different distances, within a certain range of space, is mathematically accounted for in Mr. Littleton's hypothesis in a satisfactory manner, as far as relates to the human eye, without any influence of the iris of the kind above described. But, even with such an admission, this hypothesis would not account for the power of distinct vision at such great diversities of distance as those at which the eagle, the hawk, and several other birds, possess that power. A sparrow-hawk will dart, with the rapidity of lightning, from the region of the clouds, and seize a lark on a hillock of ground, that a man could not perceive at the distance of a few yards; and a kite will drop from a height in the atmosphere where it could hardly be discerned by the human eye, and carry from the surface of the earth a bird, a field-mouse, or the smallest lizard, which it had selected as its prey.

The foregoing remarks on vision were written previously to the perusal of the treatise on the Diseases of the Eye, of Mr. TRAVERS,† who has taken a similar view of the relation of dilatation of the pupil to vision at increased distances, as that indicated by the experiment of Mr. Dunglison: so that he advances as a general proposition, that “the adaptation of the eye to light co-operates with its adapta-

* See *London Medical and Physical Journal*, Sept. 1818.

† *A Synopsis of the Diseases of the Eye, and their Treatment; to which are prefixed, a short Anatomical Description and a Sketch of the Physiology of that Organ.* By BENJAMIN TRAVERS, F.R.S. Surgeon to St. Thomas's Hospital. 8vo. pp. 425. Longman and Co. 1820.

tion to distance." It may be here repeated, though it is hardly necessary, that it has been generally known that the pupil dilates as the view extends to more distant objects; but, as the quantity of light reflected on the eye diminishes on this occasion, it had been generally considered that the dilatation has arisen expressly from this diminution of the quantity of light. The opinion here argued for, is that such a dilatation depends, in some measure, on other causes than the diminution of the quantity of light, though it "co-operates" with it. Mr. Travers adduces several arguments to prove this, and he remarks, that "where the iris is from any cause motionless, the power of adapting the eye to distances is lost." If any farther arguments to prove the truth of the explanation above given of the phenomena in question were requisite, the following observations by Dr. ROGET (inserted in the work of Mr. Travers) would seem qualified to exclude further doubts respecting it.

"When I have stated," says Dr. Roget, "that I possessed the power of dilating and contracting at pleasure the iris, the fibres of which are usually considered as no more under the dominion of the will than the heart or blood-vessels, my assertion has in general excited much astonishment. Such, however, is strictly the fact. I can easily satisfy any person who witnesses the movements I can produce in them, that this power is totally independent of the influence of light; since I can effectually exert it, although the position of my eye with regard to the window or candle, as well as the direction of the optic axis, continue unchanged. However singular this power may appear, it admits, I conceive, of a very natural explanation. The effort of which I am conscious, when performing the voluntary contraction of the pupil, is the same as that which accompanies the adaptation of the eye to the vision of near objects, and is of course productive of an increase of its refractive power. This very same power of moving the iris is, in fact, possessed in a greater or less degree, by every person who enjoys the faculty of distinct vision at different distances. It is accordingly well known that if a person, after looking at a distant object, transfers his attention to a near object, the pupil always contracts. But this change, it is supposed, can never be effected, unless some real object or image, from which light radiates, be present to direct the sight. I have never, indeed, met with any person besides myself, who, while steadily directing his eye to a distant object, and while no other object intervened, could, by a mere effort of volition exerted on the eye, augment its refractive power so as to adapt it to the vision of near objects. That I have acquired such a power, I can ascribe to no other cause than to my having, from my childhood, been much in the habit of observing optical phenomena, and of practising various experiments relating to vision,—a subject which I early took great delight in cultivating.

"It is still more easy for me, while an object is placed near my eye and distinctly seen, immediately to relax the organ so as to fit it for the distinct vision of the most distant objects: and these changes I can effect in succession with considerable rapidity, each change being accompanied with a corresponding enlargement or diminution of the pupil. The increasing the refractive power of the eye, is always the

change that constitutes the effort; the state of vision adapted to parallel rays being that of relaxation."

- Instances of *rumination* in man have been of such rare occurrence, that any thing novel relating to them is worthy of being recorded in the History of Physiology, although the phenomenon is a subject of consideration for curiosity rather than one which presents any useful indications. Eight years since, Mr. *TARBES* communicated to the Société de Médecine of Paris an account of a young man in whom this function was manifested: the subsequent history of the case has been very lately related by the same physician. Rumination was first manifested by the patient after his recovery from confluent small-pox, in the sixth year of his age; and it was constantly performed after every meal, until the period of its total cessation. About half an hour after having eaten, he suffered a slight uneasiness in the epigastric region; this sensation was followed by the transmission of a ball of food from the stomach to the mouth. The aliments thus brought up had neither a disagreeable odour nor an acid taste, and did not appear to have undergone any alteration in the stomach. The patient chewed them with as much pleasure as he did on first taking them. After this portion was again swallowed, another mass, which did not appear to have been mingled with that chewed the second time, was brought into the mouth; and so, in succession, all the food he had taken at his last meal was returned. On sitting down to eat his food, the patient, instead of masticating well what he took, only divided it in a very imperfect manner, as might be seen by the specimens returned into the mouth during rumination. If he, by chance, happened to sleep soon after a meal, he, after about two hours, awoke to vomit up all the food which had not been ruminated. He went on in this way until the time of his marriage, when this rumination ceased, almost suddenly. It was lessened on the day ensuing from it, and was entirely discontinued at the end of eight days. A great thirst, which he had suffered whilst he ruminated, disappeared at the same time. He suffered no inconveniences in consequence of the change; and, during the six years which have since elapsed, he has become more robust than heretofore.* The circumstances attendant on the rumination in this case, are similar to those which have been noticed in the other instances on record; and, in respect to the imperfect manner in which ruminating men have masticated their food on first taking it, it may be useful to observe, that, in a case related by Mr. *CULLERIER*, the subject of it ruminated only during a period when he was urgently pressed by his occupations, and ate his meals in a few minutes, almost without masticating his food. On becoming more at leisure, and being able to pass an hour at table, he ceased to ruminate. The act of returning the mass into the mouth seems to have been effected by a mechanism similar to that which produces the same effect in animals in whom rumination is natural. A deep inspiration is first made, and then a sudden attempt at expiration, (during which it is probable that the glottis is closed,) by which, and

* *Journal générale de Médecine*, No. 286.

a sort of antiperistaltic motion of the stomach and œsophagus, the ball is brought up into the mouth, without any sense of nausea or uneasiness. It is in a certain degree an involuntary act; and yet, according to the observations of Mr. TOGGIA,* who has very recently paid much attention to this subject, animals have the power of suspending or hastening it to a certain extent.

Mr. WILSON, when tracing (in his Lectures on the Bones) the characters which distinguish the *teeth* from the other bony structures,—after having stated that a tooth, whilst forming, will receive a tinge from the animal being fed with madder; but only that part of its substance actually formed at the time will take this tinge: so that, by feeding a pig for a limited time on madder, desisting for a similar time from using the madder, feeding it again, and again desisting, we shall find, in the bony substance of its teeth, four strata, alternately red and white. So that teeth, after they are perfectly formed, will not become tinged with madder; nor, when once tinged, will they lose their colour: after having stated those peculiarities of teeth, and remarking that they do not grow softer in those diseases in which bones become soft,—says that he has “met with one solitary instance of the bony part of the teeth having become softer than usual, in a patient whose bones were affected with rickets.” Information of the age of the patient is only wanting to render this an interesting fact in physiology. If the teeth were really perfectly formed, and possessed the ordinary degree of hardness, before the occurrence of the disease of the bones in general, the observation of Mr. Wilson designates a phenomenon that does not seem to have been previously remarked.

As the object of this Sketch is to trace the progress of medical science, not to present a review of medical literature, there are many new works of a systematic character, (and a few of them productions of much merit,) relating to pathology as well as to the other departments of medicine, which will not here be noticed; although it is at the same time acknowledged that such productions, when well constructed, may themselves prove conducive to the further progress of medicine. But, as it is its actual progress only which is here to engage our attention, books which merely offer a view of what has already been effected do not come under our consideration. Such is the character of the greater part of those treatises which appear, from time to time, under the titles of *Systems* and *General Elements*. Men of genius, for the most part, revolt at the duty of compiling so much of what has been promulgated by others with the results of their own original reflections that is requisite in the construction of a general treatise on pathology; and hence the only works of this kind, presenting any important original knowledge, that had appeared since the time of BROWN and CULLEN, were those of DARWIN and PARRY, until

* In his treatise, *Della Ruminazione e Digestione dei Ruminante*. 8vo. Torino, 1819.

"If erethism of the cranial brain be induced, there will be an increase of sensibility throughout the nervous system; an absence of

“I. General Inflammation. { General irritation of the nervous system,
 { General plethora.

sleep; an irritable state of the intellect; probably also an active, animated state of the intellect; an increased production of nervous power; an increase of temperature; an active state of muscles; accelerated action of the heart; increased secretion. In consequence of the increased sensibility of the nerves of the alimentary canal, as well as from the presence of an increased quantity of secreted fluids in that canal, the peristaltic action of the intestines will be increased.

"Erethism of the cranial brain may also lead to an accumulation of exhaled fluid in the ventricles of that substance, which it can only effect by increasing exhalation or by lessening absorption.

"An erethismal state of the cranial brain may also cause an increased quantity of blood to flow through the cerebral arteries, not only by increasing the action of the heart, but by its direct influence on those arteries, the tonicity of which it must consequently diminish. Such increase of the quantity of the blood which flows through the cerebral arteries, existing in combination with an erethismal state of the cerebral substance, constitutes, as I imagine, inflammation of the cranial brain.

"A high degree of erethism of the cranial brain is generally succeeded, sooner or later, by an opposite condition, or torpor, of that substance.

"If inflammation of the cranial brain be present, the sensibility of the whole of the nervous system will be much increased. Nervous power and heat will be more copiously produced; there will be wakefulness and restlessness; the action of the heart will be increased; the state of the alimentary canal will be disordered: painful sensations will be present, referred to the head, the functions of the intellect will be much disturbed, and the faculties will be greatly disordered: muscular action may be powerfully; or involuntarily, performed.

"Inflammation of the cranial brain may also lead to increased effusion from the cerebral exhalants; or to the effusion of a purulent fluid into the substance of that brain; or it may induce disorganization of that structure.

"If a torpid state of the cranial brain be induced, the sensibility of the whole of the nervous system will be lessened; the production of nervous power and of heat will be diminished; the action of the heart will be lessened; the action of the tonic power of the smaller arteries will be increased; secretion will be diminished; exhalation will be increased; the action of the alimentary canal will be retarded, or inverted; the processes of digestion, and of chylicification, will be imperfectly performed; muscular action will be lessened, or it will be per-

- "II. Local Inflammation. {
1. Local plethora.
General irritation of the nervous system.
 2. Local plethora.
Local irritation of the nervous system.
General plethora.
 3. Local irritation of the nervous system.

"Local inflammation may be further divided into:

- "Inflammation, with active resistance of the tonic power of arteries,
- "Inflammation, with enfeebled, or suspended, action of the tonic power.
- "Inflammation, in a part whose arteries possess both elasticity and tonicity.
- "Inflammation, in a part whose arteries do not possess an elastic structure."

formed involuntarily or irregularly : respiration will be imperfectly performed ; the pulmonic process will be imperfectly accomplished ; a comatose state will be present ; and the functions of the intellect will be more or less suspended.

“ There are many other morbid conditions of the cranial brain besides those which have been enumerated ; but it is not necessary to particularize them here, my present object being to show that deviations from the healthy state, occurring in the cranial brain, will induce disorder of the whole economy.

“ Erethism of the cranial brain may exist as an effect of original structure of that substance. It may arise from an altered condition of that substance, whether produced by the presence of foreign bodies or of tumors, by induration or ossification of the dura mater, by thickening of the inner table of the cranium, or by spicula proceeding from that table ; by an increase of the quantity of blood that circulates through the blood-vessels of the cranial brain, and of its membranes ; by increased velocity of the motion of that blood ; by an altered state of that fluid ; by certain substances taken into the stomach ; by concussion ; by an increase of temperature ; by the long absence of sleep ; by a similar state of the spinal brain. It may also be produced by impressions upon the anti-cerebral extremities of nerves : as by the action of light on the retina ; by pressure made on the gums by the teeth, during the process of dentition ; by impressions on the alimentary canal, whether produced by worms, by the presence of an increased quantity of secreted fluids in that canal, or of secreted fluids whose quality is unnatural, by undigested food, by hardened feces, or by food of an unusual quality ; by impressions on the liver, whether produced by tumors, by congestion of bile in the biliary ducts, or of blood in the blood-vessels of that organ ; or by impressions upon any other of the viscera. It may arise from the influence of passion, as from anger or grief ; or from powerful exertion of the faculties.

“ As an increase of the quantity of blood that flows through the cerebral blood-vessels may induce erethism of the cranial brain, so may it proceed from that state of that brain. Inflammation being then, as I conceive it to be, erethism existing in combination with plethora of the blood-vessels, it follows that those causes which induce erethism of the cranial brain may also induce inflammation of that substance.

“ Torpor of the cranial brain may be a consequence of previous erethism of that substance. It may arise from any cause that produces increased compression of the cranial brain : thus, it may proceed from a plethoric state of the cerebral blood-vessels, from an aneurismal state of the cerebral arteries, from congestion of blood in the cranial sinuses, from accumulation of watery fluid in the ventricles or between the membranes, from effusion of blood, from collections of pus, from tumors, from thickening or indentation of the inner table of the cranium. It may also be produced by concussion, by loss of some of the cerebral substance, by some peculiar alteration of the structure of that substance, or it may be connected with, and dependent upon, original formation. It may arise from diminution of the quantity of blood that flows through the cerebral blood-vessels, or from an altered state of

that fluid. Thus, it may arise from loss of blood, from diminished action of the heart, from extreme cold, from disordered performance of the pulmonic process, from vitiated atmosphere, from the respiration of carbonic acid gas. It may be caused by intoxication, by sedatives externally applied or internally administered, by great expenditure of nervous power. It may proceed from a morbid state of the alimentary canal, from worms, from accumulation of feces, from disordered state of the mucous membrane of the canal, from disease of the mesenteric glands, from congestion of bile in the biliary ducts, or from other faulty states of that viscus. It may arise from the powerful or long-continued production of sensation, from the influence of powerful passions, or from powerful or long-continued exercise of the faculties.*

There are two or three points in the foregoing extract which require a few comments to render them clear to those who are not acquainted with the previous parts of the work. The term *erethism*, it is evident, is used by Dr. Nicholl in the same sense as *excitement* and *irritation* are usually done by other pathologists; for, in the cited passage, erethism with increased quantity of the blood flowing through arteries is said to constitute *inflammation*; and he elsewhere says, the term inflammation "implies the existence of an *irritated* state of nerves, in conjunction with an increased flow of blood into arteries." Increased action of the heart is said to be a mean (in conjunction with other circumstances) of diminishing the *tonicity* of arteries; and a torpid state of the brain, by lessening the action of the heart, is stated to be productive of an increase of "the action of the tonic power of the smaller arteries." In explanation of these points, it is only necessary to state (torpor of the nervous system consequent on inordinate excitement of it being understood,) that Dr. Nicholl considers that the tonic power of the arteries* and the momentum of the blood act in opposition to each other: thus, when the momentum of the blood is increased, the development of the tonic action of the arteries is, proportionately, resisted; and, when the momentum is lessened, the tonic action is (unless the property on which it depends is itself destroyed) increased.†

The next point to be noticed is Dr. Nicholl's explanation of the

* "An artery," Dr. Nicholl says, "possesses a structure which has a constant tendency to diminish the capacity of that vessel. This tendency is called its *tonicity*. The *tonicity* increases in proportion to the diminution of the size of the artery."

"An artery also possesses a structure which has a tendency to keep it pervious to a certain extent, but which resists any effort that may be made to dilate the vessel beyond that extent. This tendency is called its *elasticity*."

"When the blood, then, endowed with force by the contraction of the heart, endeavours to make a passage for itself through an artery, it is opposed by the *tonicity* of that vessel, but it is assisted, within certain limits, by the *elasticity*; but, beyond those limits, it will be opposed by the *elasticity* also."

† It may be proper to remark here, that, although this Sketch professes to be a history of the *progress* only* of medical science, it becomes necessary, or at least prudent, in some instances, to give a relation of opinions, the correctness of which may be doubted by the writer; and that such is the case with respect to Dr. Nicholl's opinions on inflammation.

* The writer's reasons for treating the subject in the way here designated, were stated in the first of the series (published January 1819.)

phenomena of *fever*. A particular and methodic analysis of the symptoms which constitute the several stages of the morbid condition of the system which is thus termed, leads him to conclude that the "*early stage*" of it results from "diminished sensibility of the nervous system; diminished action of the heart; a contracted state of the small arteries." The "*height*" of it, from "increased sensibility of the nervous system; increased action of the heart; increased flow of blood through the smaller arteries, the opposition which is made by the exertion of the contractile power of these vessels being overcome by the increased action of the heart." The "*decline*" from "diminished sensibility of the nervous system; enfeebled action of the heart; relaxed state of the small arteries."

The respective states of the several stages above enumerated, Dr. Nicholl considers, may occur in either of the three following orders:

"1. Contraction of the small arteries; diminished action of the heart; torpor of the nervous system.

"2. Diminished action of the heart; torpor of the nervous system; contraction of the small arteries.

"3. Torpor of the nervous system; contraction of the small arteries; diminished action of the heart."

Adverting then to the subsequent phenomena, Dr. Nicholl says that

"These three states having existed an uncertain time, a new order of states arises. The action of the heart becomes increased, increased sensibility of the nervous system takes place, and an increased quantity of blood is received by the small arteries. These three changes may take place in varied order; for instance:

"The increased action of the heart may first arise: this may produce a removal of the torpor of the nervous system; and it may, by increasing the momentum of the blood, overcome the resistance which is offered by the exertion of the contractile power of the small arteries to that fluid.

"Or, the sensibility of the nervous system may become increased; and to this altered state of that system may succeed increased action of the heart, and a more copious flow of blood through the small arteries.

"Or, if the contracted state of the small arteries give way, a more free and powerful action of the heart may follow, and the torpor of the nervous system may be removed."

"The states which produce the symptoms of the second stage of fever may, then, occur in either of the three following orders, viz.

"1. Increased action of the heart; increased flow through small arteries; increased sensibility of nervous system.

"2. Increased sensibility of nervous system; increased action of heart; increased flow through small arteries.

"3. Diminished contraction of small arteries; increased action of heart; increased sensibility of nervous system.

"Having arrived thus far in our inquiry, let us return to the states which produce the symptoms of the first stage of fever.

"Let us suppose a case in which *increased contraction of the small arteries* is the first effect of the primary cause of fever.

“ In such a case, the resistance offered by those arteries to the current of the blood may induce an obstructed state of the general round of the circulation, whence will ensue a turgid condition of the larger arterial and venous trunks, and of the cavities of the heart; and from these effects will arise an oppressed and an over-powered state of the action of the heart. The obstructed state of the circulation will lead to a turgid state of the cerebral blood-vessels; which effect, as well as the want of a free supply by those vessels of blood which has duly undergone the pulmonic process, will induce torpor of the cerebral structures: and, from the torpid state of those structures, as well as from the diminution of the quantity of blood received by the small arteries, will ensue diminished sensibility of the nerves in general.

“ Thus with the several causes of the symptoms of the early stage of fever be present.

“ The action of the heart, at first over-powered by the resistance offered to the blood by the small arteries, and still more enfeebled in consequence of the diminished sensibility of the nervous system, may gradually become increased. It may become increased, possibly, from its own powers of contraction having acquired an accumulation of energy during its oppressed state,* or in consequence of increased sensibility of the nervous system; or, perhaps, in some instances, owing to a diminution of the contraction of the small arteries, and a consequent diminution of opposition to the action of the heart. Should the action of the heart become increased, the momentum of the blood will be increased, and, consequently, the resistance offered by the small arteries may be borne down; in which case the freedom of the circulation will be restored. The removal of the obstructed state of the circulation, and the distribution of an increased quantity of blood throughout the nervous system in general, will remove the causes of torpor of that system. Torpor of the cranial brain is, in many instances, succeeded by a degree of sensibility of that structure, greater even than that which immediately preceded the insensible state of it. This is constantly the case after sleep. So, also, an insensible state of the nerves in general is, in many instances, succeeded by a degree of sensibility greater than that which immediately preceded the insensible state. In the case under consideration, then, it may happen that the torpor which prevailed in the early stage of fever may, after it has existed for some time, gradually be resolved into a state of increased sensibility. The presence of increased sensibility of the nervous system will be followed by increased action of the heart, and by the flow of an increased quantity of blood through the small arteries.

“ Thus may those states be formed which give rise to the symptoms attendant on the second stage of fever.

“ Suppose that torpor of the cerebral structure is the first effect of the primary cause of fever. Such a state will give rise to general insensibility. The action of the heart will be diminished, and the contraction of the small arteries will be increased. The increased

* As the fibres of a muscle which has become enfeebled, from powerful exertion or from great distention, or which has been for a long time inactive, acquire a state of increased contraction,

contraction of these arteries will, as we have seen, tend to produce an enfeebled action of the heart; and a diminution of the action of the heart leads, as we have also seen, to increased contraction of the small arteries. Thus may the causes of those symptoms which attend the early stage of fever be present.

“ If the torpor of the cerebral structures, after it has existed for an uncertain time, subsides, and is succeeded by a degree of sensibility even greater in degree than that which preceded it, such new condition of the cerebral structures will be attended by increased sensibility throughout the nervous system generally: increased action of the heart will succeed, and an increased quantity of blood will flow through the small arteries. Thus those states will be established which give rise to the symptoms attendant on the second stage of fever.

“ If diminished action of the heart be the first effect of the cause of fever, diminution of the momentum of the blood will ensue, to which will succeed increased contraction of the small arteries and torpor of the cerebral structures, and, consequently, a general diminution of the sensibility of the nervous system. And thus those states will be formed which produce the symptoms of the early stage of fever.

“ When these states have existed an uncertain time, if, as we have supposed, the heart be enabled, from changes which take place within itself during its state of inactivity, to act with a degree of force even greater than before; the momentum of the blood will be increased; and the resistance of the smaller arteries will be overcome. The torpor of the cerebral structures will be removed, and a preternatural degree of sensibility of those structures may arise, not only as a consequence of an increased flow of blood through their blood-vessels, but possibly, as we have supposed, as a consequence of the previous state of torpor. The increased sensibility of the cerebral structures will induce a general increase of sensibility throughout the nervous system, and will cause the action of the heart to be still more forcibly exerted. Thus, in various ways, those several states which characterize the second stage of fever will be established.

“ During the existence of the second stage of fever, although an increased quantity of blood flows through the small arteries, yet this increased flow appears to arise, in the generality of cases, not from cessation or diminution of contraction in those vessels, but from the increased action of the heart enabling the blood to force its way into, and through, these arteries, in spite of the opposition which is offered by their contractile power to that fluid. So that, in the second stage of fever, there is, as it were, a continued contest and struggle between the action of the heart and the contractile power of the small arteries, the balance of power being constantly in favour of the former.

“ We now proceed to the consideration of the last stage, or the decline of fever.

“ We observe that, whenever a high degree of sensibility of the nervous system has existed for some time, it is, sooner or later, succeeded by a state of torpor, the degree of which is proportionate to the duration and the degree of the preceding sensibility.

“ We also find that, when a high degree of action has been kept up

for some time by the heart, a languid action of that organ sooner or later succeeds; the degree of languor being proportionate to the duration and the degree of the preceding strength of action.

“When a high degree of resistance to the momentum of the blood has been offered, for a length of time, by the contractile power of the small arteries, the exertion of that power will, sooner or later, become enfeebled, or it will be suspended; the diminution of that exertion being proportionate to the degree and duration of its previous activity.

“We have traced the various ways in which those states which characterize the second stage of fever may be formed. Let us suppose that, in either of those ways, these states have been produced. The high degree of sensibility of the nervous system; the high degree of action on the part of the heart; the strong, but unavailing, resistance of the small arteries; may, severally, last an uncertain time. The natural consequence of each of these states will be, as we have just seen, the formation of an opposite state. The high degree of sensibility will give place to torpor; the powerful action of the heart will subside into feeble contraction; the stubborn resistance of the small arteries will give way. Accordingly we find that these natural consequences are precisely the occurrences which are met with in the last stage of fever. The sensibility of the nervous system becomes diminished in proportion to the duration and degree of its previous increase. The action of the heart becomes feeble. Both these new states favour the increase of each other. The contraction of the small arteries gives way; the struggle between these vessels and the heart is at an end, so that the contraction of the heart, although enfeebled, may still be able to carry on the circulation of the blood; and thus an obstructed, or a stagnant, state of the round of the circulation, which would otherwise result from the languid action of the heart, may be prevented.

“It appears, then, that both in the first and in the last stages of fever, the sensibility of the nervous system and the action of the heart are diminished; while, in the early stage, the contraction of the small arteries is increased; whereas, in the decline of fever, it is diminished or altogether suspended. The different state of the contractile power of the small arteries, then, appears to constitute the leading distinction between the character of these two stages of fever.

“From the result of our investigation, it appears that the states which characterize the second stage of fever are the consequences of those states which usher in fever; and that the states which constitute the third stage are the natural results of those which are met with in the second stage. It follows, then, that the states attendant on the third stage, as well as those which mark the second stage, are the consequences of those states which appear in the first stage of fever.”

The appendix entitled “Aphorisms respecting Absorption,” comprises a very comprehensive, accurate, (if the assertion that the older matter of the solid structures of the body is continually removed, be, perhaps, excepted,*) and remarkably precise account of the pheno-

* See page xxv. of the last *Proœmium*, for some remarks on this point of doctrine.

mena of this function as it is effected under various states of the system; but there is nothing new in the author's views in this instance, after those displayed in Mr. PRING'S *Essay on the Absorbents*, from which, it seems probable, some of Dr. Nicholl's notions have been derived.

Several contributions, of considerable value, to the history of the *yellow-fever** have been recently produced in the treatises of Drs. DEVEZET and WATTS,† Mr. MOREAU DE JONNÉS,§ Dr. GIRARDIN,|| Dr. DAVID REESE,¶ and Dr. IRVINE;** in the dissertations of Drs. REVERE,†† LE FORT,‡‡ and VATABLE;§§ in some articles in the 8th Number of the *Journal of Foreign Medicine and Surgery*, the *North-American Review* (No. 27), the *New-England Medical Journal*; and in several Reports of Medical Societies and "Boards of Health" in the United States.

Whilst diversities of opinion exist respecting the nature and origin of this disease, amongst persons worthy of consideration from their talents or official character, as is the case at present both in Europe and in America, it seems to be the duty of an historian to relate the most important facts which may be developed respecting those points, even though they may, in many instances, be only such as had been already ascertained; for the frequent testimony of similar phenomena under similar circumstances must tend to remove the doubts which have been entertained of their reality, and of their dependence on constant natural laws. Such an abstract of the accounts above referred to as may be comprised within a few pages, congruous with the limits of this Sketch, will therefore be here given; and an attempt will be made to embrace in it the facts which appear to be of most importance, and such as are of most general occurrence, as well as those inferences from them which observers in different situations have concurred in establishing. Perhaps the essential traits in the history of the disease may thus be comprised in the space to which it must here be confined. The subject will be treated on under the following distinct heads: the *topography* (including meteorological facts) of the situations in which

* It may be proper to remark, that the yellow-fever manifested itself, with much severity, during the year 1819, at Cadiz, Seville, Xeres, Porto Santa Maria, and several other parts of Andalusia; at Minorca, Cuba, Jamaica, Martinique, Guadaloupe, Cayenne, and in almost all the states of the Union in North America.

† *Traité de la Fièvre Jaune*. 8vo. pp. 311. Paris, 1820.

‡ *An Account of the Yellow-Fever which appeared in various parts of the United States, during the Summer and Autumn of 1819*. Published in the *New-York Medical and Surgical Register*, Part ii. vol. i. 1820.

§ *Monographie Historique et Medicale de la Fièvre Jaune des Antilles*. 8vo. pp. 334. Paris, 1820.

|| *Memoire sur la Fièvre Jaune, considérée dans sa Nature et dans ses Rapports avec les Gouvernements*. 8vo. pp. 91. Paris, 1820.

¶ *Observations on the Epidemic of 1819, in a part of Baltimore*.

** *Treatise on the Yellow-Fever of Charleston of 1819*.

†† In the *American Medical Recorder*, vol. ii.

‡‡ In the *New-York Medical Repository*, June 1820.

§§ In the *Journal Universel des Sciences Medicales*, Octobre 1820.

the yellow-fever has occurred ; the mode of its *propagation* ; the immediate consequence of its *invasion* of the human economy ; its *symptoms*, *progress*, and *duration* ; its *etiology* ; the results of *necroscopic researches* ; the *people* affected with it ; and the *ratio* of its *mortality* to the number of its victims.

The *topography* of Louisiana, as it is given by GIRARDIN, seems to present at once all the essential circumstances which appear to generate, or to render epidemical, this disease. This country is low, to a considerable extent under water, overgrown with rushes, and the incumbent atmosphere is obscured by vapours which are impregnated with a marshy odour. The greater part of it is composed of alluvial soil, formed by means of the river which has its course through it. We may see, in different parts, places entirely inundated, meadows covered by cyprus trees, stagnant waters, and lakes filled with insects and reptiles. The land is every-where situate lower than the sea, and devoid of the smallest hillock ; it presents water at the depth of a very few feet from the surface, with trees which have been thrown down and buried by means of floods ; and the earth is black, bituminous, argillaceous, and is composed of the mud of the river and the decayed remains of vegetable substances. The winter is ordinarily foggy, humid, and rainy. The temperature, in this season, but rarely descends so low as the freezing point ; and this degree of cold soon gives place to a heat of from 55° to 60° . The changes of the seasons are effected in a very sudden manner. During the summer, which comprises the months of June, July, and August, the thermometer in the shade marks from 85 to 95 degrees. The heat, combined with the humidity, is extremely oppressive to the vital economy : animal matters putrefy in a very few hours ; metals become covered with rust ; and cloths kept folded up in wardrobes, furniture, &c. are imbued with moisture and mouldiness. There is no wind. The nights are as oppressive as the days. The houses are impregnated with the exhalations which rise from the soil, or which fall on the setting of the sun. The morasses become dry, and infect the atmosphere with deleterious miasmata : the arid earth opens, and gives vent, through large crevices, to fumes of heat and blasts of fetid gases. Storms succeed to this state of climate, and rain falls in torrents ; but the sun soon reappears, and instantly deprives the air of its freshness, and the earth of the water which is present on its surface. Plants of various kinds acquire a great degree of development, and spring up every-where in the utmost abundance.

It is in countries having such a climate as that just described, either constantly or occasionally from some accidental circumstances, that the yellow-fever is endemical, and in which it manifests itself in its most devastating forms. Such are several of the West-India islands and a considerable part of the continent of the New World.

A certain degree of heat and moisture, and the miasmata of putrefying animals and vegetables, appear to be the essential things to which the disease owes its origin ; but, besides these, there are some other circumstances which appear to have some influence in developing it, and in adding to its ravages when it already exists ; the most remark-

able amongst which are an extraordinary abundance of insects,* and storms of lightning or other evidences of inordinate electrical commotion in the atmosphere.†

It is stated above that the yellow-fever prevails epidemically,—and it might be said, with the greatest probability, that it originates endemically,—in climates where a certain degree of heat, with humidity, and effluvia of putrefying animals and vegetables, exist, either constantly, in certain seasons, or accidentally,—that is to say, from occasional or artificial causes. The latter seems to be the case with several of the cities in the United States.‡ A considerable part of several of them, especially of New-York, is built on false ground.§ The wharves, particularly, in most of the cities of the Union, are made of an artificial soil, constituted of “rotten logs, bushes, shavings, and other vegetable matter, covered lightly with swamp mud or earth;”¶ manifesting, after rain and during great heat, an extremely offensive odour. These are banked up by planks, through which the water soaks and forms pools, extending to a considerable distance under the adjacent streets, that become stagnant when the river is low during dry weather, and affect the air with “poisonous exhalations.” It is in those parts of the cities that the fever has constantly originated, and the disease has generally been confined to them alone; affecting only those persons who either resided in them or temporarily visited them, &c.

The seasons of the year in which it has generally been epidemical are the summer, especially the latter part of it, and the commencement of the autumn; and it has ordinarily been found to cease nearly or totally on the occurrence of a frost. In Baltimore, in 1819, it began to disappear about the beginning of October; but, Dr. Revere says, it was not considered safe to return to the infected district until the beginning of November. On speaking of the fever as it has occurred at Charleston, South Carolina, Dr. J. M. Campbell states, that “a severe frost always puts an end to the disease, while cool weather increases it.” In New Orleans, in 1819, it was not considered safe to visit the city until the beginning of December. It disappeared in New-York about the middle of October.

The whole of the authors above referred to—with the exception of De Jonnés, who is a *soldier*, (a *chef d'escadron*,) not a *physician*,—concur in attributing the disease to atmospheric influence, originating from the phenomena above described; and they almost universally agree in considering this as the sole means by which it is propagated. Dr. Watts says, “from one end of the continent (of North America)

* MIMAUT, (observed in Spain); the Editor of the Journal of Foreign Medicine, (in tropical climates); DEVEZE, (in the United States); North-American Review.

† DE JONNÉS, DEVEZE.

‡ WATTS, *passim*.

§ LANCISI appears to have been the first who clearly marked the influence of this circumstance in the generation of fevers, and by this he explains their frequency in certain parts of the city of Rome. See his dissertation *De Noxiis Paludum Effluviis*, (op. tom. i. p. 189;) and BAGLIVI, (op. p. 157.).

¶ WATTS, page 273.

¶ WATTS, *passim*, in a multitude of examples.

to the other, it has been officially announced, during the last season, that the yellow-fever was not communicated from one person to another, and not even in hospitals, where the sick have been admitted in great numbers." There is not a single fact related in all the writings above alluded to, that furnishes even probable evidence of the yellow-fever having been in any instance propagated by contagion: and Dr. Watts has satisfactorily shown that the cases which were adduced as examples of such a mode of propagation, during the late prevalence of the disease, tend to prove its endemic origin. A very few physicians in the United States continue, however, to maintain the contrary doctrine, though they have been unable to adduce any plausible evidence in its support from the facts presented during the period here alluded to.* In this epidemic, as it has frequently been observed on other occasions by former writers, it seemed evident that the infection prevailed in the air especially at night,† or else that the animal economy was at this time particularly susceptible of its influence. Dr. Revere says, many instances occurred of persons being taken sick after only passing through, or going for but a short time into, the affected district; and it was remarked that there was scarcely an instance of a person visiting that part of the city at night without undergoing an attack of the disease, while during the day it might be visited with comparative impunity. A similar remark is made by Dr. Reese, who observes, that "several physicians who had been attending patients through the whole course of the fever, in the very centre of infection, in the day-time, were exempted from the malady; but, by visiting it once in the night, they became diseased."‡

The period of its invasion has also been observed to be towards day-break, or between midnight and noon. Dr. Irvine says, that the seizures which occur about day-break "are more apt to terminate fatally than such as take place in the forenoon;" and he also remarks, that "persons are rarely attacked at any other than those periods in the twenty-four hours." In general, the attack is accompanied with a great shock of the system, accompanied with a sentiment of severe terror, and inavertible apprehensions of imminent death,§ even before

* The Faculty of Medicine of Baltimore has declared that no circumstance had occurred to excite a suspicion that the disease was contagious.

† DEVEZE, REVERE, REESE.

‡ The following anecdote, related by Dr. Revere, seems to be particularly interesting, as it seems to show the rapidity with which the morbid effluvia produces its influence; and it indicates, probably, what in one instance was the interval between the agency of the infection and the development of the disease. "The ship *Ceres* sailed for Liverpool on the 25th of September; amongst the passengers were a gentleman, his wife, and daughter; the latter a very interesting young lady about twenty years of age, who had lived in a very healthy spot, in the westerly part of the city, (Baltimore,) rode to Wirgman's wharf, for the purpose of embarking. The ship was already under-way, and the boat waiting expressly for her. She only waited to get from the carriage to the boat, and was then rowed off. This was the only time she had been at the point during the prevalence of the fever. The third day after she sailed, she was seized with the usual symptoms of yellow-fever, and died with black vomit, hemorrhage, &c. There were several others who died in this ship, but they had been frequently in the unhealthy part of the town."

§ DE JONNES, REVERE, IRVINE, DEVEZE.

the organic functions were apparently much disturbed. At other times its attack is more gradual, and slight rigors, succeeded by flushings of heat, occur for several hours previous to such a development of the disease as renders the patient conscious of the derangement of his health.*

The *symptoms* and *progress* of the disease, in its most ordinary form, seem to have been portrayed with much force and precision by Dr. Gordon: his account of it will, therefore, be here transcribed. Dr. Gordon divides the disease into three stages.

“The first stage is that of invasion, and marked by effects of a sedative cause upon the system. Whether that cause be only local in its operation, and arising solely from the noxious gases and detritus arising from the decomposition of organized bodies or of the soil itself, in a heightened temperature of the atmosphere, producing sporadic cases of fever in unseasoned individuals;—or whether these more local causes in combination with peculiar states of the atmosphere, as it regards the state of the electric fluid contained in it, either operating upon the human system separately, or in conjunction with high temperature and moisture, and giving rise to the epidemic forms of the disease; still the primary effects observed in the system are of a sedative nature, which are taken collectively to mark the stage of invasion. They may occur individually or in part; their duration may be so short as almost to elude detection, or they may be prolonged. The same mode of operation upon the system marks, in our opinion, the influence of contagion, especially at the commencement; and, however different the cause and the consequent phenomena which spring up during the course of the different diseases depending upon those causes, still the symptoms of invasion mark a sedative effect. Of contagion, as connected with yellow-fever, we shall at present forbear to speak, but proceed in our delineation of the symptoms marking the invasion of the disease.

“All new comers into a tropical climate are generally seized within the first twelve or eighteen months, and in sporadic cases at any time of the year; but more frequently upon the first setting-in of the rainy season. The stage or period of invasion commences with debility and coldness, sometimes chilly rigors occur; lowness of spirits; inability

* The following anecdote, related by HUMBOLDT, is a striking illustration of this fact.—“Une personne avec laquelle j'ai eu des relations d'amitié pendant mon séjour à Mexico, n'avait passé que très peu de temps à la Vera-Cruz, lors de son premier voyage d'Europe en Amérique. Elle arriva à Xalapa sans éprouver aucun sentiment qui pût lui faire connaître le danger dans lequel elle se trouverait bientôt. Vous aurez le vomito ce soir, lui dit gravement un barbier Indien, en lui savonnant le visage; le savon sèche à mesure que je l'applique, c'est un signe qui ne trompe jamais; et voilà vingt ans que je rase les *Chapetons* qui passent par cette ville en remontant à Mexico: sur cinq, il en meurt trois. Cette sentence de mort fit une forte impression sur l'esprit du voyageur; il eut beau représenter à l'Indien combien son calcul était exagéré, et qu'une grande ardeur de la peau ne prouve pas l'infection; le barbier persista dans son pronostic. En effet, la maladie se déclara peu d'heures après, et le voyageur, déjà en route pour Ferote, fut obligé de se faire transporter à Xalapa, où il manqua de succomber à la violence du vomito.”—*Essai politique sur le Mexique*, p. 774.

to enter upon any mental or corporeal exertion; pulse feeble, sometimes slower than natural, and unequal, and sometimes intermittent and easily compressible; the features pale; a visage smaller than in health; anxiety at the precordia, which the patient attempts to relieve by frequently sighing; yawning often takes place, and sometimes a copious flow of urine. The patient frequently complains, in this stage, of nothing beyond a slight indisposition, which, from the failure of the digestive energy of the stomach, he attributes to disorder occasioned by some irregularity of diet. The tongue is generally white and trembling when attempted to be pushed out of the mouth; it is at other occasions natural. Pains in the loins and weakness of the joints, especially of the lower extremities, are generally present, with a megrim, or nervous head-ach. If a few sharp or rapid rigors take place in this stage, that of re-action often soon supervenes. The symptoms of invasion may take place in the night and pass unnoticed, the person awaking in the morning with the symptoms of commenced re-action; or, when the exciting cause of the fever has invaded the system, although not in a very sensible manner, and the person undergoing fatiguing exercise, or under the stimulus of a burning sun, he may be immediately seized with the stage of excitement, without being sensible of any premonitory symptoms.

“ The second stage, after a slight or more severe invasion, commences with head-ach, suffused and injected eyes, the countenance generally red and swollen; symptoms of increased excitement become general throughout the system, or is confined more or less to one of the great cavities or viscera, while an abstraction of the vital energy is experienced in other parts of the system; and the patient feels a burning sensation in some internal organ, while the extremities remain cold: this more particularly occurs in the malignant form of the disease. The skin gives a burning and caustic sensation to the hand, and sometimes, early in this stage, partial perspirations over the face, neck, and breast, break out; the pulse is strong and full, but seldom as yet above 90, equal and not compressible; the tongue dry and furred; the bowels generally confined; all the febrile symptoms become exacerbated; the pain, especially in the forehead, loins, and limbs, distressing; and a sense of pain and anxiety about the epigastric region now supervenes. The respiration is often laborious, interrupted, and anxious, and the respired air feels unusually warm to the back of the hand when held near the patient's mouth, and seems to dry the fauces and mouth, and increase his desire for drink; deglutition becomes also more difficult, from a deficiency in the secretion of the salivary and mucous glands. Generally towards the end of the twenty-four hours, and as this stage advances, nausea and vomiting occur, the pulse becomes quicker, and the pulsation in the carotids and at the epigastric region, as the person lies on his back, may be counted at the distance of several yards; the heat of the surface becomes more acrid, thirst more urgent, and the eye begins to assume a yellow appearance; the abdomen, when pressed upon, occasions considerable pain; the vomiting soon becomes more urgent, and delirium super-

venes; the soreness of the stomach extends along the œsophagus. These are the symptoms which mark the second period of this disease, and are present according to its malignance. In the non-acclimatés this period is generally well marked; but we must observe, that in delicate habits, and in long residents, when the exciting cause has been powerful, or during the prevalence of an epidemic form of this disease, the patient may sink gradually from its overwhelming influence upon the energies of the system, without any signs of excitement manifesting themselves, and without any sensible remission.

“ The third stage or period, according to our division of the phenomena, is that marked by collapse in consequence of the previous excitement; and it answers to the second and third periods of M. Deveze. The countenance, from being turgid and florid, now becomes contracted and anxious; respiration becomes more difficult; pulse, at first, smaller and much quicker; the mouth and teeth more furred; the irritability of the stomach increases, and the matters ejected consists principally of the liquids taken, for which there is a constant and urgent demand. The vomiting, although not very violent at its commencement, unless after the reception of ingesta, affords a variably coloured matter, sometimes light and very acid, and setting the teeth violently on-edge. This appearance generally exists, according to our own observation, at the commencement of the gastric symptoms, and, in our opinion, seems to arise from substances remaining upon the stomach in the invasion of the disease, and which, during the first stage or that of re-action, cannot experience the healthy process of digestion, but undergoes decomposition and the acetous fermentation, into which it more readily enters from the increased heat of the system; and the matters thus altered, from their irritation on the mucous membrane of the digestive canal, heightens the symptoms referable to this organ. As the disease advances, the matters evacuated from the stomach are mixed with bile, which, with the diseased secretions, exhibit an inky appearance, and has membranous flocculi floating in it. In some cases the patient obstinately refuses taking any thing, in order to avoid the distress occasioned by vomiting. The matters ejected soon become black, having the appearance of coffee-grounds, and a burning sensation is felt in the stomach and œsophagus. Irritability of the bowels generally appear at the commencement of this stage or in the second, owing to the irritation of the mucous membrane of the stomach extending to the small intestines. The evacuations, which during the early periods of the disease were scanty, and often with difficulty procured, become first liquid, sometimes mucous and glairy, afterwards streaked with tints of yellow and green, and at last greenish black, and sometimes bloody. With the appearance of diarrhœa, the pain in the loins increase, the urine becomes of a light brown, afterwards of a greenish brown, and of a muddy appearance. The yellow colour of the skin now becomes general; but sometimes it appears earlier in the disease, and gradually assumes a more dirty or dusky hue; and parts pressed upon, or improperly blistered very late in the disease, become livid and even sphacelated. The delirium increases, and becomes more low and muttering; the loins and limbs

more severely painful, the latter drawn up, and the respiration more laborious; the face gradually assumes the Hippocratic character, and all the symptoms of great ataxy present themselves."

An excellent American writer observes, that, "in most cases, (as the disease occurred at Boston,) death was preceded for some hours by a great failure, and sometimes by an entire loss, of the pulse at the wrist; while the patient felt easy and even comfortable, or else was comatose."

In the early period of the disease, Dr. Revere remarks, that not only was there, in almost every case, extreme sensibility about the *scrobiculus cordis*, but this preternatural sensibility also "sometimes extended over the whole surface of the body, so that, in one instance, the patient, a man, complained seriously of the pain caused by the pressure of the finger of the physician on the wrist, in feeling the pulse:" and he adds, "in those parts which had undergone the process of vesication, the soreness and pain were most exquisite. I saw a sailor, on whom the common pains of the body would have made no impression, absolutely weep from the pain he endured in dressing his blister." It is a very bad sign when, in the latter stage of the disease, the patient expresses an opinion that he is well, or that very little is the matter with him.* At Savannah, symptoms occurred in the course of the disease resembling those which arise from taking "an over-dose of arsenic, or any other deadly substance."† Dr. Revere says, that "there were no distinct remissions or exacerbations of the fever. After the first attack there was generally an uninterrupted febrile state, until a complete intermission took place."

In respect to the *duration* of the disease, Dr. Irvine says: "In the early part of the season, the cases usually terminated on the fourth day. As the season advanced, they extended themselves to the sixth day, when the worst symptoms appeared on the fifth. Towards the end of the summer, the disease was sometimes protracted to the eighth and even thirteenth days: during this long collapse, the patient generally remained perfectly quiet, though very much prostrated, except when injudicious attendants tampered with the stomach, by improper administrations, and thus brought on the vomiting and other afflicting symptoms. Dr. Revere says, "One gentleman assured me, that, of the twenty-three last cases which were under his care, nineteen died in the following manner: after two or three days of smart fever, a complete intermission would take place, the patient would seem convalescent, his appetite would return, and every thing would appear most flattering; when suddenly, and without any apparent reason, he would be seized with nausea, or rather a most distressing sensation of weight about the pit of the stomach; soon after which, black vomit, or hemorrhage, would ensue, which were the harbingers of death." Dr. Revere became acquainted with many cases in his own practice, in which the patients died almost immediately after the attack, before re-action ordinarily commenced; and he remarked that the cases

* IRVINE.

† Report of the Medical Society of Savannah.

which occurred during the latter part of the period in which the disease was epidemic, were "more malignant and fatal" than those which happened about its commencement.

In the *etiology* of yellow-fever, there is no, at all general, congruity of opinion, even amongst the physicians of the United States, where the disease may be supposed to have manifested itself with much similarity of character. Dr. Watts regards it as "an aggravated form of bilious remittent fever:" Dr. Irvine says, that it is a totally distinct disease from bilious fever; and he rests his diagnostic on the great irritability of the stomach in yellow-fever, which, he says, constitutes "a sufficiently broad line of demarcation between the respective diseases." Girardin considers it as a form of *gastritis*; but necroscopic observations do not seem to warrant this opinion: neither do they establish the doctrine that it is dependent on hepatic irritation, as will be presently shown. Dr. Gordon considers that the violent irritation, and consequent inflammation, of the mucous membrane of the stomach "arises in part from the altered ingesta remaining in the stomach, as well as from the increased action of the biliary organs, and consequent flow of bile into the stomach and duodenum, which washes along with it the mucous and other secretions: thus a naked and delicate texture is left highly sensible to the stimulus of any substance received. As these secretions become more vitiated, the irritation they produce upon this tissue advances to inflammation, and a train of consequent symptoms is produced. That the secretions become altered, and even vitiated, during the progress of the disease, may be admitted. "Dr. Physick's hand," says Dr. Rush, "was inflamed, in consequence of its being wetted with bile in this state, in dissecting a body."

Pathological anatomy has not presented any constant results: after a review of the observations of several of the most experienced practitioners on this subject, Dr. Gordon remarks, in a summary manner, that "the internal surface of the stomach, jejunum, and small intestines, with the biliary system, has been found frequently in a state of lesion; next, the brain and its membranes; afterwards, the viscera of the chest, the spleen and pancreas, and serous membranes of the abdomen; and, most seldom of all the parts of the system, the urinary organs." Dr. Deveze, whose experience has been very extensive, in the hospital at Bush-hill, near Philadelphia, and whose talents are of a very respectable kind, arranges the morbid appearances somewhat differently: he says, that the encephalon is but rarely affected; the lungs in a great proportion of cases; that the mucous membrane of the stomach and intestines almost always presents signs of violent inflammation; and that the liver is diseased in a great many cases.

The *persons* who are especially the victims of yellow-fever in tropical climates, are strangers, or persons but lately arrived in such climates;* but, in latitudes of mild temperature, the aboriginal inhabitants seem to be almost equally liable to it with foreigners.† One attack, for the most part, but by no means constantly, prevents a

* DE JONNES, DEVEZE, VATABLE, GIRARDIN.

† The works of all the American authors already cited.

subsequent one; but instances of secondary attacks,* and of *their* having proved fatal, are by no means rare. In the warm climate of Charleston, Dr. Irvine says, "children, from the age of one to twelve years, are equally with strangers exposed to the disease, and they can only obtain security against it, if they remain there, by passing through what is denominated a yellow-fever season." Speaking of the same city, Dr. Campbell says, "The subjects of it are strangers and children, there being no instance of a native adult having the disease, unless he shall have estranged his constitution by several years' absence. It appears necessary to live in the yellow-fever atmosphere to be exempt; for persons who reside but a short distance from the city are liable to the disease on coming to town. Europeans and northern men have the fever in a more violent degree, and the chances of recovery are less, than in those accustomed to a warm climate."

At Savannah, according to the Report of the Medical Society, comparatively few of the native adults were affected by it: during the last week of September, 1819, there were one hundred and sixty-three cases, thirty-four of which occurred amongst the resident citizens; and, of the thirty-two deaths reported for the same period, thirty-one occurred amongst foreigners. At Mobile, "it spread rapidly through the whole town, and affected the Creole inhabitants, people of colour, and even slaves." At New Orleans, the planters of the Antillas, the Creoles, and the inhabitants of the surrounding country, generally escaped the fever; and not one Negro is remembered to have had the disease.† In New-York and the more northern States, the native inhabitants seem to be almost as commonly its victims as strangers.‡

The *ratio of the mortality* of the disease in the United States, varied much in different districts; but there are no records by which it can be ascertained in its full extent. At Charleston, during the last two weeks of September, it was, as it appears from a passage already cited, one in five amongst the residents; but it is said at the same time that "the Irish people that have come here at different times during the season, have been almost entirely swept away." At Mobile, the resident population of which was estimated at thirteen hundred, it is supposed that one hundred and fifteen died of the yellow-fever of the same inhabitants. At New-Orleans, "it appears to have prevailed with a desolating violence little short of the plague."§ At New-York, "the general mortality does not appear to have been as great as in former seasons of yellow-fever. The first ten or twelve cases, however, proved fatal."|| At Cuba,¶ in 1819, the total number of deaths from yellow-fever was 5,162.

The typhoid form of fever, which had been, during the last few years, epidemical to a very unusual extent, in most parts of Great Britain, and especially in Ireland, has almost totally disappeared:

* WATTS, p. 254.

† Report to the Medical Society of New Orleans.

‡ WATTS, *passim*.

§ Ibid.

|| Ibid.

¶ DE JONNES.

this disease is, at least, not more prevalent now than it has been at almost all times in cities inhabited by a dense population. The only recent historical accounts of it are those of Dr. PRICHARD,* Dr. O'BRIEN,† and Mr. SANDWICH;‡ the whole of which contain observations of more or less value. As the subject of them has already been noticed on former occasions, it is not necessary to enter into the consideration of it here. The opinions which Dr. O'Brien has founded on the results of his experience of the fever as it existed in Dublin, have also been detailed: the following paragraphs will show those of Dr. Prichard.

"That the derangement of the system of functions which constitutes fever is very nearly allied in its nature to the disease which accompanies the inflammation of particular organs, is an opinion which is gaining ground every day among medical practitioners. It is, indeed, very remarkable that they have been so long in arriving at it, when we consider the striking analogy in the phenomena of these distempers. Fever of almost every type and variety is occasionally imitated so closely by morbid states that occur in the phlegmasiæ, that it is sometimes necessary to make a diligent inquiry into circumstances, in order to distinguish them. It was long ago remarked by an eminent practical writer, that there is no one pathognomic symptom by which fever, properly so called, may be discriminated.§

"Whether fever and inflammatory disease are identical, in respect to the disordered action which constitutes them, will be rather a matter of curiosity than of useful inquiry, so long as we remain ignorant in what inflammation really consists. While one party of physiologists declare that inflammation is increased action of the arteries, and another that it is diminished action,—while a third avers that the arteries are never capable of any action at all,—we cannot hope to throw much light on the nature of fever, even if we could prove that fever is inflammation. But the fact that these diseases are nearly allied, and that the former is very often productive of the latter, is of great practical importance, since we happen to understand, more fully than most other parts of medical practice, how to treat inflammatory disorders. And the fact I have just alluded to must be allowed, without controversy, by every person who is possessed of competent knowledge. Fever is only dangerous when it gives rise to, or displays, the symptoms of visceral inflammation.

"In that milder form of the disease which is termed simple fever, there is no certain proof of any local inflammation. The blood, however, when drawn, even under these circumstances, often exhibits, as

* *A History of the Fever which prevailed in Bristol during the years 1817. 1818, and 1819: founded on Reports of St. Peter's Hospital and the Bristol Infirmary.* By J. C. PRICHARD, M.D. late of Trinity College, Oxford; Fellow of the Linnean and Wernerian Societies, &c.; Physician to St. Peter's Hospital and the Bristol Infirmary. 8vo. pp. 112. 1820.

† *Annual Report of the Managing Committee, and Medical Report of one of the Physicians of the Fever Hospital in Cork-street, Dublin.* 8vo. pp. 44. 1820.

‡ *A History of the Epidemic Fever which prevailed in Bridlington and the Neighbourhood, in the years 1818 and 1819.* 8vo. pp. 173. Burgess and Hill, London.

§ Dr. G. FORDYCE'S Essays on Fever.

it was observed by Sydenham, the inflammatory crust; the state of the secretions is similar to the condition they assume in the phlegmasiæ; the relief also which is produced by venesection, and by other evacuations, tends to prove that the disease is in its nature analogous to these distempers. But this milder species is ever liable to be converted into a severer fever, exhibiting both in the living and the dead body unequivocal marks of inflammation in the brain, the lungs, the liver, stomach, or bowels. Simple fever most naturally and frequently degenerates into cephalic: indeed, the cases to which these names are respectively applied, are only distinguished by the different violence in the symptoms; the head is more or less affected with pains and other disorders, in both cases. The seat of these, in the more severe form, seem to be the membranes of the brain; and to the same parts, as well as to the coverings of the nerves, we may, with great probability, ascribe the pains in the head and limbs which attend the first attack of almost every case of fever.

“Simple and cephalic fever may therefore be considered as the genuine forms of this disease, the attack being milder in the first, and more severe in the second. The pneumonic, hepatic, gastric, enteric, and rheumatic, forms may be regarded as varieties.”

The evidence given before a committee of the House of Commons, in 1819; the treatise of Sir ARTHUR BROOKE FAULKNER; and the comments of Dr. ARMSTRONG on the observations of other physicians, have lately thrown much light on the nature of *the plague*, and obviated many of the erroneous notions respecting it which had been promulgated by earlier writers. Dr. Armstrong, especially, has shown its close analogy with *the typhous fever* of Europe. Yet, with this accumulated knowledge, there still remains so much to be ascertained as must render the acquisitions of subsequent observers, and the results of further experience, highly interesting: and hence the treatise of Dr. FRANK on the subject* has excited a degree of anxious curiosity it is but very ill qualified to gratify. Dr. Frank resided for several years, about the end of the last century, at Cairo and Alexandria, and during that time he was chiefly employed in collecting materials for a history of the disease above named. His opportunities for observation have been very ample, and his zeal and intrepidity in pursuing his inquiries were strongly manifested during the invasion of Egypt by the army of Buonaparte. The particular disquisitions of this author are preluded by some generalities respecting the epidemic prevalence and etiology of the plague, in which he evinces some notions that will probably be rejected by the greater part of modern pathologists; especially that of its being a malady of an asthenic nature, and never inflammatory.

The disease, Dr. Frank says, was unknown in Egypt previously to the time when it was subjugated by the Ottomans; and he thinks it came from either Constantinople, Smyrna, or Barbary. The people

* *De Peste, Dissenteria, et Ophthalmia Ægyptiacâ.* Auctore L. FRANK, M.D. suæ Majestatis Mariæ-Ludovicæ Archiducis Austriæ, Archiatro, &c. 8vo. pp. 223. Vienna, 1820.

of Egypt even now believe that they continue to receive it from the two former countries; whilst the Turks attribute its existence amongst them to its transmission from Egypt. The truth is, perhaps, that it originates in each of those countries endemically, as typhous fever does in Europe, and the yellow-fever, probably, in the West-Indies and America. VORDONI, who resided for fifteen years at Cairo, relates some circumstances which tend strongly to support this opinion. The pestilence can often be traced to an inland origin; and, in 1797, it appeared that an epidemic which commenced at Minniam, in Upper Egypt, as what is ordinarily termed bilious fever,* afterwards degenerated into the common form of plague.

Dr. Frank thinks that the ancient notion of its being connected with the overflowing of the Nile, is erroneous; but his judgment respecting its origin is prejudiced: he believes that the cause of it is primarily generated in the human body itself, in certain states of the system, and which are produced, he thinks, by misery and famine. Epizotic maladies and famine, he allows, are very frequently its precursors; two circumstances which have frequently happened previously to the development of the yellow-fever and the typhous fever of Europe.

The plague, in Egypt, ordinarily makes its appearance in the months of February, March, and April; and sometimes, though but rarely, in those of September, November, and December. Dr. Frank is satisfied that it is more prevalent, as well as more fatal, about the time of the new moon than at any other period. The number of patients is lessened when the wind blows from the north or north-east; and is increased when the south wind prevails; with which the temperature of the atmosphere ordinarily rises to from 100° to 106° . Almost all observers have concurred in asserting that it constantly ceases about the summer solstice; but Dr. Frank says, that its existence is occasionally prolonged until the middle of July. The atmosphere at this season is generally cooled by the prevalence of a north wind, and the temperature is also more nearly uniform than it is at any other season. Dr. Frank says, the malady totally disappears after this season in Egypt. He is firmly convinced that those who have supposed that it always exists to some extent, or *sleeps* for a time, as they have expressed it, have been deceived. In 1798, the progress of plague had ceased but about thirty days, at Cairo, when the French army occupied that city, and used the beds and clothing of the Mahometans who had lately fled from it, and yet a year elapsed from their arrival without an instance of the appearance of the dis-

* Dr. ADAM NEALE (in his *Travels through some parts of Germany, Poland, Moldavia, and Turkey*, published in 1818,) says, "The real truth may be, that plague is nothing but bilious remittent fever, under its worst possible form;" and he considers that the situation of Constantinople,—it being bounded in almost every direction by a great extent of marshy ground,—and the manners of its inhabitants, "fasting all day, gorging all night, wallowing in sensuality, or expiring from misery, clothed constantly in animal substances generally in a decaying state, without change of linen, and hovering all winter over the effluvia of charcoal," are sufficient to account for such people being the victims of pestilential poison.

ease amongst them. Buonaparte, himself, took as his residence the palace of Mourah Bey, on the western bank of the Nile, where sixty persons had died of the plague but a short time previously. These circumstances have been made the ground of an argument against the probability of the communication of the disease by *fomes* imparted to, and preserved in, apparel and similar things, without, at least, a concurrence of a certain state of the atmosphere: but this negative argument is opposed by others, positively in favour of the opposite opinion, against the validity of which there are no apparent objections; and a very forcible case of this kind is related by Dr. Frank. A European physician, who had for some time resided at Rosetta, left this city to go to Alexandria, where he was affected with the plague, died of it, and his effects were packed-up in a chest, without the circumstances becoming known to the officers whose duty it is to watch over the health of the public. The father-in-law of the physician, having come to Alexandria, carried the chest back with him to Rosetta, where the widow and himself opened it, that they might make use of what it contained. In a few days afterwards, this woman, then her father, and afterwards her mother and two of her children, were seized with the plague; and the disease afterwards spread over Rosetta, apparently from this source, though it was in the month of August, when its prevalence is a very extraordinary occurrence in Egypt.

Contrary to the inferences of the generality of physicians who have observed this disease, Dr. Frank says, the more rapid the appearance of buboes, the greater is the danger: others have thought that the occurrence of this symptom was a favourable sign. Perhaps these incongruities may be thus explained: that, in the more violent attacks of the disease, the patient generally dies before the stage at which bubo ordinarily appears: and hence, the coincidence of the milder cases with the formation of buboes, has been so misinterpreted as that the latter has been regarded as the cause of the former character of the malady. When the buboes are of a bright-red colour, Dr. Frank says there is some reason to hope a favourable issue. Diarrhœa, he regards as a sign of certain death. A general and equal sweat over the body, is the most favourable symptom; but the patient infallibly perishes if it is by any cause suppressed.

In the years 1798, 1799, and 1800, about one in five survived amongst those who were affected with it at Alexandria. But the natives recover in a greater proportion, though the only medicine they take is an infusion of the *panicum dactylum*. The disease in them is, probably, not so severe as it is in strangers.

The extent of the mortality at Cairo during one season, is about four-and-twenty thousand, when the malady is not prevalent in its most severe form; but in the latter case it often rises to seventy thousand.

Necroscopic researches have not been effected to any considerable extent in the subjects of the plague: from the observations which have been made, it would appear that the brain and nervous system are especially affected with disease of an inflammatory kind.

As an exposition of the "*new Italian medical doctrine*" was given in the first of this series of *Proœmia*,—not, it may be prudent to remark, from an impression of its truth, but from the consideration that a view of such systems as obtain a considerable proportion of the physicians of a nation as their sectaries, should be presented in the history of medicine,—it becomes right just to notice a recent work, in opposition to it, by Dr. GUANI.* It is, however, written too much after the manner of polemical disputations, and involves in its combatting arguments too many hypotheses, to have much influence against the doctrine it opposes. Several disquisitions, for the most part of a similar character, have been produced in support of the doctrine of the existence of *idiopathic fevers*; amongst which may be noticed that of Dr. CHOMEL.† The younger PINEL has also written on the same subject,‡ with the view of making it appear that the author of the *Nosographie Philosophique* had referred all fevers to organic lesions, as the consequences or effects of the latter; and that nothing but a change of terms is necessary to render his dissertations conformable with the doctrine just indicated. A very able essay in support of this doctrine, that fever in all its forms is but the result of local organic disease, has been produced by Dr. DUCAMP.§ The question above indicated is, however, one that is not likely to be settled for some time hence; for clinical researches, with a view to its solution, have been but too lately instituted to permit the necessary facts to have been attained to; and there are yet living too many physicians who commenced their professional career with the profession of the doctrine of the existence of idiopathic fever, to permit the contrary opinion becoming generally prevalent at the present period under any acquisitions of knowledge.

The elementary principles of the *new Italian doctrine*, as it is somewhat improperly called,—for it is merely a scion of Brunonism, fostered by that spirit which, with the help of a love of novelty, so commonly leads men to abandon altogether the opinions they once entertained, and assume such as are directly the converse of them, as soon as they become at all displeased with them, no matter how partially they may be erroneous, instead of preserving what is good|| in them, and correcting what is not so,—have been vigorously attacked by Professor MARZARI;¶ and it seems probable that this system will soon be overthrown by its opponents, even should it not be deserted by its partizans as soon as their passion for it has cooled. At present they seem to be as much enamoured of it as a young lover is of his mistress, and, like him, to regard as most beautiful just those features of it which others think its greatest deformities.

* *Del Controstimolo e delle Malattie Irritative.*

† *De l'Existence des Fievres.* 8vo. Paris, 1820.

‡ *Considerations sur les Maladies dites Fievres essentielles.* 8vo. Paris, 1820.

§ *Réflexions critiques sur un Ecrit de M. CHOMEL, &c.* 8vo. pp. 82. Paris, 1820.

|| The best of the late modifications of what is good in the principles of Brown, is, perhaps, that given by Dr. PEARSON, in the preliminaries to his "*Principles of Physic*," used as a syllabus to his Lectures.

¶ *Memorie Scientifiche e Letterarie dell' Ateneo di Treviso*, vol. ii.

Continued observation serves to prove the correctness of the views which have lately been taken of the nature of the *puerperal fever*, and further experience equally tends to show the propriety of the indications for its treatment which have thence been adopted, and which are almost exclusively due, originally, to British physicians. A pupil of Professor CHAUSSIER, who was for several years a resident student at the Lying-in Hospital *La Maternité* at Paris, has lately produced a work,* (a development of his Inaugural Thesis,) in which he presents abundance of evidence of the kind above alluded to. Although only congruous with the precepts given in the treatise of Dr. ARMSTRONG, it is perhaps right, in an historical record, to adduce the following theorems, founded on that evidence, as they result from experience under different circumstances of local influence from those of our English writers; a circumstance to which much importance has been attributed by some authors who entertain different opinions of the nature of the disease.

" 1°. Excepting in the cases where there exist some particular and manifest contra-indications to the use of local, and especially of general, evacuation of blood, in the treatment of *puerperal peritonitis*, this measure should be the first to be employed:

" 2°. In order that bleeding may be followed by success as certainly as we can ever expect in the treatment of diseases, it should be employed in the first stage,—that is to say, in by far the greater majority of cases, before the termination of the first day of the disease; and, moreover, that a sufficiently abundant quantity of blood should be taken away to destroy at once all the violence of the malady.

" 3°. When employed not in conformity with those two essential conditions, sanguineous evacuations, far from being useful, always become very injurious to the patient, by destroying her strength and the resources of nature, without having a durable influence against the disease.

" 4°. In the cases,—and these are the most common instances,—where constipation is one of the attendant circumstances of the disease, another indication, not less pressing for its fulfilment, is joined with that of blood-letting, which is to excite alvine evacuations, by gentle, or even by somewhat active, purgatives, and thus to maintain a sort of artificial diarrhoea during the whole of the duration of the disease; a duration which, in the greater number of cases where the termination is favourable, does not exceed the space of from two to four days.

" 5°. When a diarrhoea exists from the commencement of the disease, nothing should be done to suppress it when it is moderate: when, on the contrary, it appears excessive, none but mild and soothing means should be employed,—such as emollient clysters, mucilaginous drinks, and some opiates; and we should by no means add to the irritation by the administration of astringents."

A few isolated facts respecting a peculiar lesion of the brain, in

* *Reflexions et Observations sur l'Emploi des Saignées et des Purgatifs dans le Traitement de la Péritonite Puerperale.* 4to. pp. 78. Paris, 1820.

which the structure of this organ is, to a greater or less extent, converted into a pultaceous mass, were related in a former Number of this series of Essays. This subject has for some years formed an especial object of the researches of Dr. ROSTAN, and he has lately published the results of them in an *ex-professo* treatise.*

There are, he says, two distinct and well-marked periods in the disease.

The first presents only some vague phenomena which happen in numerous other affections, and which in themselves are of hardly any value as signs, though they become of the highest importance, and serve considerably to characterize the malady, when it is manifested by the symptoms of the second period. They are, indeed, of so much importance, that, when they have not existed, or when they have not been observed, the diagnosis is extremely obscure. The following are the precursory phenomena above alluded to:—The patient is afflicted with a fixed pain in the head, which is obstinate and intolerable. This pain ordinarily resists all the means employed for its alleviation: it continues for a greater or less length of time, which may be several days or several months. This pain is not a constant precursor of the disease. Vertigo renders the step of the patient vacillating; the intellectual faculties become dull; the perceptions are slow; the judgment is not exerted without much difficulty; the memory is weak and treacherous; the imagination is extinct; the ideas are confused. The answers of the patient are, however, proper; but they are given in a tardy manner, and only after very long consideration. The tongue may be embarrassed in its movements, but sometimes the patient expresses himself with remarkable rapidity; his disposition of mind changes, he becomes morose, taciturn, peevish, and sometimes indifferent to external objects. There is often much tendency to sleep. The patient does not become delirious, but the persons who are about him perceive that his intelligence is not quite in its natural state. Besides those symptoms, creeping sensations, and often numbness, are experienced in one of the limbs, ordinarily towards the fingers; there is difficulty in seizing objects, or else a stiffness of the limb very often, approaching to a permanent contraction of it: its sensibility is not always diminished in proportion to its contractility; sometimes, indeed, it happens that the sensibility is so much increased that the slightest touch causes the patient to cry out. These pains cannot be confounded with rheumatism, as there is neither heat, redness, nor tumefaction; phenomena which always accompany rheumatism when it is acute. Sometimes there is delirium, with extreme agitation and febrile symptoms; and, lastly, mental alienation, senile demency, often accompany this softening of the brain, as Mr. GEORGET, as well as Dr. ROSTAN, have ascertained.

Intolerance of light, and strabismus, have but rarely been observed by Dr. ROSTAN to precede the lesion under consideration; but a dimness of sight, and even blindness, are not unfrequently precursory

* *Recherches, sur une Maladie encore peu connue qui a recue le Nom de Ramollissement du Cerveau.* Nouveau Journal de Médecine, tome ix.

phenomena. There is often ringing noise in the ears; occasionally the slightest noise is insupportable, but more frequently the acuteness of the sense of hearing is diminished. The smell and taste are but rarely so deranged as to lead the patient to complain of them. Many of the phenomena just enumerated, the author acknowledges, are common to this disease and ordinary apoplexy.

Such are the precursory affections of relative or animal life: the functions of organic life are also often deranged during this period. The appetite is diminished, the thirst increased; digestion is difficult; the mouth is clammy, and the tongue white. Nausea, and even vomiting of an abundance of green, porraceous, bilious matter, often ensue. The epigastrium, as well as the rest of the abdomen, is very sensible to pressure. Diarrhœa is sometimes present, but more frequently there is constipation, or rather a tardiness in the evacuation of the rectum. The stools are but rarely passed involuntarily at this period: it is not so with the evacuation of the urine, which the patient, for the most part, has much difficulty in retaining. The quantity of the latter secretion is often less than natural. Respiration is but very rarely disturbed, although the patient sometimes complains of a slight sense of suffocation, or of oppression of breathing: it is more frequently inordinately slow than quickened, though difficulty of breathing is ordinarily manifested with acceleration of it. The pulse is very variable; it is but rarely increased in frequency; it is sometimes inordinately full, and in certain cases it is slower than natural. The symptoms presented by the other functions, as those of *absorption*, *exhalation*, the *secretions* and *excretions*, and *nutrition*, furnish no constant phenomena, nor such as are worthy of being noticed.

It is not rare for some intense internal phlegmasia, thoracic or abdominal, to precede the softening of the brain. Dr. Rostan has seen it, in some cases, preceded by a general inflammatory diathesis; all the viscera were inflamed, the lungs were hepatized, the pleura covered with false membranes, and the digestive tube inflamed throughout its whole extent. These last examples are but rare. It is more common to see the symptoms of the second period of the disease show themselves in a person who has been affected only with violent enteritis, or an intense peripneumony.

In the *second period*, the patient, after having presented some of the signs above enumerated, either gradually in a more or less rapid manner, or suddenly, loses the use of one of his limbs, sometimes of one-half of his body. If he is standing, he falls. It even happens, when he is lying down, that he falls from his bed on the floor. Most frequently he does not lose the use of his senses, and preserves his intelligence; but he has extreme difficulty in replying to the questions put to him, and it is often only by gesticulations that he expresses himself. There are cases in which the coma is perfect, but these cases are rare. When the comatose state has come on suddenly, as well as the paralysis, the patient commonly regains his consciousness on the day after the attack, and the inexperienced physician does not fail to felicitate himself on the success of his treatment; but new accidents soon take

place to dissipate his illusion. The symptoms are again aggravated; intelligence and the sensual functions are entirely abolished; the patient falls into a state of complete coma; his limbs become immovable; and he dies at the end of a few days, ordinarily between the fourth and the fifteenth, presenting in the greater number of cases the symptoms of typhous fever.

The state of the limbs, in the course of this period of the disease, is not the same in all patients. The most frequent is that of diminution or abolition of the power of motion. The patient frequently experiences a sense of numbness, great heaviness, creeping or stinging sensations, and at last intolerable and shooting pains, increased on the limb being touched. It is not very rare to observe a great degree of stiffness, or an invincible contraction, of the affected side. The forearm is bent on the humerus; there is much difficulty in bringing the limb into the straight position, and this is then preserved only for a moment. A much more rare state of the limbs, is that in which they are affected with convulsions.

The face may be either pale or strongly injected with blood, according to different circumstances. The pain in the head, which existed before the manifestation of the symptoms of the second period, now increases in intensity: this symptom even comes on when the patient had not previously suffered it. If he be asked where he feels uneasiness, sometimes after the first, but more frequently after the second or third, time of being thus questioned, he carries the arm, of which he preserves the power of motion, to some part of his head. It is remarkable that this is, almost always, precisely on the seat of the disease, and on the side opposite to that which is paralytic. When the patient has been delirious, the delirium persists after the occurrence of the palsy, but it is now more taciturn. Vomiting, first of food, then of bile, is often observed. The belly often furnishes evidence of extreme sensibility. Sometimes the feces are passed involuntarily, but more frequently there is constipation of the bowels. The urine generally escapes unknown to the patient; the respiration is often oppressed. The pulse is sometimes more frequent, and stronger, than ordinary.

Dr. Rostan has seen but one case in which the symptoms diminished in severity in the second period of the disease, and in this instance they soon returned with their former intensity. The progress of the disease is, then, essentially continuous, and it is always increasing in violence. There are, besides, evening paroxysms, when inflammation of any organ is present.

Dr. Rostan has not yet been able to ascertain any thing of a positive kind respecting the predispositions and causes of this malady: he can only state that, although he has for several years exercised the duties of physician to a dispensary at Paris, where the number of patients of every age is very considerable, he has never yet seen the disease in any but old persons; a circumstance which differs from the experience of some former writers on the subject.*

* The observations made by Dr. Rostan in his necroscopic researches respecting this disease, will be noticed under the head "Pathological Anatomy."

Dr. LALLEMAND, who has lately published some observations respecting diseases of the brain, characterized by a remarkable degree of perspicuity and precision,* has taken a different view of the origin of this disease, and considers that it is the result of an inflammatory affection. He has, however, supported this proposition by ingenious hypothesis rather than by precise inductions from facts.

An interesting case of *angina pectoris*; which tends to show the propriety of the views that have been generally taken of this disease by English physicians, at the same time that it proves that considerable organic lesion may exist about the origin of the aorta without permanent obvious functional derangement,—has lately occurred to the observation of Dr. JEMINA, of Mondovì.† This case may, also, be perhaps considered to support an opinion expressed on a former occasion, that inflammation of vessels may be a cause of ossification of them. The patient was a man 56 years of age, who had enjoyed good health for thirty years, when he was attacked with what appeared to be bronchitis. About a month after this, the paroxysms of *angina pectoris* occurred; at first after an interval of a month, and then at shorter periods, until several occurred in a day. In the early stages of the affection, it was sufficient for him to stand still for a few instants, and keep himself upright with his arms extended, or to lie down on his back, to avert a threatened paroxysm. The general symptoms were similar to those of ordinary cases of what is termed *angina pectoris*. He died at the end of about six months. The brain, abdominal viscera, and lungs, were perfectly sound. The heart seemed to Dr. Jemina to be somewhat smaller than ordinary. The coronary arteries were dilated, full of blood, and enveloped in a good deal of fat in the situation corresponding with the right ventricle. The semilunar valves of the aorta were indurated. There was no other apparent lesion about the heart. The parietes of the arch of the aorta were thickened from effused coagulable lymph, and presented numerous vessels running along their exterior surface filled with red blood. The thickening of the aorta increased as it approached the heart. Between the internal and middle tunics, there were numerous points of ossification, in scales or plates, which also increased in contiguity towards the origin of the vessel; so that, about this part, the vessel was so hard that it could not be divided by a scalpel without much difficulty.

The limits within which this Sketch should be confined are so nearly approached, that it becomes necessary, in the ensuing part of it, to refer merely to several matters which should form subjects of it, and which were considered in a particular manner in the last volume of

* *Rochéres Anatomico-Pathologiques sur l'Encéphale et ses Dependances.* Par F. LALLEMAND, Professeur de Clinique Chirurgicale à la Faculté de Montpellier, Chirurgien en Chef de l'Hôpital Civil et Militaire de la même Ville, &c. 8vo. Montpellier, 1820.

† *Annali Universali di Medicina*, No. xlii.

the Medical and Physical Journal. Amongst which, should be noticed, in this place, the treatises of Professor RUDOLPH* and Dr. BREMSER† on the *entozoa*, or animals which inhabit the bodies of other animals: the former relating especially to their natural history, the latter to their origin and the means of removing them.

It is necessary to refer in the same manner to the memoirs of Dr. HARRISON on those diseases of the spine which are attended with unnatural curvatures of it,‡ in which he has presented some new views of the origin of these affections, and traced, with much perspicuity, their influence on the functions of the system in general. His observations will, however, be noticed in a particular manner in a subsequent Historical Sketch, when he shall have completely developed his sentiments on this subject: at present it may be stated, that he has shown that the spinal distortion, in by far the greater proportion of cases at least, originates from a morbid state of the *ligaments* of the spine, without any disease of the bones or cartilages being present,—the unnatural curvatures taking place in consequence of the want of power in the ligaments to retain the vertebræ in their proper relative situations. This original view of the nature of those affections has led Dr. Harrison to adopt a new mode of treating them that is not less remarkable for the rapidity with which it effects a perfect cure of the disease in old cases, than for its efficacy in, the writer believes, every instance in which it has hitherto been employed. From the evidence already published, it would appear that the means here alluded to may be regarded as one of the most important of the modern improvements in the practice of medicine.

Various ideas have been entertained at different periods respecting the mode in which the union of fractured bones is effected, and though it is a subject which, it may be supposed, might be readily elucidated by necroscopic researches, pathologists of the present day do not at all agree in their notions respecting it. The diverse opinions now prevalent may, however, be arranged under those which were established by DUHAMEL, (or rather those which Duhamel took from GALEN,) and those of BORDENAVE; and the different views here alluded to appear to have originated only from a want of sufficiently extensive inquiries respecting the progress of the union in its different stages; for, by pursuing such inquiries, Mr. DUPUYTREN has been led to the knowledge of facts§ which seem to reconcile them with each other.

Duhamel was led, by his experiments and observations, to consider the re-union of a fractured bone to be owing to a tumefaction, elon-

* *Entozoonum Synopsis*. 8vo. pp. 811. Berolini. See *London Medical and Physical Journal*, No. 261.

† *Ueber lebende Wurmer im lebenden Menschen*. 4to. seit. 284. Wien. See *London Medical and Physical Journal*, No. 262 and 264.

‡ See *London Medical and Physical Journal*, Nos. 261 et subsequ.

§ *Exposé de la Doctrine de M. le Professeur Dupuytren sur le Cal, avec des Observations à l'Appui*. Par L. J. SANSON, Docteur en Chirurgie, &c.

gation, coaptation, and adhesion of the periosteum and medullary membrane of one of the fragments, with the periosteum and medullary membrane of the opposite fragment: to this process succeeds that of ossification of the points of union; the result of which is the formation of two annular cylinders, one situate towards the centre, the other at the circumference of the ends of the fractured bone, which are thus united. Duhamel also observed, that in some instances only one of the membranes above mentioned underwent this process; in which case the union was not the less complete. Here his observations stopped. He was not at all acquainted with the phenomena which succeed to that process; and which, from his views failing to account for the results of them, caused his explanations to be generally regarded as incorrect.

Bordenave was led by his observations to consider the union to be effected by means of effused coagulable lymph, which, after a certain length of time, became vascular, organized, and then interspersed with ossific matter; and he believed that the effusion took place from the whole disunited surfaces of the bone, without any particular dependence on the periosteum or medullary membrane; and he was induced, from examining the parts too late, (as it would appear from Mr. Dupuytren's observations,) to consider that no such cylinders as those spoken of by Duhamel were ever formed. The notions of Bordenave have been adopted by BICHAT, SCARPA, BRODIE, and by far the greater part of modern pathologists.

Professor Dupuytren has had very ample opportunities for the investigation of the facts relating to this subject, by means of his office as surgeon to the Hôtel Dieu, and his duties as "chef des travaux anatomiques" of the Faculty of Paris, led him to cultivate them with assiduity: the results of them, as already hinted, have made him conclude that the views both of Duhamel and Bordenave were correct; but that those of the former had been confined too exclusively to the early stages, and those of the latter, in a similar way, to the consequent results in the advanced periods of the re-union.

Prof. Dupuytren distinguishes the means of re-union of a fractured bone into two distinct species: the first, which is that described by Duhamel, he terms the *provisory callus*; the second, which is that of Bordenave, the *definitive*. They are, he says, effected in the following manner:

At the instant of the fracture of a bone, the medullary membrane, the medulla itself, the periosteum, and the cellular tissue, are lacerated; blood escapes from the torn vessels, it surrounds the fragments, flows into the medullary canal, and infiltrates the surrounding cellular texture. A slight degree of inflammation is then developed in all those parts. The cellular tissue becomes red and tumid, from its minute vessels being injected with blood; it becomes condensed, thickened, loses its elasticity, and acquires a considerable degree of consistence: it sends irregular prolongations into the interstices of the muscles, alters their organization, makes them participate, to a greater or less extent, in the changes which it suffers, and unites or confounds them with the periosteum; which, on its part, is thickened

and penetrated by a close net-work of fine red vessels. The marrow also becomes inflamed, tumid, and indurated, and then of a grey or whitish hue. The medullary canal is contracted in diameter, by thickening of its membrane, which becomes red, and as it were fleshy, in consequence of a sort of gelatinous infiltration. The coagula resulting from the primary sanguineous effusion are absorbed, and entirely disappear. A fluid of a viscous, filamentous, and sometimes gelatinous, appearance is thrown out between the fragments. Sometimes, also, a reddish, and somewhat tomentous, substance is developed between them, which originates between the inequalities they present, by red points, which rise, expand, meet, and are confounded with each other. This latter production never acquires any considerable degree of density: it unites interiorly with the medullary membrane, and externally with the tumefied soft parts. It does not always exist; and, when it is present, the viscous or gelatinous matter previously described is wanting. Both those substances appear to perform an important part in the construction of the *definitive* callus. After they are formed, the *second period* of the uniting process commences: this extends from the tenth or twelfth to the twentieth or twenty-fifth day.

The engorgement of the soft parts now diminishes. The tissue of the muscles re-assumes its distinctive characters, and these organs recover to a certain extent their freedom of motion. The cellular texture, however, remains condensed. The existing swelling is concentrated about the fracture, and there is soon a tumor very distinctly separated from all the surrounding parts, not even excepting the adjacent tendons, which it more or less embraces; presenting, however, grooves in which they run. This tumor is homogenous in its tissue; its colour is whitish or light-grey colour; it is firm in consistence; and its resistance is analogous to that of the fibro-cartilaginous structures, like which, it also creaks under the instrument which divides it. Its most deep-seated strata, formed by the periosteum of the fractured bone with which its tissue is confounded, are more adherent to the bone as it approaches the broken extremity, where it is difficult to separate the two structures. Towards the remote extremities of the tumor, the periosteum becomes distinct, and can easily be detached from the bone. The medullary membrane—tumefied and united with the matter with which it is infiltrated—sometimes obliterates the canal, even to some distance beyond the fractured points. The plug it forms rapidly passes to a cartilaginous, and afterwards, with yet greater rapidity, to the osseous, state; and is confounded, at the level of the fracture, with the whitish, rose-coloured, red, viscous, gelatinous, or tomentous, substance which is interposed between the fragments; and which loses itself, on the other hand, in the exterior callus.

The *third period* extends from the twentieth or twenty-fifth day, to the thirtieth, fortieth, or sixtieth, according to the rapidity of the process, or the age and constitution of the patient. The formation of cartilage now proceeds from the centre of the tumor towards its circumference, and ossification rapidly follows it; until, at length, the whole of the tumor has become of a bony nature. The periosteum, thicker than in the natural state, becomes distinctly apparent, and

Vetch desires to impress on the mind of the reader of his dissertations, must immediately become familiar, even to one who was previously almost wholly unacquainted with the subjects of them; and, in drawing his inferences from facts and forming his general principles of pathology, he has, also, designated in so admirable a manner the striking traits of the objects to which they relate, that he develops, in a few sentences, views of the most interesting kind, which present a multitude of important indications to a reader even of very ordinary powers of reflection.

The word *ophthalmia* has of late years been indiscriminately applied to inflammation of the eye itself as well as of its investing membrane, the conjunctiva, though it was originally employed by HIPPOCRATES as a term for catarrhal or purulent inflammation of the conjunctiva; the different forms of which were distinguished by the addition of specific or qualifying terms, to which the successive writers of the Greek, Roman, and Arabian schools implicitly adhered, but which have long ceased to convey any accurate or practical grounds of distinction. Dr. Vetch again restricts the use of the word *ophthalmia* to the sense above stated: inflammation affecting the eye itself, and cognizable as an external disease, he designates by the appellation of *sclerotic inflammation*, or *ophthalmitis sclerotica*, as the term which most distinctly describes the seat and nature of the disease.*

In conformity with this view, Dr. Vetch divides the diseases of the parts above designated into two genera, the first of which comprises the various forms of *ophthalmia*, or conjunctival inflammation; the second, those of *ophthalmitis sclerotica*.

Ophthalmia presents two species of inflammation:

1°. The catarrhal ophthalmia, or *ophthalmia mitior*, sporadic, endemic, and epidemic, with or without chemosis.

2°. Puriform, or *ophthalmia purulenta*, *ophthalmia gravior*, the *lippitudo*,† *ophthalmia vera*, *ophthalmia humida* of the ancients, *blepharoblenorrhœa*, and *ophthalmoblenorrhœa* of the German ophthalmologists.

a. Ophthalmia of infants.

b. ——— produced by the infection of ophthalmic virus..

c. ——— by the infection of gonorrhœal virus.

d. ——— by the metastasis of gonorrhœal inflammation.

e. Rheumatic, syphilitic, and arthritic.

○ Ophthalmitis sclerotica also presents two modifications:

1°. Idiopathic, or corneal.

2°. Iridial, or symptomatic.

* It might appear that *iritis* could not be properly comprised by this appellation; but the author, when treating of this affection, remarks, that, although inflammation may commence in the iris as a primary affection, while the redness of the sclerotic coat appears to be secondary or symptomatic, "in either case, the formation of iritis is so blended with a modified inflammation of the sclerotic coat, that it is impossible to separate the consideration of the one from the other."

† "The misapplication of *lippitudo* to a chronic and glandular disease is perhaps too inveterate to be now rectified; in its original sense, it applies to the acute stage of ophthalmia."

To those primary forms of disease Dr. Vetch traces nearly the whole of the morbid affections of the eye, as the proper sources of their origin: he, therefore, treats of the consequent maladies here alluded to in the histories of the former; such a way of regarding them being the only one calculated to explain their nature in a perspicuous and methodic manner.

The term *catarrhal*, rather than *purulent*, is applied by Dr. Vetch to the ordinary ophthalmia of temperate climates from atmospheric influence, although the discharge "may, and does," he says, "generally assume more of the properties of pus than of mucus;" but he chooses to reserve the term *purulent* for the more severe form of ophthalmia which occurs from inoculation or infection.

Dr. Vetch, on all occasions, preludes his particular disquisitions with some general observations and reflections on the physiological and pathological relations of the parts concerned in the diseases of which he is about to treat; and thus, after the manner of PINEL in his Nosography, he prepares the reader for the ready comprehension of the particular details into which he subsequently enters. After pointing out, with accuracy and conciseness, adducing here and there original observations, the most remarkable circumstances in the pathology of mucous membranes, and showing their especial liability to become diseased from atmospheric influence, he adverts to the ophthalmia of Egypt; a country which, of all others, seems to be the most favourable to the production of inflammation of the conjunctiva; and the causes of which he, in the first place, endeavours to determine. He shows, satisfactorily, that it is not, as it has commonly been supposed, on the sands, or the influence of the drying winds, that it depends; but on the extreme humidity and relative coldness of the atmosphere during the night. An opinion very early published by Dr. Vetch, in the fourth volume of the Edinburgh Medical and Surgical Journal, and in which ASSALINI and Mr. POWER coincide with him; though each of them seems to have been led to it, nearly at the same time, by his own observations.

Dr. Vetch considers that ophthalmia may first arise from atmospheric influence, and afterwards be propagated by contagion, by the matter formed being communicated from the eyes of the diseased to those of the healthy; a proposition which he has satisfactorily and originally established in regard to the ophthalmia acquired by the British army in Egypt, in the commencement of the present century, and afterwards propagated in several parts of England and Ireland, when the soldiers affected with it were dispersed on the return of the army. He indeed goes so far as to advance as a general proposition, that, "from whatever cause inflammation of the conjunctiva may originate, when the action is of that nature or degree of violence as to produce a puriform discharge, the discharge so produced operates as an animal virus, when applied to the conjunctiva of a healthy eye."

A very curious fact respecting this ophthalmia, analogous to that for which Mr. JESSE FOOTE first argued in regard to gonorrhœa, which Dr. Vetch has ascertained, is that the purulent matter from the eye of one man has the power of infecting the urethra of another man, and

of producing gonorrhœa; whilst it is innoxious to his own urethra. This law the author has never known to be violated. This subject is more extensively discussed in the chapter on *gonorrhœal ophthalmia*, and the disquisition necessarily comprises some reflections on the laws of metastasis of diseased action; because it has been made a question in respect to gonorrhœal ophthalmia, whether the ophthalmia arises from *metastasis*, (or, as it is with more propriety termed by Mr. PRING, *related extension of disease*,) or from the application of the gonorrhœal virus of the patient, or of another person, to the eye which has become affected.

If ophthalmia be the result of the application of gonorrhœal matter of the same person to the affected eye, it is obvious that the law above stipulated for is violated; and ophthalmia co-existing with gonorrhœa would appear to be too frequent an occurrence to permit the supposition that it has arisen from the contact of gonorrhœal matter of another person. In order to ascertain more satisfactory information on this subject, Dr. Vetch applied the matter formed by the eye in ophthalmia to the urethra in several instances; but no disease ever resulted from it, when the inoculation was effected in the subject from which the matter was taken; whilst in one case, (the only experiment, it appears, of this kind,) the ophthalmic matter produced very severe gonorrhœa in another individual than that from whom it was taken. Dr. Vetch was led, from these results, to regard the converse view of the subject as a fact. "I could no longer admit," he says, "the possibility of infection being conveyed to the eyes from the gonorrhœal discharge of the same person. Some time after this, the improbability, or rather impossibility, of this effect was rendered decisive by an hospital assistant, who, with more faith than prudence, conveyed the matter of a gonorrhœa to his eyes, without any affection of the conjunctiva being the consequence. From this time I was led to look for an explanation of the connection subsisting between gonorrhœa and ophthalmia, arising in the same person, in some peculiarity of the constitution; and to conclude, that the disease is an extension of an inflammation which first showed itself in the urethra, and of which the different structures of the eye are liable to participate, in common with many other parts."

The inflammation of the sclerotica is very frequently, in the cases under especial consideration, communicated to the conjunctiva; and so is decidedly idiopathic inflammation of the sclerotica from other causes. It is but seldom, Dr. Vetch believes, that "the gonorrhœal action is translated to the conjunctiva, without attacking the sclerotic coat also." These facts strongly favour the author's opinion of the nature of gonorrhœal ophthalmia, as it most ordinarily occurs; and several others are also brought forward by him in its support, that are equally forcible.

On treating of the *purulent ophthalmia of infants*, Dr. Vetch remarks, that the most remarkable point of diversity between this affection and the conjunctival ophthalmia of adults, depends on the greater facility with which inflammation is transferred from the conjunctiva to the sclerotica in infants than in the latter subjects: and

hence destruction of the cornea occurs in the former with much the greatest rapidity. Its most frequent causes are considered to be some morbid secretion in the vagina of the mother, and an atmosphere imbued with animal effluvia; and hence (that is, from the latter cause,) it is a disease of very frequent occurrence in lying-in hospitals, where the greatest attention is not paid to ventilation. Want of due cleanliness, and negligent exposure of the head to inordinate cold, seem also to be productive of it. Sometimes it ensues as a consequence of chronic inflammation of the glands of the eye-lids. The disease, Dr. Vetch adds, seldom ceases in less than six weeks, and often continues from three to four months before the patient opens his eyes without assistance.

In the physiological remarks with which Dr. Vetch precludes his disquisition on ophthalmitis sclerotica, he states that the *cornea*, although it differs from the proper sclerotica in its minute anatomy, is nevertheless a strict continuation of the latter membrane, *as far as concerns its vascular connection*; and that inflammation does not take place in the cornea, and consequently neither suppuration nor ulceration, until inflammatory action has been set up in the vessels of the proper sclerotica, even when the exciting cause is applied directly to the substance of the cornea. No symptom of re-action is visible until that part of the sclerotic coat which is nearest the injured portion of the cornea has put on the appearance of inflammation: on the other hand, idiopathic inflammation once excited in the vessels of the sclerotic coat, invariably tends towards the cornea; and there, in consequence of the more destructible nature of the part, the common consequences of inflammation take place,—such as effusion of lymph and ulceration.

“The intimate connection formed on the internal surface of the sclerotic coat with the iris and the ciliary structure,” Dr. Vetch remarks, “would naturally lead us to expect that the inflammation would have an early tendency to seize upon these parts: attentive observation, however, will prove that, in ordinary or idiopathic inflammation, any apparent affection of the iris is more the effect of sympathy than of the actual presence of disease, and that the supervention of iritis is, according to my view of the subject, to be considered as a distinct form of sclerotic inflammation, connected with some idiosyncrasy, or morbid diathesis, previously existing in the constitution; and is, for the most part, more insidious in its progress than violent in its symptoms. As, before any degree of acute inflammation can establish itself either in the cornea or in the iris, a similar action has taken place in the sclerotic coat, so the farther progress of disease, in either of these parts, continues to be indicated by the greater or less activity of the inflammation in the sclerotica, and to this appearance too much attention cannot be paid. The feelings of the patient, the stationary appearance of other symptoms, may lull the practitioner into a fatal security, which he will best avoid by making the condition of the sclerotic coat the only safe test of the arrested progress of the disease.”

In further support of those views it is remarked, that injury done

to the iris by accidental or artificial wounds excites no disturbance in the part or uneasiness to the patient, and will eventually heal without any troublesome symptoms, if inflammation does not appear in the sclerotic coat also; but in that case the structure of the iris is liable to be destroyed, with all the signs of active and acute disease.

There is a diversity in the original seat of sclerotic inflammation, in the opinion of Dr. Vetch, as it arises from external or local sources of irritation, and as it results from "some more general and pre-existing disease of the system," which has not been before remarked. In the former case, the inflammation occupies the external surface of the sclerótica, and is termed *sclerotico-corneal inflammation*: in the latter, it has a tendency to proceed to the choroideal coat, when it is commonly known as *iritis*; this Dr. Vetch terms *sclerotico-choroideal inflammation*; and he thinks it of great importance that it should be understood that, "whether the inflammation proceeds from the iris to the sclerotic coat, or *vice versa*, the inflammation has its active character and basis in the structure of the sclerotic coat." This form of the disease has a rheumatic character, without any definite termination, and may occur as the local manifestation of a rheumatic and arthritic diathesis. It is frequently met with as a symptom of syphilis; and apparently, also, as the effect of a mercurialized state of the system, where no syphilitic taint can be suspected.

In the first species of sclerotic inflammation—the *sclerotico-corneal*,—the first set of symptoms, Dr. Vetch says, may be considered as generic or common to both species, and may be comprehended under the following heads: "An increased vascularity of the part; morbid sensibility to the impression of light; contraction of the pupil; pain; heat; augmented secretion of the lachrymal fluid; pyrexia." In his particular description of the phenomena of this affection, he adds, that the encroachment of the vessels on the margin of the cornea, (as they radiate from the angles of the orbit to the centre of the eye,) and an immediate intolerance of light, are inseparable consequences. It is stated that the iris is seldom more safe from any actual attack of inflammation than when this action is proceeding with its utmost violence in the substance of the cornea; and that we never find the urgency of intolerance of light so distressing as in those cases where extensive destruction of the cornea is going on. The morbid sensibility to light keeps pace with the visible inflammation of the sclerótica and the cornea, both in its advancement and its decline. The views here indicated are illustrated by a multitude of original pathological observations, and the inferences which Dr. Vetch has drawn from them are established by several very forcible arguments.

Hypopion, opacity, and ulceration, and various other lesions of the cornea, to which different names have been given, proclidentia of the iris, and staphyloma, are consequences of the disease just alluded to: but a detailed account of their mode of origin and progress cannot be comprised in the limits of this abstract.

Dr. Vetch is not perfectly satisfied with the explanations of either Dr. FARRE or Mr. TRAVERS, of the curious circumstance that mercury, although it seems to act as a cause in the production of *iritis*,

is nevertheless one of the most efficacious remedies for it when established. He offers himself an hypothesis of the relations of those supposed facts, which is founded on the opinion that inflammation immediately results from the absorbent veins not carrying forward the blood with an activity proportionate to that by which it is brought to them by the arterial capillaries. Mercury excites the action of the absorbent veins, and thus produces a due relation in those functions, which is consequently followed by the disappearance of the inflammation. In endeavouring to account for the production of the inflammation by mercury, he says, "in proportion as the functions are excited beyond their natural standard, any sudden check given to the mercurial influence will necessarily interrupt the balance which it has established in the state of the circulation, raised at the same time beyond the natural standard. According as the operation of the interrupting cause is general or local, the effect will be a state of general fever or topical inflammation."

Dr. Vetch, however, in admitting the influence of mercury in the production of iritis, does not seem inclined to regard its operation as direct: he appears to consider it as acting only by "intermediate relations," as Mr. PARCER would express it; and, viewing the subject as it is illustrated by the doctrines of causation of this physiologist, there is not the least difficulty in conceiving how the disease may be both produced and cured by means of mercury.

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"In the inflammation of the iris by continuity, the conjunctival vascularity is more conspicuous and diffused, and the cornea is so much clouded as partially to obscure the view of the iris; the albuminous deposit is wanting, or, if any has taken place, it is small in quantity, white, flocculent, and partially diffused in the aqueous humour, or is deposited at the ciliary margin of the iris, forming a lymphatic hypopion; the pupil is little, if at all, misshapen. Although the vision is much bedimmed, there is greater susceptibility to the painful impression of light."

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rotic coat is liable to happen, that it can no longer preserve its regular spherical form, but suffers a partial projection in some part more than another, known by the name of *staphyloma sclerotica*. Hypopion, cataract, and amaurosis, are other occasional consequences of sclerotic iritis.

Lenticular and capsular inflammation, is considered by Dr. Vetch to be the common origin of cataract. This inflammation is generally of a chronic kind, but the lens is also subject to a more acute and destructive form of inflammation, which, in contra-distinction to that producing the more gradual loss of transparency terminating in the lenticular and capsular cataract, may be termed *lentitis*. Cataract exemplifies the adhesive process of inflammation; this, the suppurative. When suppuration is established, ulceration of the cornea comes on in process of time, and the aqueous humor is evacuated. Dr. Vetch seems to regard the diagnostic remarks on cataract and amaurosis proposed by Professor BEEB as the most accurate.

When treating on *amaurosis*, Dr. Vetch adduces some forcible illustrations of a pathological principle, apparently true, which he seems particularly desirous to establish, and the knowledge of which is of great importance in regard to practice. He had already remarked, on speaking of iritis, that inflammation of the nerves of the senses properly is not accompanied with increased sensibility, as is the case with the other nerves; but, on the contrary, with a diminution of their proper sensibility: hence, inflammation, or increased vascularity of the retina, instead of being attended with increased power of perception of light, is productive of amaurosis. It is common to hear this affection spoken of as a state of *debility*, or want of excitement of the optic nerve, and not of inflammation, on the analogy that irritation of nerves in general is accompanied with increased sensibility; but, Dr. Vetch remarks, it will be found that the causes of amaurosis are often those which are productive of increased determination of blood to the head, and to the eyes especially; and he, by a series of arguments, shows that amaurotic blindness may be the consequence of direct inflammation of the retina, or of the vascular structure on which it rests. Whilst he establishes this point, he takes care also to point out that it may in some cases arise from "local debility influenced by repletion of the system, obstructed circulation," and from "actual loss of power, from the natural decay of age or protracted debility." He however considers that, excepting when it occurs at a very advanced period of life, it is almost always a consequence of some pre-existing disease.

Mr. Travers has known it arise from a wound of the scalp, caries of the skull, abscess and caries of the antrum maxillare, a large abscess under the masseter and muscles of the neck, and an abscess at the extremity of a molar tooth, whilst the crown of the tooth was sound. Excessive use of the visual faculty, and various disorders of the digestive organs, have been observed by him, also, as sufficiently obvious causes. Mr. Travers hence draws an important distinction between amaurosis from functional disorder and that from organic lesion; the sensible diagnostic appearances of which, presented by the

organ itself, are minutely traced by him. It is a frequent sequel of epilepsy, and other nervous disorders; and here Mr. Travers says, "it is a hopeless form of the malady:" with this remark, he joins the following curious observation: "I know a family of well-formed children, three of whom have dark hair and eyes, the others light hair and blue eyes. Towards puberty, all the dark-haired children have become epileptics, and gradually lost their sight: the eyes, except in the expansion and immobility of the pupils, retaining every appearance of health."*

Another work, comprising some original pathological reflections, for details of which the reader is referred to the ensuing volume of the Medical and Physical Journal, is a thesis on phthisis or tabes originating from disorder of the abdominal viscera, defended at the university of Bonn, under the Presidency of Prof. HARLESS,† and which is evidently the result of his especial instructions, as it contains a development of some views which this physician had previously given a glimpse of on several occasions.

An account was lately read to the "Société de Médecine" of Paris,‡ by Dr. LEGOUPII, of Valognes, of the expulsion, by the anus, of the inferior extremity of the ileum, the whole of the cæcum, and the commencement of the colon, (in the whole a portion of intestine between fourteen and fifteen inches in length,) in a boy four years and a half old, subsequently to the ordinary symptoms of enteritis. The invaginated portion of intestine presented at the anus, at the end of four days from the attack of the disease, but was not completely expelled until thirteen days afterwards. The patient entirely recovered his former health, suffering only a sense of tension in the belly. The case seems to be sufficiently well-authenticated, and the portion of intestine was submitted to the examination of the Medical Society, and recognized as such by some of its most distinguished members. Indeed, there were already several well-authenticated cases of a similar kind on record;|| which those men who profess to disbelieve the account do not seem to have been aware of, or they would, probably, have evinced more modesty in the expression of their scepticism. The thing is not a miracle; nor does it require, in order that it may obtain our belief, as Voltaire says miracles do, that it should have occurred in a

* Mr. Travers has not mentioned whether or not the epilepsy was obviously connected with hereditary predisposition. As the account stands at present, it does not indicate that dark-haired are more disposed to epilepsy than light-haired children: because the father or the mother of the children may have had epilepsy and dark hair; and the causes of the latter properties may have been transmitted with the predisposition to epilepsy merely as casual contingencies.

† *Phthiseos Ventriculi Pathologia, Dissertatio inauguralis medica, &c. &c. publice defendet* HUGO FRANCISCUS KLEUDGEN.

‡ *Recueil Périodique de la Société de Médecine de Paris*, Octobre 1820.

|| See *Mem. de l'Académie de Chirurgie*, tom iv. p. 215; *Duncan's Medical Commentaries*, vol. ix. p. 278; *Duncan's Annals of Medicine*, vol. vi.; *Journal de Médecine*, tom xxvi. p. 515; *Bulletin de la Faculté de Médecine de Paris*, No. 9, 1818; *Medico-Chirurgical Transactions*, vol. ii.; *Bulletin des Sciences, par la Société Philomatique*, No. 46; and the *Medico-Chirurgical Transactions*, vol. i. for some general remarks on the case by Mr. THOMAS BLIZARD.

public place; in the presence of the Royal Academy of Sciences of Paris, and the Royal Society of London, escorted by a regiment of guards,

In a paper lately read to the Society above mentioned,* Dr. HENRI DE CHÉGOUIN gives an account of a species of fracture of the neck of the femur, that does not appear to have been previously noticed:† it is one in which the neck of the bone is driven into the spongy structure of the great trochanter. He has had one opportunity of ascertaining the fact, by examination of the parts after death. The subject of this case was an old woman, and the accident had happened from a fall on the great trochanter. He thinks he has seen another instance of it, judging from the symptoms, in a patient who recovered from the accident. A man, forty-four years of age, fell from a height on the outer part of the left thigh. He was able to get up, and walk for several paces; but it was necessary to carry him to the hospital on a handbarrow. There was a considerable degree of tumefaction about the base of the great trochanter, without any discolouration of the skin, which still remains. The limb presented but little deformity, and the point of the foot preserved its natural direction. The patient could not submit to any treatment. On measuring the length of the limb a short time afterwards, it was found to be a little shortened. The patient suffered considerable pain about the hip-joint, and the weight of the bed-clothes was distressing to him. When he attempted to raise the limb, he lifted it from the bed, but the pain produced obliged him to let it fall again instantly. He remained in the hospital for two months. At the end of this time, the limb was about half an inch shorter than the other; and pressure on the foot produced pain about the great trochanter.

Such an accident as this will readily explain how it has happened that patients have had some of the signs of fracture of the neck of the femur, without such an accident having been detected in the first instance by eminent surgeons; until, from some slight motion of the limb, (or, at least, some movement of it utterly inadequate to produce fracture of the bone,) it has suddenly become considerably shortened, and the fracture has been manifest.

Dr. RIBES has also lately met with a case of the same kind, which shows very satisfactorily the nature of the affection. It appears, from his preparation, that there is not properly a fracture of the neck of the femur; this part is rather thrust and jambed into the root of the great trochanter, and the neck of the femur forms, in this case, a right angle with the body of the bone, instead of the natural obtuse one. It is easy to imagine the circumstances which might lead us to suspect the existence of such an accident. After a fall on the outer part of the thigh, some degree of tumefaction about the base of the great trochanter, pain in this situation, a little shortening of the limb, whilst the point of the foot has its natural direction, and inability of reducing the limb to its

* *Rocueil Périodique*, &c. No. 284.

† DESAULT and BICHAT seem, however, to have had some conception of such a case.—See *Œuvres Chirurgicales de Desault*, par Bichat, tom. i. p. 222.

natural length; and the want of crepitation on the parts being handled in the ordinary manner.

It has been generally understood, by well-informed surgeons, that the long bones of children will often *bend*, without breaking, even in the healthy state; though but few instances have occurred in which there have been opportunities for examining the state of the parts, soon after such an accident. Mr. WILSON, alluding to this subject,* says, "In a child, in whom I had set a fracture of this kind in the fore-arm, which was bent to nearly a right angle in its middle, I had an opportunity, twelve hours afterwards, of examining the bone; (the child was three years old, and had fallen on the pavement, from a height of eighteen feet, and fractured its skull, which produced its death.) The periosteum round the fracture was torn in part, the osseous matter of the bones had given way, but their animal substance had not separated."

Every science has amongst its pretended votaries men who—like ill-bred mongrels in a pack, that join in the cry with others, they know not wherefore, and glab the louder the more completely they want the scent—vaunt an affected zeal in the pursuit of some object, which has been started by those who lead the chase, whilst they are ignorant of the means of tracing the proper course to it, and know not how to act with it when it is attained. It has of late been the fashion, in regard to medicine, for such men to raise a clamour of admiration for *Pathological Anatomy*; and many of them are so rapt in their ardent devotion to it, that they even neglect, in their writings, to quote LORD BACON in praise of enquiry after *facts*, and in reprobation of hypothesis and theory (which, with them, are often synonymous terms): and hence it is that the medical journals, especially some of the continental ones, have lately so abounded with accounts of necroscopic observations, merely repetitions of what had been already recorded, and which have not been accompanied with any useful inferences or indications, by which their relations to the phenomena of disease during life may be explained. Amongst these, often very remote, *palpable* consequences of certain alterations in the vital properties, and of morbid actions, those which, in the present day, most engage the attention of pathologists, are such as take place in the course of fevers; but modern researches, in this instance, have furnished no other real knowledge than what is conveyed in the following expressions of HOFFMAN: "*Ego certè plenè testari possum fide, quod omnes eos ex febre acutâ obiisse novi, ex inflammatione ventriculi, intestinorum, vel meningum superveniente, decessisse deprehenderim: idque non modo ex symptomatibus dijudicavi, sed et dissectionibus, quas administravi, et de quibus alii fidem mihi fecerunt, compertum habui.*" As this subject has been already noticed under the head of "Pathology,"—when the different views which have

* In his *Lectures on the Structure and Physiology of the Parts composing the Skeleton*, p. 199.

been taken of the relation of the appearances, mentioned in the foregoing paragraph, to the phenomena of fevers during life, were designated,—it may be passed over in this place without any further remarks.

Of the few original necroscopic observations lately made, of an interesting kind, the most important seem to be those of Dr. ROSTAN, respecting "the softening of the brain." After having removed the skull-cap, he says, and divided the dura mater, there is ordinarily seen an effusion of serum, of a gelatinous appearance, generally situate between the pia mater and arachnoid membrane. Sometimes there is no serum present, and the membranes just named are injected with red blood; and very rarely they are covered with purulent matter. The morbid consistence of the brain varies from that of the most liquid porridge to a degree of firmness approaching to that of the healthy brain. The colour of the softened portion is yellowish, greenish, red, chestnut, or dull white, or like that of the lees of red wine, in different instances, or the several hues may be combined. The yellowish-green colour is ordinarily found in cases where the softening has been consequent on an old attack of apoplexy; it is the centre of the softened part which then presents this appearance. The reddish hue is found when the affection is primary, and is present, especially, at the circumference of the morbid part. The softening is sometimes merely at the surface of the brain; at others it penetrates deeply into this organ. When it is superficial, the convolutions are found deformed, and, as it were, puffed up, in a more or less circumscribed part of one or both hemispheres. Every part of the brain has been the seat of this disorganization: the corpora striata, and the thalami nervorum opticorum, are most frequently affected; after them, the central part of the hemispheres (the middle lobes) are most commonly its seat; and the cerebellum and prolongation of the cerebrum are not exempt from it. It has not hitherto been found in the spinal marrow. In the greater proportion of cases, it is confined to one of the hemispheres. It is sometimes accompanied with sanguineous effusion, in many cases immediately enveloping it, in others situate in a remote part. The ventricles often contain an extraordinary quantity of serum; and the cerebral arteries are ordinarily ossified when this lesion exists.

A vexatious exemplification of the conduct alluded to at the commencement of this section is produced in some observations by Dr. COZE, (a physician attached to the French embassy in Russia,) respecting some tumors of the nerves, in which those parts have assumed either a cartilaginous hardness, or a sort of lardaceous structure. This is an affection of which but few instances are recorded;* and yet, with this want of knowledge, Dr. Coze has contented himself with giving an account of the volume and seat of the tumors, the length of time they had existed, and their appearances on dissection, without any informa-

* Dr. COZE, according to a common custom, making his ignorance the standard of information, says there is none. Mr. PRING'S *View of the Relations of the Nervous System* contains some very interesting observations and remarks on this subject.

tion respecting the symptoms which attended their origin and progress. He has not even said whether or not pain was present.

In one of the cases related by this writer, the tumor existed in the sub-orbital nerve, and was removed by an operation, with a favourable result. In another, the trifacial was found degenerated throughout "into a carcinomatous substance." "The sub-orbital portion was of the size of the little finger, within the canal of the same name, and it was still larger on its escape from the bone." The patient was an adult man. The third was an affection of the posterior tibial nerve: "it presented nodosities very like raisin-stones, situate at short distances from each other, and extending very far along the nerve." The diseased part was removed by an operation, by Mr. Dupuytren, with a favourable result, the patient recovering the ordinary powers in the limb.

A memoir, addressed to Dr. Rostan, by Dr. PASCAL,* (physician to the Hotel-Dieu at Brès, Comte Robert,) containing an account of some cases of asthma, with observations made on dissection, which seem to favour the assertion he made a short time since,† that asthma is always the consequence of organic lesion of some of the thoracic organs, has led the latter to state more clearly the sense in which he employed the term organic lesion on the occasion alluded to; he employed it in the most extensive acceptation, as comprising "every visible alteration which is not acute inflammation." In such an acceptation, there are but few who will doubt the correctness of his proposition, and not many who will consider that he has here any pretensions to originality of views.

References were made in former *Proomis* to the cases which have been recorded of rupture of the heart, when the organ has been, apparently, in a healthy state, and when there existed no obvious obstruction to the circulation of the blood.‡ Two cases of this kind have recently been witnessed by Dr. St. VINCENT, (surgeon to the Marine Department at Brès,) in both of which the patients, who were old, but robust, marines, fell down and died suddenly, without having been known to have suffered (for a short time previously,) any moral or physical disorder. In both these instances the heart ruptured in a somewhat transversely vertical direction, from the left ventricle, a little beneath the aorta, towards the right side of the heart. Dr. Vincent considers, with apparent propriety, from the course of the fibres of the heart, that the rupture began from the inner surface of the parietes; in some other instances it seems to have commenced on the external surface.¶ There could not be discovered, in either case, he says, any erosion or loss of substance, any pultraceous disorganization, any *détritus* of the fleshy fibres, nor the smallest change of structure or suppuration of those

* *Nouveau Journal de Médecine*, tom viii.

† See the Historical Sketch published in July 1819.

‡ CHAUSSEER found, in some of his experiments on animals, that the heart dilated and burst suddenly, on the circulation in the aorta being completely stopped.

¶ This seems to have been clearly evident in the case related by Dr. FISCHER, and inserted in the forty-first volume of the *Medical and Physical Journal*.

fibres." The aortic valves were in the healthy state, as were the lungs, and all the other parts of the body. Dr. St. Vincent, however, remarks that the heart, in both cases, was "soft, flaccid, pale, and easily torn, as it is in all subjects of the same age," though it was neither thinner nor more distended than ordinarily.

Some observations were made in the *Proëmium* to the forty-third volume, on inflammation of the inner tunic of the arteries, from internal causes; and it was there hinted, that this affection was sometimes related, as an effect, with rheumatism in various parts, especially when the heart has become diseased subsequently to an attack of rheumatism. The history of a case was lately communicated to the "*Société de Médecine-pratique*," at Paris, by Dr. BARDE, that somewhat favours this notion. A man, twenty-seven years of age, previously in good health, was affected with acute rheumatism in the lower extremities, which disappeared after the application of leeches and emollient cataplasms. A few months afterwards, he began to experience pain in the epigastric region, and between the shoulders, with a frequent and dry cough, which were soon succeeded by palpitation of the heart. These symptoms were somewhat alleviated by the remedies employed; but returned, with increased violence, after various intervals; and at the end of two years the patient died, after suffering the most severe derangements of the circulation. The interesting circumstances discovered on dissection, in the view in which the case is here regarded, were the following:—The heart was larger than ordinary, the whole of its cavities were dilated, the membrane lining them was of a brownish colour, covered with brownish mucus, and spotted with deep red patches. The inner membrane, on further examination, was found to be destroyed where these patches were present, and in all the rest of its extent it was much thickened, and adhered so strongly to the muscular structure, that it was not possible to detach it. The ascending aorta, the carotids, and the branches they supply to the face and the brain, the subclavian arteries, the brachial and its divisions, as far as the lateral arteries of the fingers, all presented a thickening of their internal membrane, which was also hard, of a deep red colour, and covered with a whitish purulent matter; between the internal and the fibrous membranes there was a layer of very consistent serous fluid, of a citron colour. The pulmonary arteries and veins presented similar alterations of structure. The abdominal aorta, and the divisions it sends to the viscera, were also equally inflamed, and the thickening, with redness of their internal membrane, was very strongly manifest, as far as the hypogastric and crural arteries: the inflammatory appearances then diminished, as the arteries proceeded along the lower extremities. The trunks of the two venæ cavæ, from the subclavian superiorly, and inferiorly from below the diaphragm, participated also, to a very high degree, in the inflammation: their internal membrane and its valves were red, thickened, and were torn by the slightest distensive force. The mucous membrane of the œsophagus, stomach, and small intestines, showed signs of inflammation, as well as the omentum. "The liver was very voluminous; the spleen, on the contrary, very small."

The rarity, and, perhaps, the singularity, of the case of perfect re-union of the neck of the thigh-bone, after fracture, which was observed by Mr. LISTON,* is rendered still more striking by the remarks of Mr. WILSON on this subject:† his opportunities for observation must have been very extensive, during the, perhaps, nearly forty years that he has cultivated anatomy at a public school; and yet he says, "In all the cases which have come under my observation, in examining the dead bodies of those to whom this fracture had happened, there have been no appearances of bony union within the capsular ligament; the union has been formed by a ligamentous substance; and, in almost every instance, the neck of the bone has been either entirely, or in a very great degree, absorbed. Bands of a ligamentous nature have been found, passing from the head of the bone which remains in the socket, to the edge of the acetabulum, and to various parts of the internal surface of the capsular and synovial ligament: the membranes have been very much thickened, and much adventitious adhesive substance, now become vascular, has been found filling up a large proportion of the cavity of the joint. A fluid has been found occupying the remaining part, in which flakes of coagulable lymph, having become solid, were floating, sometimes loose, and sometimes adhering to parts of the inside of the ligament. The cancelli of the broken surfaces have been occasionally found filled up with a deposition of bony matter, and covered with crusts of a substance apparently between the nature of cartilage and ligament, from which processes extend, connecting the bones to each other, and to the surrounding ligaments; and adhesive ossific inflammation is often found to have taken place on the outside of the capsular ligament of the joint. In quadrupeds, where the bone has been broken intentionally, the union has been found of a ligamentous nature."‡

Although devoid of inferences usefully applicable to pathology, it may be proper to adduce the following observations, from the source last quoted: "I have seen (says Mr. Wilson,) two varieties in the number of vertebræ: one was in a lascar, who had twenty-five; the additional vertebra was in the back; there were on each side thirteen ribs. The other was in an European woman, who had only twenty-three vertebræ: she had eleven ribs on each side. As these varieties were met with in recent bodies, brought for dissection, there could be no deception used."—MORGAGNI (in one of his chapters on the Diseases of the Head,) mentions having witnessed thirteen ribs on each side, corresponding to as many dorsal vertebræ, in a woman, where there were five lumbar vertebræ.

Similar in its general character to the foregoing account, but yet somewhat more interesting to the physiologist, is that of a general transposition of the thoracic and abdominal viscera; so that the parts which ordinarily occupy the right side were situate in the left, and *vice versa*;

* Noticed in the last *Proœmium*, p. lxxi.

† *Lectures*, &c. delivered before the Royal College of Surgeons.

‡ These observations accord, very exactly, with those of Dr. COLLIER, in his paper on this subject, in the second volume of the *Dublin Hospital Reports*,

and which has been related by Drs Naogulano and Ploazny? This is the most complete example of such a transposition on record. The subject of it had enjoyed a tolerable, though somewhat languid, state of health—having experienced no remarkable disease, and being of a very intelligent character—until her seventh year, when she became affected with what seems to have been chronic bronchitis, which continued for six months, when, after exposure to inclement weather, she had an attack of croup, and died on the second day of the disease. The large extremity of the stomach was situated on the right side, the pylorus on the left. The convolutions of the duodenum were on the left side, in an inverse direction to that which they ordinarily have on the right. The cœcum was on the left side; the ascending arch of the colon also on the left, whilst its descending arch and sigmoid flexure were on the right. The liver and gall bladder were on the left side, in the same relative direction with that which they ordinarily have on the right, whilst the spleen had a similar position on the right. The duplicatures of the peritoneum were transposed in a manner analogous with that of the viscera to which they were distributed. The lungs (presenting signs of bronchitis and of very severe laryngitis) were also transposed. That with two lobes was on the right, and that with three on the left. The apex of the heart presented its direction, downwards, from forwards and to the right side; its base, backwards and to the left. The arch of the aorta, as well as the general course of the thoracic and abdominal portions of this vessel, assumed a direction inverse to that which they have in ordinary subjects. The cranium and spinal canal were not opened.

An instance of duplication of several muscles, in the same subject, has lately been observed, in a very athletic young man, by Dr. TIERMANN,† professor of Anatomy at the University of Heidelberg. Beneath the great pectoral muscle, on each side, there was a second, separated from the former by a thick layer of cellular substance. The second muscle arose from the external surface of the cartilage, and anterior extremity of the second, third, fourth, and fifth, ribs; its fibres took the same direction as those of the superior muscle, formed a single belly, and were inserted at the internal surface of the tendon of the great pectoral, a little before its ordinary termination in the great tubercle of the humerus. The second muscle had no clavicular portion. There was also a second pectoralis minor, which arose, by two heads, from the second and third ribs, and was inserted, like the ordinary one, in the coracoid process of the scapula. The gluteus maximus, on each side, was also double.

Various cases are recorded of the formation of calculi from the lachrymal fluid;‡ but one, which has lately been published by Professor

* *Journal Universel des Sciences Med.* October 1820.

† *Deutsches Archiv für die Physiologie*, 1820.

‡ By HALLER, MORGAGNI, BLEIGNY, SCHMUCKER, and SANBIFORT.

WALTHER, of Bonn,* is particularly remarkable, from the rapidity with which the concretions were produced. The patient was a young woman, otherwise in apparent health. The formation, between the eye-lids (in both eyes), originated without any evident cause, and was followed by severe inflammation of the eyes, coming on with shiverings followed by general heat. The calculi continued to form for ten weeks, in a first attack of the complaint; and for several days in a second, which occurred after an interval of some years. The concretions were formed of carbonate of lime, (which constituted the greater proportion of them,) a small proportion of phosphate of lime, and albumen.

In order to avoid an undue extension of the limits of this sketch, it is necessary to confine our observations respecting SEMIOLOGY to notices of especial dissertations on this subject, and leave readers to draw their own inferences of this kind from the facts which are stated in the sections on Physiology and Pathology. The only late production of the sort above-designated, presenting considerations of an original character, relates to a matter which may at first appear trivial, but which will be found of some importance in practice.†

It is not an unfrequent occurrence for a medical man to be consulted respecting an infant, a few days old, which is suspected by its nurse to be tongue-tied, because it is unable to suck, though it uses violent efforts, and, as the women say, "falls into a passion because it cannot do it;" when, upon enquiry, it will be found that it had sucked very well for a few days after its birth, and where no malformation about the mouth exists. The patient takes the nipple into its mouth; but has hardly made three or four suction, when his respiration appears to be obstructed; his face becomes of a violet colour; he precipitately abandons the breast, utters some cries, or is seized with a severe fit of coughing, from which he falls into a state of stupor. These accidents disappear in a short time, but are reproduced whenever the child attempts to suck. If the patient be attentively examined, some degree of tumefaction about the nostrils and the eye-lids, with a shining appearance of the skin of the former parts, will be observed; the mouth will be found kept constantly a little open, and the lips and tongue will be found dry. There will, probably, be also some degree of nasal sniffling or wheezing evident. These phenomena indicate the nature of the complaint, which itself satisfactorily accounts for the accidents which ensue from the attempt to suck. The patient has *coryza*, or inflammation and thickening of the mucous membrane of the nasal cavities, which so far obstructs the passage through these, that the child cannot breathe with its mouth closed; and, consequently, is in

* *Journal der Chirurgie und Augenheilkunde*, herausgegeben von C. Graëf, ordentlichen öffentl. Prof. der Medizin und Chirurgie zu Berlin, u. w. s. and Ph. v. WALTHER, ordentl. öffentl. Prof. der Medizin und Chirurgie zu Bonn, u. w. s. 1 Band, 1 Heft.

† *Observations sur la Coryza des Enfants à la Mamelle*; par P. RAYER, D.M.P. 8vo. Paris 1820.

danger of suffocation when it performs the actions necessary for *steking*. After these symptoms have continued for a few days, generally three or four, a considerable discharge of mucus from the nostrils takes place, which will subside in a few days, if the patient be kept in a warm room and preserved from exposure to currents of cool air: provided it is, during this time, properly supplied with nutriment administered by means of a spoon.

Obvious as the foregoing explanation may appear to some persons, it has, nevertheless, been novel to many practitioners neither deficient in ordinary intelligence nor inexperienced in the observation of the diseases of infants: and it is very probable that many children who have died, on being thus affected, might have been preserved had the nature of their disorder been understood.

The writer of this sketch has so much aversion for *Nosology*,—in the sense in which this term has generally been employed,—that it is probable he would not do justice to such dissertations of this kind as have lately been produced, were he to enter into any particular considerations on them. He is willing to allow that there was a period in the progress of medicine when nosology proved beneficial, by leading pathologists to trace the mutual relations and distinguish the phenomena of diseases with more care and accuracy than they would have done without the views it exposes: but, all the good that can be derived from it seems to have been attained; and by perpetuating its influence, we are only preserving the errors which have resulted from it. There is not one of the systems which have been promulgated that, if employed as a guide in practice, must not lead to serious evils, without being productive of any important advantages;—by inducing the practitioner to treat the *symptoms* of disease merely, in almost every instance; and, in a word, by tending to establish a purely empirical mode of proceeding which differs from that of the common mountebank, only in being directed by somewhat nicer distinctions, and more particular analogies, than those on which his pretensions are founded. It is, besides, impossible to refer a great proportion of the diseases of common occurrence to any of the distinctions assumed by the nosologists. As examples; a child has, amongst other less important symptoms, a yellow furred tongue, its bowels are disordered in their actions; its feces are dark-coloured and otherwise unhealthy; it has head-ache, a contracted pupil, intolerance of light, and fever. This derangement will, probably, produce, at length, hydrocephalus, amongst other affections; but what *name*, of all those in the nomenclature of nosologists, can be applied to it in its present state? Another has scrofulous tumours, enlarged and otherwise diseased mesenteric glands, a swelled and hard belly, general emaciation, its fluids are in a morbid state, and it has a remittent fever: here are *six distinct diseases* of the nosologists, five of which are merely ordinary consequences of the first of them, on which they essentially depend, and cannot be removed unless this be first obviated: though the nosologists have their proper mode of cure for each of them; and it is not very rare for practitioners, who

are guided by their systems, to treat, especially, some one of those consequences on grounds not very different from that presented by the hazard of the die. Other, and more evil, consequences have been pointed out by several modern writers, and a disposition to regard diseases in the manner in which PINEL has done in his *Nosography* seems to be getting very prevalent; though the views here alluded to are shown to be far too superficial, if medicine is to be really raised to the rank of a science, by the disquisitions on this subject in the "General Indications which relate to the Laws of the Organic Life."*

The only nosological dissertations published in the period embraced by this history, are those of Dr. GRANVILLE, respecting the diseases of children,† and Dr. MERRIMAN,‡ relating to the varieties of parturition, which, for the reason about stated, are here merely referred to in a general manner.

The most important considerations relative to HYGIENE which have been lately promulgated, are those comprised in the Report of a Committee of the House of Commons relative to the doctrine of the contagion of the plague,§ the Memoir of Mr. WEBB,¶ and the work of Sir ARTHUR BROOKE FAULKNER on this subject;|| Dr. CLARKE's Treatise on the influence of Climate on Consumption; in the work of Dr. HARTY on the Fever lately epidemical in Ireland;** the several treatises on the yellow fever, noticed in the section on pathology,††

* Dr. NICHOLL, it may be right to state here, has adduced some very forcible objectionable remarks on nosological classifications in his *Elements of Pathology*.

† *London Medical and Physical Journal*. Dec. 1820.

‡ *A Synopsis of the various kinds of difficult Parturition, &c.* 8vo. pp. 323. Callow, London, 1820.

|| See the *Collectanea* in the last and ensuing volumes of the *London Medical and Physical Journal*.

§ *Medical Transactions*, published by the College of Physicians of London, vol. vi.

¶ A particular account of this work will be given in an early number of the *London Medical and Physical Journal*, as well as of that of Dr. CLARKE.

** *An Historical Sketch of the Causes, Progress, Extent, and Mortality, of the Contagious Fever epidemic in Ireland, during the years 1817, 1818, and 1819, &c.* 8vo. pp. 512. Hodges and M'Arthur, Dublin; and Callow, London, 1820. See *London Medical and Physical Journal*, for Nov. 1820.

†† That part of this *Proœmium* which comprises the discussion on the yellow fever had passed the press before the writer had an opportunity of seeing the work of Dr. PARISSET on this subject, which was not published in Paris until the month of December.*

Dr. PARISSET left France at the beginning of November 1819, for the purpose of making observations on the yellow fever of Spain, where it had been prevalent in the year just mentioned. He was sent by the Duke DE CAZES. Though he did not arrive there until the disease had disappeared, excepting in a few cases, which Dr. Pariset saw in the Military Hospital at Cadiz, he has, nevertheless, chosen to write a large book on the subject, from the information he obtained from the Spanish physicians and from literary records. Although opinions founded on such a basis may not be considered worthy of much attention, especially when they are in opposition to those of nearly the whole of such well-informed patho-

* *Observations sur le Fièvre jaune faites à Cadix en 1819.* Par M. M. Pariset et Mazet, (Docteurs en Médecine de la Faculté de Paris;) et rédigées par M. Pariset. 4to. pp. 144. Paris, 1820.

and in the Memoir (read to the Medico-Chirurgical Society of London) of Mr. SMITH, of Bristol, on the relative frequency of stone in the bladder in the different counties of Great Britain and Ireland.

logists as have cultivated opportunities for extensive observation of the disease; it may be prudent to state here those which he has formed respecting the most interesting circumstances in its history. Dr. Pariset says "he does not know that contagion has ever been more solidly proved than the contagion of this fever (the yellow) in Andalusia." This opinion is formed in an exclusive manner: he believes that the disease always has a foreign origin, in respect to Spain: this is an inference, he says, "founded on the strongest presumptions; and I venture to adduce it, if not as a demonstrated truth, at least as a very great probability." He thinks it comes most frequently from America, whilst that of 1819 came "to all appearance from the East Indies." His arguments for these opinions are as devoid of solidity and force as it is, almost, possible for arguments on such a subject to be; they are not much better than the absurd and ridiculous stories which he cites as such from the information of the Spanish physicians. Each account of its origin seems to satisfy the generality of them, until it is accidentally disproved, for they take but little pains to ascertain its validity. Thus, it was for some time confidently believed that it arose in the part of the city of Seville called Santa Cruz (in 1819) from infection in some bodies (in which the causes of it must have been dormant) which were disinterred in an adjacent burial-ground, for the purpose of being carried to Madrid. It was, however, at length, found that these bodies were disturbed in the month of April, whilst the disease did not make its appearance before September. It was necessary, then, to seek for another origin, which was readily found, and which satisfied the enquirers for a while, until it was found to be untenable, when it was changed for another; so that a dozen different ones were assumed, all of which are equally unsatisfactory. Some vessel or other which had arrived in the port a short time previously to the prevalence of the disease, became, at last, an almost never failing resort. MONTAIGNE says it is requisite that we should observe very acutely to see that which is always before our eyes: and hence it is, perhaps, that the source of the disease in Seville has, probably, been overlooked. The houses, especially in the quarter called Santa Cruz, are very low, and the streets so narrow that in no part hardly can two men walk abreast; and in many places we can touch with our elbows the houses on each side as we walk along them. As much care, too, seems to have been taken to make the streets run in a crooked direction, as in most other cities is used to have them straight. The stench that rises from these gutters (the most proper name for them) in a hot summer's day, especially soon after rain, exceeds every thing excepting that from some of the Dutch cow-houses: and the emanation of an abundance of hydrogen gas from them, at certain times, is so manifest, that a chemist, who was aware of the circumstance, would think it hazardous to walk through them with a light in his hand. The yellow skin and black vomit have been, it appears, commonly present, or doubts as to its being the yellow fever (if there be a specific disease of this kind) might be reasonably entertained. It, however, seems that disputes often existed amongst the Spanish physicians respecting its identity with, or diversity from, "bilious fever," especially in the early periods of its prevalence in different seasons, and that its existence was often not acknowledged until the mortality from it had arrived at a certain extent.

Dr. Pariset fears that, unless some quarantine restrictions are imposed, the yellow fever may extend, by land, into France. It is somewhat remarkable that he should make his journey at a time when he could not have expected to have opportunities for observing the disease to any considerable extent, much less to trace its origin, had he been as well informed respecting its history as a physician of his character should be: for it is sufficiently well known that it is hardly ever prevalent as late as the month of December. The late physician to the Bicêtre is a man of acute observation, vivid perception, considerable talents for a diversity of objects, and somewhat of a politician: it is, therefore, probable, that, whilst the public have this book on the yellow fever presented to

This last production is an extension of the views of the same subject given by Mr. A. COPLAND HUTCHISON in a paper published in a former number of the *Medico-Chirurgical Transactions*, and is the result of very extensive and minute enquiries; but, before the full value of it can be ascertained, and the useful inferences drawn that it, probably, will admit of, it is necessary that a similar statistical account of the diet and regimen of the people of the several counties, and a very particular topography of each of these, should be formed, as well as a comparison in this respect between England and other countries. A general summary of the results of Mr. Smith's enquiries respecting the number of cases for which the operation of lithotomy has been performed, within certain periods, is all that can be given in this essay.

"*Bristol*.—At the Infirmary during the last 82 years there have been cut about 361 persons; but, if we make divisions of this period, we shall see that the numbers have regularly diminished in a very remarkable degree, so that at present we have from 3 to 4 in two years, and formerly there have been more than 6 in one year.

	Years.	Cases.	That is, about
From 1735 to 1770	35	222	6 $\frac{1}{2}$ yearly.
1770 to 1800	30	108	3 $\frac{1}{2}$
1800 to 1817	17	31	less than 2

Within the last 20 years, not more than 10 private operations have been performed either in the city itself or within a circle of 8 or 10 miles around its boundaries, although we are surrounded on all sides by exceedingly populous villages, and lie on the borders of two large counties; add to this the easy and constant communication with Wales, which gives us almost an exclusive preference for the whole southern parts of the principality.

"About a year ago, a patient was cut at Monmouth, which I believe to be the only operation ever performed on the other side of the Severn.

"*Bath*.—18 in 20 years; not one annually for all the rest of Somersetshire known to have been cut; there were 12 at Exeter, and 6 at Bristol in 10 years, making less than one annually. All the county, therefore, Bath included, gives about 1 and $\frac{1}{2}$ every year.

"*Gloucestershire*.—for the last 61 years' average 1 and $\frac{1}{2}$ yearly.

"*Worcestershire*.—from 1767 to 1779 averaged 1 and $\frac{1}{3}$ in two years. During the last 25 years the cases averaged a fraction more, than 2 annually; a considerable increase.

"*Wiltshire*.—sent to Bristol from 1750 to 1770, 18 cases; and from 1770 to 1790, 8 cases. Probably several were cut at Salisbury.

them,* the Duke DECAZES has other interesting information in the results of his enquiries.

* We should not neglect to notice that it contains several *pictures* (copies of some made by a Spanish physician): four of the bust of a handsome young Spaniard; in the first of which he is represented as he was in health; in the second his whole face is of an unvaried brick-red colour; in the third it is of a dun hue; in the last he is shown in "the horrible conflict with death;" the sheets and pillows about him covered with blood and black vomit, which are still running from his mouth. There are, besides, paintings of two series of tongues, that are very well calculated to make the French ladies and gentlemen wonder at what a thing a person's tongue may become changed into; and dread to think that such might be the fate of theirs should the *yellow fever* find its way into France.

also in the above 30 years. In the last 14 years the county sent to Bristol 5 cases, and 7 were cut at Salisbury, but of these 3 came from elsewhere. The total, however, will not make one yearly.

"*Oxfordshire* ;—12 cut and 8 uncut ; call them 20 in 10 years, will be two annually.

"*Devonshire and Cornwall* ;—from the year 1745 to 1775, 30 years, 65 patients, that is, $2\frac{1}{2}$ annually. From end of 1802 to end of 1805, 3 years, 7, that is 2 and $\frac{1}{3}$. From 1805 to 1816, 11 years, 18, that is, 1 and $\frac{2}{3}$.

"*Hampshire* ;—from 1808 to 1817, 9 years, 20 cases, that is, $2\frac{2}{3}$ annually.

"*Warwickshire* ;—during the last 38 years, less than 1 annually at the Infirmary. Perhaps after the same rate in private practice.

"*Cambridgeshire* ;—from 1766 to 1799, 4 only, that is, rather more than 1 in 4 years ; during the last 40 about 4 annually, a great increase. Probably this Infirmary and the Northampton include the cases from Huntingdonshire.

"*Northamptonshire* ;—from 1 yearly to 3 in 2 years ; occasionally 2 in the year.

"*Norfolk* ;—during the last 40 years, $11\frac{1}{2}$ yearly.

"*Kent* ;—during the last 25 years, about 1 and $\frac{1}{2}$ yearly.

"*Salop* ;—from 1747 to 1779, 13 years, 8 were cut : of late about 4 in 3 years, that is, 1 and $\frac{1}{3}$ annually, a considerable increase.

"*Staffordshire* ;—for some years before or after the year 1792, there were 5 or 6 *per annum* ; about 1800 it fell to 3 or 4 ; and of late amounts to 1 yearly. The disease here lessens very considerably.

"*Leicestershire* ;—from 1771 to 1779, 8 years, 3 cut, that is, $\frac{3}{8}$ in a year ; during the last 12 years, there have been 3 annually ; a very great increase.

"*Cheshire* ;—from 1755 to 1779, one only at the Infirmary. Private cases 6. From 1787 to 1817, in all 12 ; making in 30 years, 12 cases, equal only to $\frac{2}{5}$ in a year. Even the greater part of these, came from North Wales, the cases from which Chester appears to have exclusively.

"*Derbyshire* ;—6 in 7 years, that is, not one annually.

"*Nottinghamshire* ;—in 36 years, 64 public, and 40 private, 104, nearly 3 annually ; the proportion of private exceeds apparently any other county.

"*Lancashire* ;—Liverpool. From 1749 to 1779, 30 years, there were 6 cases. For the last 10 years there were about 9 ; being rather less than 1 a year.

"*Manchester* ;—from 1752 to 1779, 26 years, 62 cut, that is 2 and $\frac{2}{3}$ yearly. From 1809 to 1818, 10 years, 29 cut, nearly 3 *per annum*. All Lancashire therefore was formerly rather more than 2 and now is nearly 4.

"*Lincolnshire* ;—one annually.

"*York city* ; from 1740 to 1779, 50 cases, being 1 and $\frac{1}{2}$ annually, or 5 in 4 years. There are now about 15 cases in 20 years, that is, not one a year.

"*Sheffield* :—from 1774 to 1798, 40 cut, rather less than 2 a year. From 1798 to 1817, 17 were cut, less than one annually.

"*Leeds* :—from 1767 to 1817, 197 cut, and 65 uncut; total in 50 years, 262; 24 annually. Any division of this time contains nearly the same average of patients.

"*Halifax* :—during the last 7 years, 24 cases; more than 3 annually.

"*Hull* :—from 1801 to 1817, 10 cases in 16 years.

"In all *Yorkshire*, there are cut about 8 or 9 annually, according to the reports sent to me.

"*Northumberland, Durham*, and probably a great part of *Cumberland* included;—from 1751 to 1779, 28 years, 93 cases; being rather less than 34 annually. From the year 1798 to 1817, being 19 years, there were 29 cases, which is nearly 14 yearly.*

The total number of cases annually operated on in England and Wales, in the counties which have come under the cognizance of Mr. Smith, is, then, about 60. He says, he does not think that in Scotland (the population of which amounts to about 1,600,000) the yearly amount is more than 12, and he considers the same number may be reckoned for Ireland; the population of which is between four and five millions.

General references only can be given here to several original views and interesting reflections relating to THERAPEUTICS, that were particularly developed in the last volume of the *Medical and Physical Journal*, or that would require details incompatible with the limits of this Sketch to expose them in a proper manner. Amongst the former class are two papers by Dr. KINGLAKE;† the treatise of Dr. GRANVILLE on the use of the Prussic Acid;‡ the observations of Dr. COINDET on the efficacy of Iodine as a remedy for bronchocele;§ additional illustrations of the efficacy of compression and percussion in the cure of rheumatism and sprains, by Dr. BALFOUR;|| some observations by Dr. EVANSON, of Cork, on the treatment of undue determination of blood to the head;¶ the cases of Mr. SAMUEL YOUNG, exemplifying the efficacy of pressure as a remedy for cancerous and some other diseases, attended with ulceration; ** the remarks of Dr.

* Mr. Smith remarks, respecting the above statements generally, that "it is to be presumed that several operations were performed not included in the records. † See *London Medical and Physical Journal*, vol. xlv. pp. 27 and 99.

‡ *An Historical and Practical Treatise on the Internal Use of Hydro-Cyanic (Prussic) Acid, in Pulmonary Consumption and other Diseases of the Chest, as well as in several Complaints attended by great Nervous Irritation or acute Pain: with full Directions for the Preparation and Administration of that Medicine; and a preliminary descriptive Account of the principal Diseases in which it has been employed, illustrated by numerous Cases.* By A. B. GRANVILLE, M.D. F.R.S. F.L.S. M.R.I. &c. 12mo. pp. 417. Second Edition, greatly enlarged. Longman and Co. 1820.

§ See *London Medical and Physical Journal*, vol. xlv. p. 485.

|| *Illustrations of the Efficacy of Compression and Percussion in the Cure of Rheumatism, Sprains, and Debility of the Extremities.* By WILLIAM BALFOUR, M.D. 8vo. pp. 36. Edinburgh, and London, 1820.

¶ See *London Medical and Physical Journal*, vol. xlv. p. 379.

** *Op. cit.* vol. xlv. p. 265, 353.

PRICHARD on the treatment of typhous fever;* the case of old, and for some time, irreducible hernia, removed by confinement to the horizontal posture and the use of pressure, by Mr. BAMPFIELD;† the observations of Mr. ALCOCK on laceration of the perineum, occurring during parturition;‡ the treatises of Professor RUDOLPHI and Dr. BREMSER on intestinal worms;§ and the memoirs of Baron LARREY on thoracic wounds|| The efforts made to keep pace, in the regular series of the Medical and Physical Journal, with every advancement in this department of medicine, have left but few subjects of the latter class to be noticed on this occasion. There are, however, some which have hitherto not been particularly noticed in them; as, the works of Mr. HADEN on the use of Colchicum,¶ and of Dr. BALFOUR on that of tartarized antimony;** the memoirs of Dr. LATHAM, on the employment of venesection in cases of sudden seizures, commonly called fits,†† in which the indiscriminate use of that practice is reprobated; the observations on the Duodenum, by Dr. YEATS;‡‡ the cure of preternatural joints by means of a seton in two cases, by Dr. MOTT, of New-York;§§ a case of successful extirpation of a carcinomatous eye, in a young man, by Mr. BARLETTI,||| (surgeon to the hospital at Vigevano;) a successful instance of the performance of the operation for empyema, by Mr. ALLONEAU,¶¶ by which about thirteen or fourteen pints of purulent fluid were evacuated; the performance of the high operation for the stone, by Mr. SOUBERBIELLE,** in a female child three years and nine months old, with a favourable result;††† and some forcible illustrations of the efficacy of the practice of burning moxa on the body, in various diseases,‡‡‡ on the same principles as caustics, rubefacients, and vesicatories, are ordinarily employed. Some other views, which partly come within the dominion of therapeutics, but which more especially relate to Chirurgery, will be presently disclosed, after some particular observations on the treatment of the yellow-fever of America, and on the treatment of cases of loose pieces of cartilage in joints. The observations on the former subject relate

* See *London Medical and Physical Journal*, vol. xlv. p. 137.

† *Op. cit.* vol. xlv. p. 295.

‡ *Op. cit.* vol. xlv. p. 193.

§ *Op. cit.* vol. xlv. pp. 342, 427.

|| *Op. cit.* vol. xlv. pp. 238, 288, 484.

¶ *Practical Observations on the Colchicum Autumnale, as a general Remedy of great Power, in the Treatment of Inflammatory Diseases, both acute and chronic; and therefore as a Substitute for Bleeding, in Disorders which are connected with increased Action of the Heart and Arteries.* 8vo. pp. 84. Burgess and Hill, London, 1820.

** *Illustrations of the Power of Emetic Tartar, in the Cure of Fever, Inflammation, and Asthma; and in preventing Consumption and Apoplexy.* Second Edition.

†† *Medical Transactions*, vol. vi.

‡‡ *Op. cit.* vol. vi.

§§ *New-York Medical and Surgical Register.* Part II. Vol. I. See *London Medical and Physical Journal*, No. 264.

||| *Annali Universali di Medicina*, fas. xxxvii.

¶¶ *Bulletin de l'Athénée de Médecine de Paris*, Sept. 1820.

** *Annales du Cercle Médical*, tome i. partie i.

††† It is remarked that the subject began to suffer the symptoms of stone "from her birth." The calculus was of the size of an almond within its shell.

‡‡‡ *Revue Médicale*, No. iv.

especially to the use of *mercury*; and they are adduced here in order to obviate, in some degree, the bad consequences of a fanatical disposition to the use of mercury, in all forms of fever, that seems to be much too prevalent. After stating that the yellow-fever had presented itself with different degrees of violence, (in one of which it was so mild that, in a great majority of cases, it terminated favourably on the third day,) the Report of the Medical Society of Savannah states that this mild form is, however, capable of being exasperated into a serious disease by improper treatment, particularly by the administration of mercury, which brings on all the affections which characterize the higher grades of the fever. Dr. WATTS also himself recommends, "as particularly deserving of attention, the strong condemnation of the mercurial plan of treatment, so earnestly set forth by Dr. Irvine."

There is not, perhaps, an operation in CHIRURGERY, on the propriety of which different surgeons are more divided, than on that for the removal of loose cartilages from the cavities of joints in individual cases. That the operation is not without danger of serious ill consequences, is sufficiently well shown; but the fear of these has probably been much increased by inferences drawn from the results of exposure of the same cavities in other cases of disease, when the parts are in a state of irritation, or are particularly disposed to inflammation. The practitioners on the continent resort to it much more frequently than English surgeons. This point of practice was taken as the subject of a clinical lecture by Prof. SCHREGER, (a practitioner of much ability and experience,) of Erlangen, a few years since, where he reprobated the unjust prejudices entertained respecting the operation in this country; and related three cases in which he resorted to it himself, with favourable results,—once in the shoulder, and twice in the knee-joint.

Mr. WILSON, on noticing this subject in his Lectures before the College last year, observed, that, "if the substance can be secured in a convenient part of the joint by bandages and compresses, this should be preferred to the risk which must be incurred by opening the capsular ligament. They often, however, prove so troublesome, inconvenient, and even dangerous, by insinuating themselves between the articular cartilages, that we are justified, when we find them lodged in a proper part for the operation, to remove them. I have known several instances of the operation succeeding, and none where such injurious consequences as some surgeons apprehend have followed."

Of new or particularly interesting operations in Surgery, and improvements on those previously in use, there are to be noticed one by M. DUPUYTREN, on an un-united fracture of the lower jaw;* two cases of lithotomy, published in the eleventh volume of the Medico-Chirurgical Transactions; and an account of a new instrument to be used in that operation, by Mr. HENRY EARLE.

The two cases of lithotomy above referred to, (the one by Mr.

* See *London Medical and Physical Journal*, vol. xliv. p. 197.

MAYO, of Winchester, and the other by Mr. DICKINSON, of Macclesfield,) merit notice, in consequence of their comparatively successful results, though the stones measured, in one instance, eight inches and a half in its smallest circumference, and rather more than ten inches in its largest; and in the other it weighed about eleven ounces, (its dimensions were not ascertained.) Both these were removed after having been broken in the bladder. The generally unfavourable results of the cases, in which this operation of breaking the stone has been resorted to, in consequence of the injury usually done to the bladder by the instruments hitherto employed having been but ill appropriate for the purpose, led Mr. Henry Earle to endeavour to improve their construction; and he has recently presented to the Medical and Chirurgical Society an instrument which seems to be at least much better than those which have been hitherto employed.

"The instrument consists in three blades, united by two pivots at their upper extremities, and attached to three cylinders below, which move one within the other. The outer blade and cylinder are fixed below to a hollow steel handle; the two inner cylinders and blades are made to revolve to the extent of one-third of a circle, one to the right and the other to the left, by means of two rings with uprights, which move round the lower extremity of the outer cylinder, and are fastened with a pin when the blades are expanded. The cylinders are four inches in length. The outer blade is broader than the other two, to admit of their closing under it. The length of it is likewise greater, and this projection nearly answers the purpose of a fourth blade. The blades are connected with the cylinders by means of a nut and two screws, which can easily be removed to allow of other sets of blades of different sizes and forms, adapted to calculi of various dimensions, to be readily attached. The other parts of the instrument consist in a wedge-shaped perforator and screw, made to pass up through the hollow handle and inner cylinder. The perforator does not turn round, being kept steady by two pins which work in its groove. The screw turns upon the lower extremity of the perforator, to which it is connected by a socket which works round a shoulder. The lower extremity of the steel handle is tapped for the male screw to work in. The steel handle is octangular, and a small movable key, similar to that used for unscrewing pocket pistols, is adapted to it, to enable the operator to have a more steady hold of the instrument. When it is to be used, the screw, perforator, and key, should be withdrawn; and the operator, having previously endeavoured to ascertain the size and form of the stone, should attach that set of blades which appears to him best adapted to the case. The inner blades should then be closed together under the broader outer one; the whole will thus occupy very little more space than one blade. The form of the whole instrument, in this position, bears some analogy to a catheter, and it should be passed in the same direction, upon the fore-finger of the left hand, under the calculus. The inner blades should then be expanded by moving the rings and uprights. When the blades are equidistant, the two uprights will exactly oppose each other, and the steel pin should be passed through the openings left in them, which

will prevent the blades from moving. The operator should now carefully examine, with his fore-finger, to ascertain whether the calculus be properly secured within the blades; the small space occupied in the tract of the wound readily admitting of this examination. The next stage in the operation consists in fitting on the key, and introducing the perforator and screw, which is to be gradually turned round until it meets and crushes the calculus. The screw and perforator should then be withdrawn, the uprights unpinned; the blades closed, and the instrument removed from the bladder, with the same precaution and in the same direction that it was introduced. The fragments of the stone are then to be removed by common forceps; and, should any portion of it be still too large for extraction, the instrument with smaller blades must be again introduced to effect a further division of it."

The Obstetrical Art, as well as Chirurgery, has arrived so nearly to as great a state of perfection as it is probable it ever will attain, that but little can be expected to occur, in so short a period as that embraced by this history, which can be recorded as instances of its absolute advancement. A memoir, however, on the use of the forceps, published in a late Number of the Medical and Physical Journal,* has been considered by several able and experienced obstetricians, as an addition of considerable value to the existing instructions for the practice of this branch of the medical art, chiefly from its presenting precepts for their application, and the mode of acting with them, much more precise and determinate than any which had been previously advanced. These circumstances are pointed out by Dr. GRANVILLE, in his comments on that memoir;† whilst he has, himself, discussed and illustrated several points respecting the application of the instrument, and added some important remarks on the shape of it most proper in particular cases.

The Synopsis of Dr. MERRIMAN, already noticed in this Sketch, is said, by a very able and experienced obstetrical physician,—who has himself recently produced a practical work on several subjects in midwifery, that is much praised by competent judges,‡—to contain "much valuable information," and to be a book "which must benefit every reader by its perusal."

Although it assumes no claims to originality, a little work by Dr. CONQUEST§ seems to require notice in this place, from its being regarded as a production calculated to be of considerable utility, by presenting, in a concise and well-arranged form, a code of the best principles and precepts for the practice of midwifery. Its especial pretension is to serve as a book of reference to young practitioners,

* See *London Medical and Physical Journal*, No. 259.

† *Op. cit.* No. 260.

‡ The work here alluded to has been published since the commencement of the present year; and will not, therefore, be noticed in this Sketch in a particular manner.

§ *Outlines of Midwifery, developed in Principles and Practice.* 12mo. pp. 193. Anderson, London, 1820.

"under circumstances of doubt or difficulty, either in the study or lying-in room:" and it is said, by able obstetricians, to be very well qualified for such a purpose.

The explanation given by Dr. DOUGLAS of what is termed the "spontaneous evolution" of the fetus,* in some cases of arm-presentation, has been found to be correct in one instance, (the only opportunity he seems to have of witnessing the process,) by Dr. GOOCH;† not in a case where the child was small or the pelvis very large,—under which conditions alone the process as described by Dr. Douglas has been supposed, by some obstetricians, to take place,—but where "the pelvis was not unusually spacious; the patient was at her full time; it was a first labour, and the child was large." Dr. Gooch was satisfied that the presenting arm did not retreat, but continued to advance still forwarder under the arch of the pubis, as the nates took the sweep along the sacrum that terminated in their expulsion.

On the subjects of Medical Jurisprudence and Medical Police, there is nothing particular to be adduced: it is only necessary to refer to the works on yellow-fever, plague, and typhous fever, noticed in former parts of this Sketch, as sources of important inferences relative to the latter.

In Chemistry, as it is immediately applicable to physiology, pharmacy, and the detection of poisonous substances in reference to juridical investigations, there are but few novelties.

The most interesting account of recent researches in animal chemistry, is that of Mr. PROUST, on the composition of urinary calculi,‡ which will be given in detail in the ensuing volume of the Medical and Physical Journal.

Mr. VOGEL, of Munich, has published an account of a biliary calculus, passed from the intestines by a man who had been in bad health for several years; that was chiefly composed of *cholesterine*, (92·0 cholesterine, and 8·0 of yellow powder of bile,) which has itself much analogy with ambergis; the latter substance containing 85·0 of what chemists call the ambric principle, which is capable of being converted into cholestric acid by the addition of nitric acid. These circumstances much favour the notion of the animal origin of ambergis. The concretion above alluded to weighed 180 grains: it was of the size of a nut, of a soft consistence, and fatty to the touch. There was no perceptible nucleus in it: the interior was composed of crystalline layers, of a yellowish colour. Its specific gravity was 0·912. It melted, in a strong heat, into a transparent matter, resembling melted wax, without, however, in this state, leaving a greasy spot on paper.

* See *London Medical and Physical Journal*, July 1819.

† *Medical Transactions*, vol. vi.

‡ *Annales de Chimie*, Juillet 1820.

It burnt, by the light of a candle, with a vivid flame, without developing a sensible animal odour. It was very nearly spherical in shape, being only compressed a little on the sides.

A very similar case to the foregoing has since been related by Sir EVERARD HOME.* The gall-stone here, however, weighed 239 grains: "it is," says Mr. Brande, "almost entirely composed of the spermaceti-like substance which Mr. Thenard has called *cholesterine*; it is soluble in hot alcohol, and deposits crystalline plates as the solution cools, leaving a very small portion of brown insoluble matter."

In vegetable chemistry, the most interesting original observations seem to be those of Mr. TODD THOMSON, on the means of ascertaining the goodness of dried vegetable substances which contain gluten,† which will be noticed somewhat particularly under the head of Pharmacy, (to which they more especially apply,) in order to avoid repetitions.

Now that *iodine* has been introduced into the *materia medica*, it becomes a matter of interest to ascertain the substances in which it naturally exists.

Mr. FIFE, of Edinburgh, detected it in sponge towards the end of the year 1819; and Mr. CHEVREUL has found it in the bones of the head of the crab and of the large lobster. Mr. STRAUB, of Hofwyl, as early as December 1819, appears to have shown its existence in the former substance, and proposed the use of it instead of the *spongia usta* in medicine. In order to obtain the iodine from sponge, the latter, after being burnt, was washed with water, and the solution decomposed by sulphuric acid; and in this way so much was obtained from half an ounce of sponge as to confirm the ideas previously entertained, that its medicinal properties were owing to this substance.

Mr. Straub also asserts the existence of iodine in turf. He was led to examine this substance in consequence of the peculiar odour he observed in the neighbourhood of those buildings where turf is burnt. Repeated experiments confirmed this conjecture; and, by acting on two pounds of turf, abundant evidence of the existence of iodine in it may be obtained. It was found also in the cinders of the *helminthocorton*, though in very small quantities.

The French chemists are proceeding to extend their analyses of vegetable substances to all those which are admitted into the *materia medica*.‡ M. M. LASSAIGNE and FENUELLE have examined the *asarum europæum*,§ and found it to consist of a volatile concrete oil; a very acrid fat oil; a yellow matter, analogous to cytisine, in which the medicinal properties of asarum seem especially to reside; some fecula, mucous matter, alumine, citric acid, citrate and malate of lime, an acetate, a salt with an ammoniacal base, and some mineral salts.

* *Journal of Science*, &c. No. xix.

† See *London Medical and Physical Journal*, October 1820.

‡ See *London Medical and Physical Journal*, August 1820, for analyses of belladonna and hyoscyamus.

§ *Journal de Pharmacie*, Decembre 1820.

Mr. CHEVALLIER (of Paris,) has found the virginian serpentary root to consist of a volatile oil ; starch ; resinous matter ; gummy matter ; albumen ; a yellow, bitter, matter, causing irritation in the throat, soluble in water and alcohol ; malic and phosphoric acids, combined with potass ; a small proportion of malate of lime ; phosphate of lime ; iron ; silica.*

In mineral chemistry, as far as it is concerned in jurisprudential inquiries, the only observations to be here noticed are those of ORFILA, respecting the means of detecting various poisonous minerals, when combined with different coloured fluids, commonly used as drinks ;† and a new and delicate test for copper, in solution, in the tincture of guaiacum.‡

The deep colour of some liquids, as red wine and coffee especially, renders the detection of several of the poisonous minerals in them, by the ordinary tests, extremely difficult, if not impossible : the colour of the precipitates, &c. being obscured by that of the liquids alluded to, which is imparted to the former. This difficulty in the way of jurisprudential inquiries, as far as red wine and coffee are concerned, has been obviated by a new mode of proceeding lately suggested by ORFILA. It consists in first destroying the colour of those liquids by some substance which will not, at the same time, decompose the poisonous mineral : such a substance is found in *chlorine*, dissolved in water, (oxymuriatic acid) which decomposes but a very few of the mineral poisons, and is applicable, for the purpose in question, to the process for detecting white oxide of arsenic, arsenic acid, arsenite of potass, corrosive sublimate, acetate and sulphate of copper, the salts of lead, nitrate of bismuth, sulphate of zinc, hydrochlorate (muriate) of gold, hydrochlorate of barytes, composition blue (a mixture of sulphuric acid and indigo), and alum.

The mode of proceeding is as follows : a sufficient quantity of a concentrated solution of chlorine in water (strong oxymuriatic acid,) is poured into the liquid supposed to contain some one of the poisonous minerals above enumerated, until its colour is destroyed, or, rather, until it is reduced to a yellowish colour ; a precipitate is formed (composed of chlorine and the colouring matter), which is to be permitted to settle ; the liquid is then to be filtered, and the ordinary tests applied to it, when the results do not differ from those obtained when the poisonous mineral has been simply dissolved in water.¶

* The writer supposes that it is his duty to detail these matters ; for, though he cannot discern what good there is to be derived from them, he imagines, perhaps, that they must be fertile in interesting and useful inferences, or men would not consume their lives in such researches, and think that there is nothing else in nature worthy of the attention of human beings.

† *Nouveau Journal de Médecine*, Juillet 1820.

‡ See *London Medical and Physical Journal*, July 1820.

§ It is necessary that this liquid be in a concentrated state.

¶ Should no precipitates be produced on the application of the tests, the liquid should be evaporated to one-third, or a still less proportion of its volume ; as when the poisonous mineral is much diluted, or when the oxymuriatic acid has not been in a concentrated state, the tests will fail to detect the presence of the former, unless the fluid be evaporated as above directed.

The only new addition to the *Materia Medica* is in the common potato-plant (*solanum tuberosum*), proposed as a narcotic by Dr. LATHAM,* "as superior to hyosciamus and conium." The *lytta vitata*,† and the *lepidium ruderale*, although not novel as medicines, should be noticed, from their having lately been particularly recommended to our attention. The former is said, by Dr. DANA, of the United States,‡ to possess vesicating powers in a higher degree than the *Meloe vesicatorius*, or common Spanish fly.

The use of the *lepidium ruderale* has been advocated by Dr. RUHL, first physician to the Empress Dowager of Russia, as a remedy for intermittent fevers. He states that Dr. RITMEISTER (physician to the Hospital of Powlowsk,) was, in 1812, affected with tertian ague, for which cinchona had been taken without success. Some peasants, who knew him, brought him a plant, which they called, *herb against fever*. Dr. R. recognized in it the *lepidium ruderale*, took an infusion of it, and was cured. After this he employed it in numerous cases in the Hospital, and with the most complete success. He communicated this knowledge to Drs. TRINIUS and RUHL, who have given it in a great number of cases of intermittent fever, of all types, but especially in those occurring in the spring, with results highly favourable to the character of the remedy. It was administered in the form of an infusion, made with an ounce of the herb to a pint of boiling water, of which two table spoonfuls were taken every two hours in the intervals of the paroxysms.

The application of vegetable chemistry to the purposes of PHARMACY, by Mr. TODD THOMSON, which has been already alluded to, presents an original view of this object that will, probably, hereafter become the source of many important improvements in that art. "Whilst lately repeating the experiments of Mr. Taddei on gluten," says Mr. Thomson, "I was astonished to find how readily that principle is destroyed, in many plants which contain it, by the action of heat; and, reflecting on the changes which necessarily take place in all the components of a vegetable body when one of them is destroyed, I was naturally led to infer, that those specimens of any medicinal plant, in which the components can be preserved in a state the nearest to that which they hold before it is dried, must be the most efficacious and certain in their remedial properties. I had ascertained the correctness of M. Planche's assertion, that gluten is one of the constituents of the recent bulb of meadow saffron; and discovered that its presence can easily be detected in the dried bulb; and, as I had found that those specimens of it in which this principle can be demonstrated are the most powerful in their effects, I inferred that these only should be regarded as properly dried, and fit to be medicinally employed, in which it is found, provided they bear marks of having been dug up at a proper season; and, therefore, that the agent by which gluten is detected in the dried bulb of colchicum, must be regarded as a proper test of its goodness." The propriety of these opinions has been proved,

* *Medical Transactions*, vol. vi.

† See *London Medical and Physical Journal*, December 1820.

‡ In Dr. SILLIMAN'S *Journal of Science and the Arts*, vol. ii.

by Mr. Thompson, by a series of experiments. As it may not be known to every reader, it seems proper to state, that a beautiful carulean blue colour is developed on the combination of the tincture of guaiac (which is the agent alluded to in the foregoing observations), with gluten. Mr. Thompson adds, "I ought to mention that, in all the experiments with the dried bulb, the powder was first rubbed with distilled vinegar; because I had ascertained that, in dry amylaceous powders containing gluten, this principle is in a state which prevents it from being readily acted on by the guaiac, unless it be previously dissolved; and, as acetic acid is the best solvent of gluten, distilled vinegar was employed for that purpose. Distilled vinegar, when added alone to the alcoholic solution of guaiac, merely precipitates the guaiac unaltered; consequently, the change of colour cannot in any degree be ascribed to that agent; and the same colour is evolved if the powder of colchicum be well rubbed with the solution alone, although it is by no means so quickly produced."

From his observations on the physiology of bulbs in general, Mr. Thompson has been enabled to determine the exact period of the year at which the colchicum root should be dug up, which is towards the end of July. "The bulb of the last year is, then, (he says,) completely decayed, and its progeny, those of the present year, are at their full size and in their most perfect condition. When dug up, each bulb is found covered with a smooth, shining, thick, mahogany-brown coat; it is plump, solid, and in shape somewhat resembling a roundish pear; the withered remains of the foliage answering to the stem, with a sharp ridge partially surrounding the base, and terminating in a short appendix. On removing this coat, and a spongy one which is interior to it, a small white oval body, about a line in breadth, one-third of an inch in length, and slightly elevated, is seen seated in a depression near the point of the appendix, on one side; and another of the same description, a little higher up, in another slight depression, on the opposite side of the bulb. These are the rudiments of the expected flowers and leaves; and, when the sheath by which they are covered has attained an inch in length, the bulb has already begun to lose its medicinal qualities. If it be taken up in July, and sliced transversely, each slice has a nearly circular figure, slightly approximating to the oval, but perfectly free from any indentation on either side; and this form is retained in the dried slice: whereas, when the flowers appear, every slice is panduriform, or an oblong oval, with a deep semicircular notch or depression on each side; and every approach to this figure implies that the bulb has been dug up too late in the season. These indentations arise from the sheaths of the flowers, as they enlarge, pressing upon the sides of the bulb, which now readily yield, owing to the absorption of its contents for the support of the growing flowers first taking place in the part immediately under that on which the sheath presses. At that part the exhausting process is immediately recognized, by the spongy appearance which is seen surrounding each indentation, when the bulb is transversely divided."

The bulb, when taken up, should as soon as possible be cut into transverse slices, equal in thickness to half a crown; and these, being spread out upon white paper, should be dried without artificial heat, in an airy situation, screened from the sunshine.

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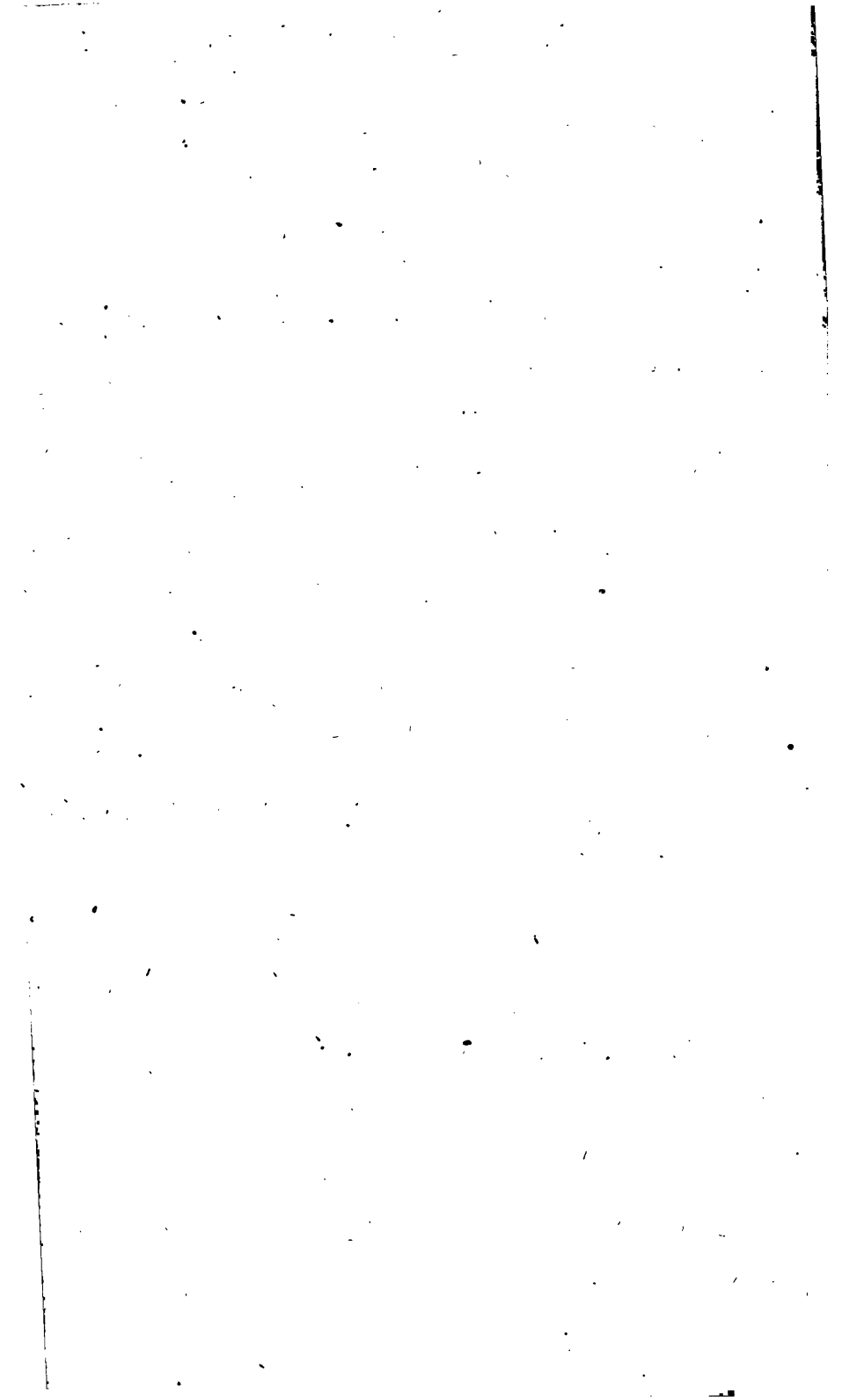
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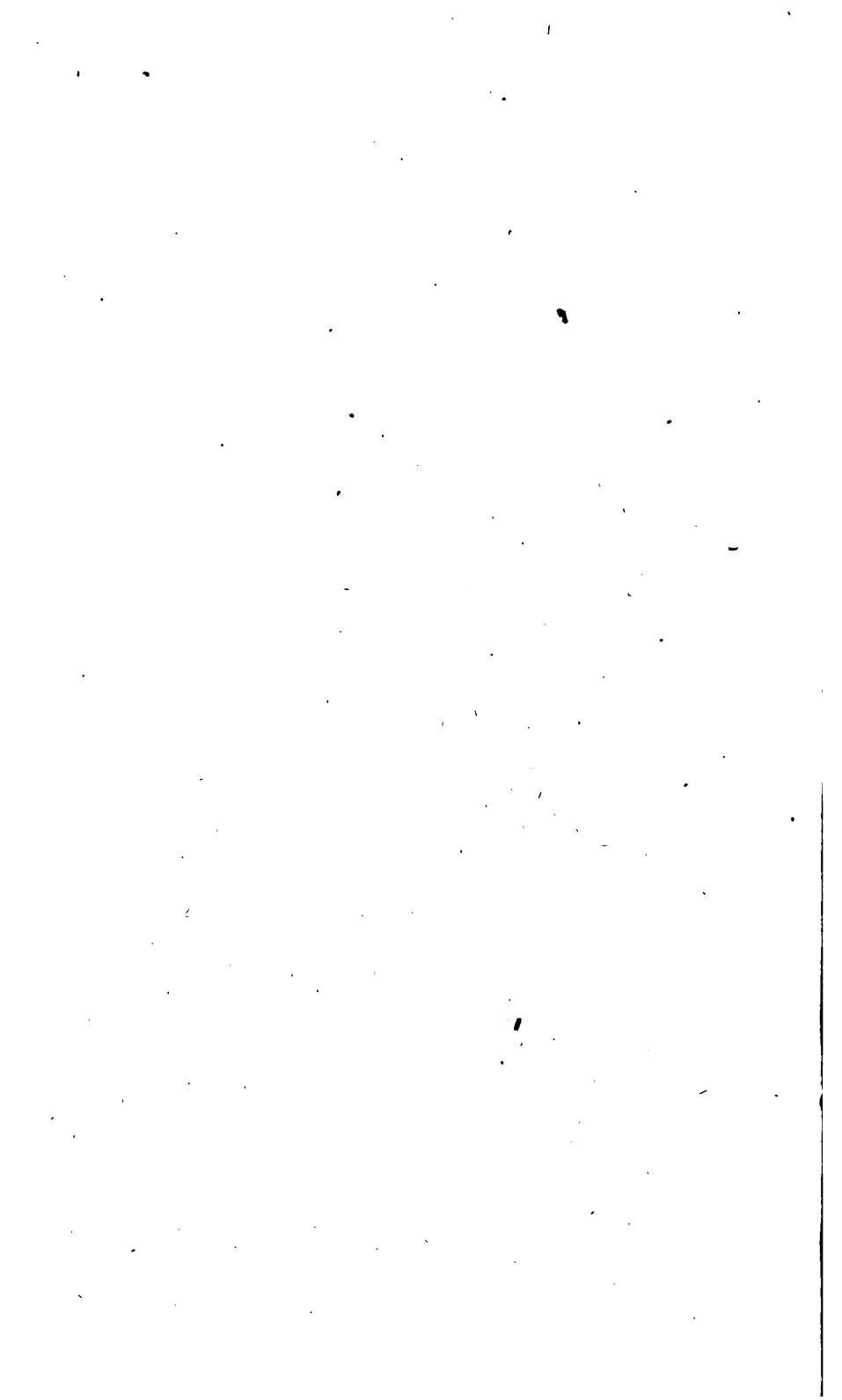
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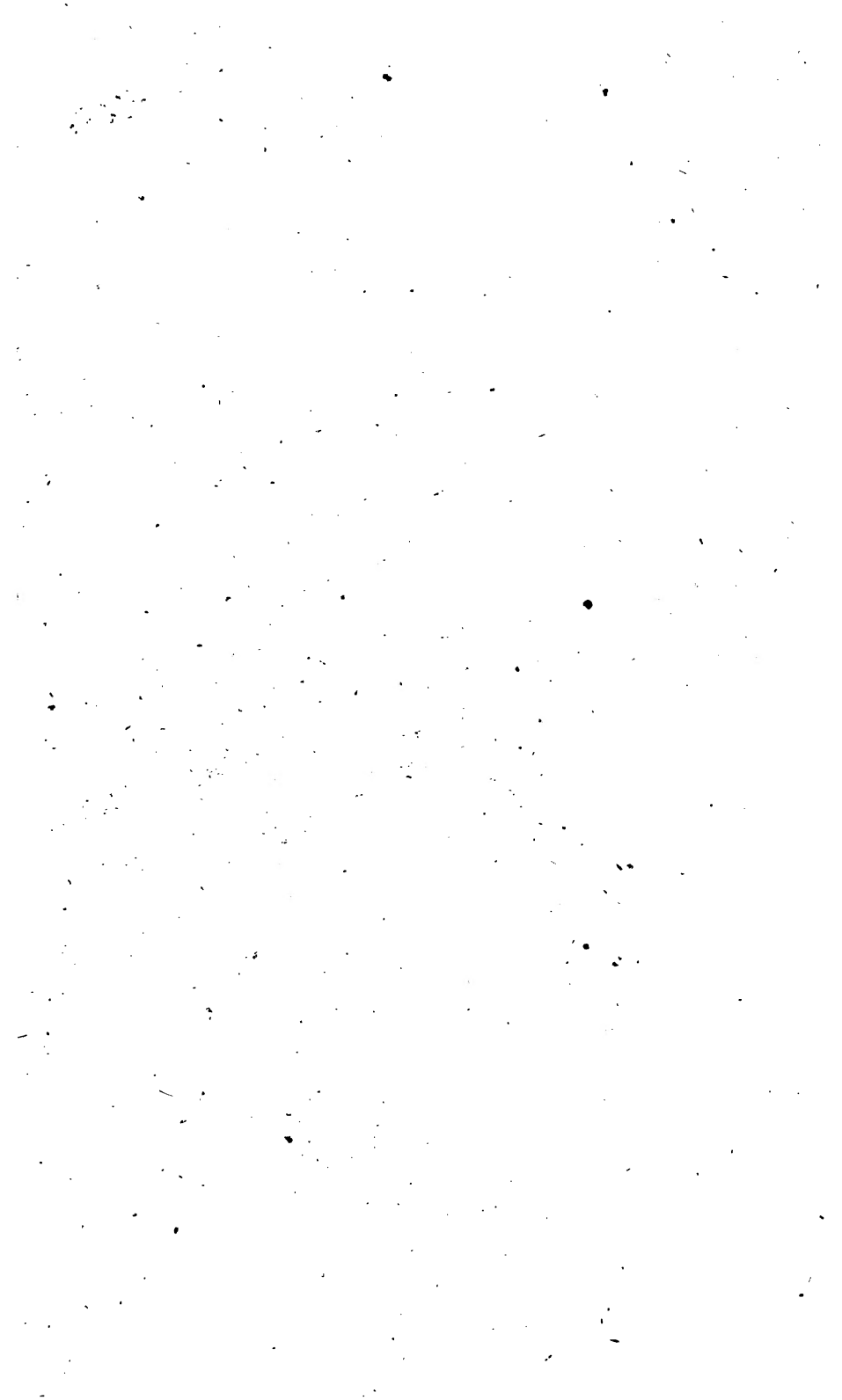
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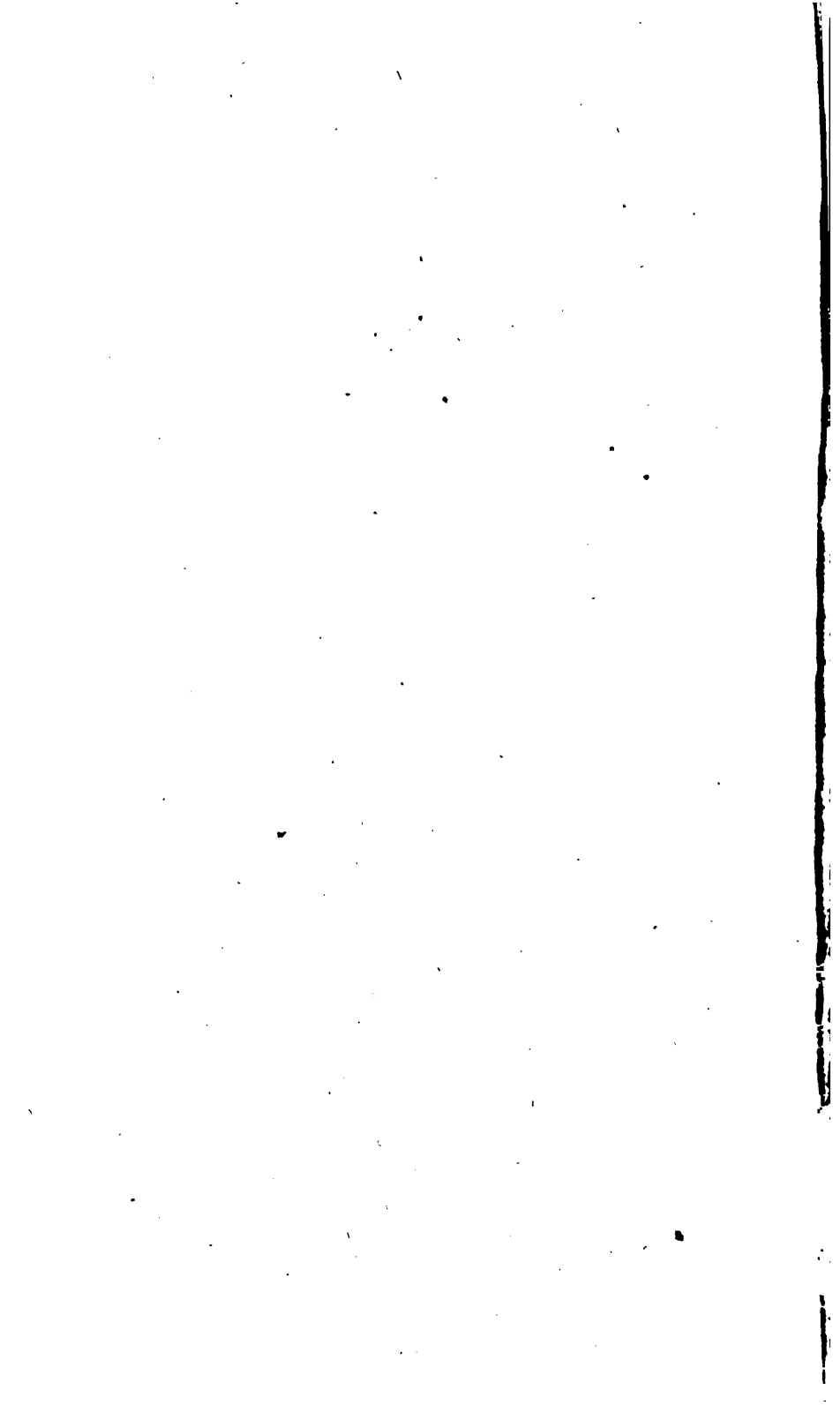
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